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Articles and Notes

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THE AMERICAN JOURNAL OF PSYCHOLOGY

EARLY PUBLICATION OF PAPERS

To the Contributors:

In spite of a rigorous selection of papers THE AMERICAN JOURNAL OF PSYCHOLOGY usually has in hand sufficient copy to fill a complete volume. Since publication follows the order in which manuscripts are accepted, papers may appear, under present conditions, nine months to a year after they are received from the author. This is unfortunate both for the contributor and for the Journal.

As a means of avoiding this long delay, the editors have frequently been urged to publish two volumes a year, or to establish a monographic series. While either plan would relieve the situation, the editors believe that a greater financial burden should not be placed upon the subscribers.

The editors have therefore tentatively adopted, as an alternative, a plan that has been successfully used for more than five years by the *American Journal of Botany*. In accordance with this plan, contributors may secure early publication of their articles by assuming the cost of their publication. This plan will, in effect, increase the size of the Journal at no additional expense to the subscribers, as these articles will be published *in addition* to the pages of each quarterly issue (156) to which the editors are committed.

The plan will not delay the appearance of other papers. On the contrary it may, in point of fact, insure their earlier publication. For example, an author, whose paper is scheduled to appear in the July number, may have it published, under this plan, in the preceding January. The place of his paper in the July number would then be filled by one previously designed for the October number.

The following items enter into the cost of publication: (1) composition; (2) plate making (cuts and half-tones); (3) correction of proof; (4) press-work; (5) paper; (6) binding; (7) postage and mailing costs; and (8) offprinting (paper, press-work and binding). Under the plan now proposed a contributor may, by assuming the costs of items (1), (2), (4) and (5)—the other items being absorbed by the Journal—have an article published in the first issue of the Journal made up after its acceptance.

It is thought that institutions may be willing at times to meet the expense involved in this arrangement in order to secure the early publication of papers for the members of their faculties or staffs, and that individuals will appreciate the opportunity thus offered of having graduate theses and other important papers brought out promptly.

Since every article published in the Journal carries its date of acceptance, the question of priority is clearly established.

Papers submitted to the editors for publication under this plan will receive the same critical scrutiny as all others.

Estimates of the cost for publication under the plan outlined here may be obtained from any one of the editors.

January 1, 1930

M. F. WASHBURN
MADISON BENTLEY
K. M. DALLENBACH
E. G. BORING

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THE EFFECT OF PRACTICE UPON THE UPPER LIMEN FOR TONAL DISCRIMINATION

By JOHN F. HUMES, University of Pennsylvania

It has been shown by Cameron that practice is one of the factors involved in the improved discrimination of vocalized tones.[1] Gough has also demonstrated that "practice has a marked effect on an individual's ability to identify musical notes that is distinct from the initial capacity of the individual or from the type of instrument used in producing the notes."[2] These results suggest that practice might increase the sensitivity of hearing to such an extent as to affect the upper tonal limen.

The purpose of this study was, therefore, to discover whether practice affected the upper terminal limen for tone, and, if so, how.[3]

METHOD AND PROCEDURE

A sound-proof room was not desired as silence and freedom from distraction produce in the *Os* a critical attitude toward the stimulus,[4] and as minor distractions have been found to increase accuracy of judgment.[5]

*Accepted for publication April 20, 1929.

[1]E. H. Cameron, Effects of practice in the discrimination and singing of tones, *Psychol. Monog.*, 23, 1917, (no. 100), 179.

[2]Evelyn Gough, The effects of practice on judgments of absolute pitch, *Arch. Psychol.*, 47, 1922, 86.

[3]A series of preliminary observations was made in November and December, 1926, the object of which was to choose suitable *Os* as well as to find a room which would meet the requirements of the experiment. The only room available at that time was an unoccupied office on the third floor of Bennett Hall. In March 1927, a room which was more adapted to the needs of the problem was procured in the Medical Laboratory. Here the investigation progressed until June when it was interrupted by the close of the term. This room was not available in the fall, consequently the experiment was not resumed until December, 1927, at which time a room was obtained, through the courtesy of Dr. R. Tait McKensie, in the Hutchinson Gymnasium of the University. These changes produced no apparent or measurable effect in the results.

[4]D. A. Anderson, Duration of tones, the time interval, the direction of sound, darkness and quiet, and the order of stimuli in pitch discrimination, *Psychol. Monog.*, 16, 1914, (no. 69), 155.

[5]F. O. Smith, The effects of training in pitch discrimination, *Psychol. Monog.*, 16, 1914, (no. 69), 92.

Apparatus. A set of 40 steel cylinders of the Koenig type,[6] ranging from 16,200 to 24,000 d.v. by increments of 200 d.v., were employed.[7] The apparatus used in actuating the cylinders is fully described and pictured by Fernberger,[8] hence its description is omitted here.

The apparatus, it must be said however, eliminated two disturbing sources of error—variations in intensity[9] and in localisation.[10] The cylinders were actuated by a falling steel ball.[11] Six balls of uniform weight were used in chance order during the course of an experimental hour. Table I gives the weights, distance of fall, and the resultant energy of the balls at the moment of impact.[12] The accuracy of the measurement of the weights is ±0.0002 grm., and of the distance of fall, ±0.10 cm. The energy of the ball at the moment of impact is the product of the weight of the ball by the distance through which it falls. For the purpose of this experiment we may neglect the slight variation in the third column of Table I and consider the factor of intensity as practically constant.[13]

TABLE I

SHOWING THE WEIGHT OF THE DIFFERENT BALLS, THE DISTANCE OF FALL, AND THE ENERGY OF IMPACT

(Distance of fall 20 cm.)

Ball No.	Weight (grm.)	Energy (grm. x cm.)
1	8.205	164.10
2	8.208	164.16
3	8.213	164.26
4	8.222	164.44
5	8.223	164.46
6	8.224	164.48

Observers. The *Os*, all members of the staff of the Department of Psychology, were Carl Altmaier, Jr. (*A*), F. R. Gillespie (*G*), Francis Irwin (*I*), Dr. Miles Murphy (*M*), Sidney Sanderson (*Sa*), and E. M. Twitmyer (*T*).[14] Their ages ranged between 24 and 35 years. While none of the *Os* was, in the strictest sense, highly trained in psychophysical observation, *Sa* and *M* were more

[6]Manufactured by the Standard Scientific Co., of New York.

[7]Cf. E. G. Boring, The construction and calibration of Koenig cylinders, this JOURNAL, 38, 1927, 125-127.

[8]S. W. Fernberger, An improved apparatus for actuating tonal cylinders of the Koenig type, *ibid.*, 38, 1927, 120-124.

[9]Cf. E. F. Möller, The effect of change of intensity upon the upper limit of hearing, *ibid.*, 33, 1922, 570-577.

[10]Fernberger, *op. cit.*, 120.

[11]*Ibid.*, 123.

[12]Cf. E. W. Scripture and H. W. Smith, Experiments on the highest audible tone, *Yale Studies*, 2, 1894, 105.

[13]C. C. Pratt (Highest audible tones from steel cylinders, this JOURNAL, 31, 1920, 406) found that slight variations in intensity did not significantly alter the relative frequencies of judgments.

[14]I am grateful to the *Os* for their coöperation which extended over a protracted period. I wish also to acknowledge my indebtedness to Professor Fernberger, who suggested the problem and assisted me throughout its course; to Mr. M. R. Wehr, who made the weighings of the steel balls in the Physics Laboratory; and to Mr. John R. Roberts, who supervised the care of the apparatus.

experienced than the others, all of whom had had but slight acquaintance with this type of work. *Sa* and *M*, some years before this experiment was begun, had made several observations on the same material used in the present study; but it was assumed, and subsequent observation bore out the assumption, that the previous experience would exert no influence on this experiment. No indication as to the purpose of the study was made to any of the *Os*. *Sa* and *A* observed during the preliminary series only, for reasons later to be described.

Procedure. In some respects the method follows that used by Möller.[15] *O* was seated with his back toward the apparatus, at a distance of 4 ft. from the center of the cylinder being actuated. The relation of the position of *O* to the arrangement of the apparatus was such that the actuated cylinder was always in the median plane of *O*'s head. The following instructions were read at the beginning of each period of observation: "You will experience a number of auditory sensations. In each case you will immediately report whether the experience was tone or not tone; if tone say, 'Yes;' if not tone say, 'No'."

No further instructions were given. A preparatory signal, 'Ready, now,' preceded the actuation of each cylinder by about 2 sec. Observations were recorded immediately. Since it was desired to study the effects of practice, each response was considered important and consequently recorded. Frequently an *O* would request that a stimulus be repeated, invariably because of doubt as to judgment. Repetition was permitted as often as requested.

The *Os* varied considerably in the length of time required to determine a critical stimulus. When this was once determined, it was made the central stimulus of a series of seven, three of which were the next in order above, and the other three the next in order below that point. The cylinders were then arranged on the apparatus so that the presentation of the stimulus would be in haphazard order. Fifty judgments were made on each cylinder, 350 in all, at every session, for the purpose of calculating a statistical limen. In several instances, however, the number of judgments was greater or less than that just specified, due to difficulty in determining a critical stimulus. Rest periods of from 1-3 min. were permitted after every 100 judgments. Every observational session consumed about an hour.

The psychometric function that determines the upper tonal limen was calculated from the observed relative frequencies of affirmative judgments in accordance with the phi-gamma hypothesis. Urban's Tables[16] were used to simplify the calculations, which are recapitulated in Table II. This table shows the numerical value of the statistical thresholds. The limens (L) and the indices of precision (h) were computed by linear interpolation between successive values.[17]

At the completion of each session the *Os* were asked to write an introspective report, including a statement of the criteria upon which their judgments were based.

[15] Möller, *op. cit.*, 572.

[16] F. M. Urban, Hilfstabellen für die Konstanzmethode, *Arch. f. d. ges. Psychol.*, 24, 1912, 236-243.

[17] Cf. S. W. Fernberger, A preliminary study of the range of visual apprehension, this Journal, 32, 1921, 123. Also E. G. Boring, Urban's tables and the method of constant stimuli, *ibid.*, 28, 1917, 282.

Results

Statistical limens (L) of tonal quality, measured in terms of frequency of vibration, and coefficients of precision (h) are presented in Table II for every *O*, for every series of experiments.

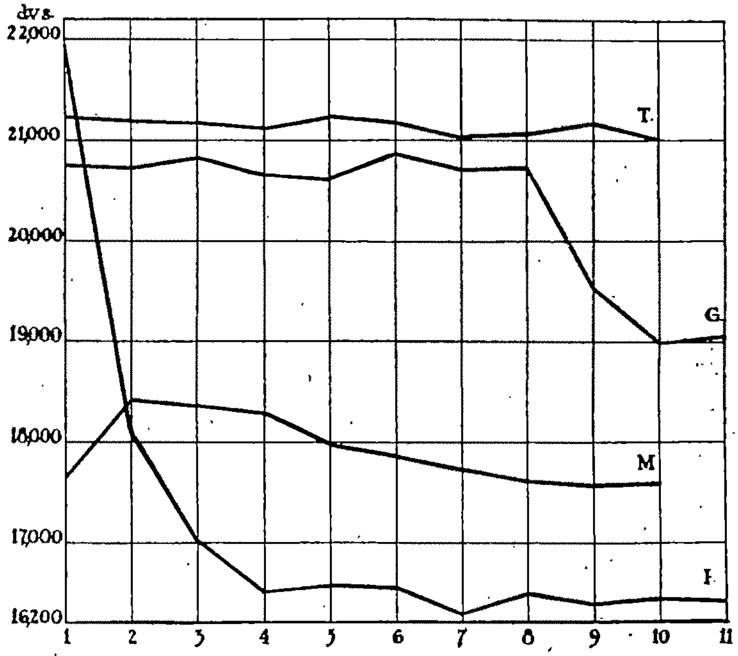

FIG. 1. SHOWING THE EFFECT OF PRACTICE UPON THE UPPER LIMEN OF TONAL DISCRIMINATION

Eleven calculations were made for *G* and *I*, and 10 were made for *T* and *M*. It should be borne in mind that these values, because the vibration rates of the tonal cylinders were calculated, have relative rather than absolute validity.[18]

It was expected that practice would increase the magnitude of the upper terminal limen. An inspection of Table II reveals, however, no such result. On the contrary, there is, in the case of every *O*, a noticeable decrease in this value. The higher frequencies, at first experienced as 'tone,' are no longer experienced as such.

[18]Boring, *op. cit.*, 125.

TABLE II

SHOWING THE TONAL LIMEN (L) AND MEASURE OF PRECISION (h) FOR EVERY *O* FOR EVERY SERIES OF EXPERIMENTS

Series	*G*		*I*		*T*		*M*	
	(L)	(h)	(L)	(h)	(L)	(h)	(L)	(h)
I	20750	0.0023	21942	0.0012	21202	0.0019	17641	0.002
II	20741	0.0037	18178	0.0013	21177	0.0022	18402	0.0011
III	20802	0.0026	17056	0.0021	21174	0.003	18359	0.0013
IV	20680	0.0017	16530	0.0013	21124	0.002	18275	0.0018
V	20623	0.0026	16584	0.0016	21251	0.0026	17912	0.0017
VI	20855	0.0022	16569	0.0016	21171	0.0023	17863	0.0024
VII	20706	0.0034	16267	0.0012	21055	0.0021	17754	0.0024
VIII	20745	0.0034	16496	0.0016	21061	0.0023	17668	0.0025
IX	19527	0.0022	16393	0.0018	21167	0.002	17646	0.0016
X	18922	0.0029	16432	0.0011	21019	0.002	17659	0.0024
XI	19056	0.0028	16420	0.0012				

The limens. An examination of Fig. 1, which pictures diagrammatically the results shown in Table II, reveals marked individual differences in the upper limen. There are four well-defined levels, and but one overlapping.

I's limen in Series I is well above that of any of the other *O*s. In Series II, however, his limen drops to a point far below that of *T* or *G* and slightly below that of *M*; and in Series III it drops still further to a point where it is much lower than that of any of the other *O*s. In Series IV, *I*'s limen reaches a point of relative stability. Thus *I* starts out with the highest terminal limen and ends with the lowest. It should be noted here that 9 weeks elapsed between *I*'s first and second series. During that time he observed at 6 different experimental sessions, the results of none of which were sufficiently homogeneous to admit of calculation. Series II was the last one calculated before the close of the regular school year. Between Series II and III there was another interruption of about 9 months. During the latter part of this period (*i.e.* from the first of December, 1927—when work was resumed—to February 23, 1928) *I* observed at 5 sessions the results of which were again so irregular that no limens could be calculated. Series III was obtained during the latter part of February, 1928, Series IV 5 days later; and from then on the different series were obtained at weekly intervals.

Two interruptions occurred in *G*'s work: one between June and December of 1927; and the other between the latter part of December and the middle of the following February when the holidays and mid-term examinations interrupted the work. These interruptions are not reflected in his results. The drop in his limen in Series IX, which was the first value calculated after work was resumed in February, was due, not to the interruption, but to inspissated cerumen of his left ear. This troubled him for over a month. Series X, obtained one week after Series IX, shows a further drop; but Series XI, obtained after an interval of another week, shows a slight rise. Observations obtained still a week later were so irregular that a limen could not be calculated.

Except in the case of *M*, the values as determined at the beginning of the study, are all higher than at the end. The first limen of *M* turns out to be his lowest. The highest value determined for him was in Series II. A steady, though gradual, descent in the curve from this point is apparent up to Series X, which is but slightly higher than the one previously calculated. Successive periods of observation occurred closer together, in point of time, for *M* than for any of the other *O*s, the ten series having been made within two months (April 18 to June 14, 1928). One set of observations, made between Series IV and V, could not be statistically treated.

The curve of *T*, aside from one inversion, which is not strikingly significant, is the most stable of all. While there is a fairly gradual decrease in the liminal values, there is no significant variation except in Series V. For *T*, continuity of observational sessions was interrupted three times. The first limen was determined about the middle of April, 1927; the second, just previous to the December holiday. A lapse of 2 mo. occurred between Series III and IV; and

4 weeks intervened between Series IX and X. These interruptions were due to vacation periods and to *T*'s inability to serve in the experiments. The results plainly indicate that the intermissions were inconsequential. The observations of *T* were uniform throughout, so that in no instance was it necessary to discard any of his results.

Coefficients of precision. Tables of observed relative frequencies are omitted, because of their bulk, but an idea of the comparative steepness of the curves may be gained by considering the coefficients of precision (h). These have been averaged and the mean variation determined for every *O*. The values are as follows: *G*, 0.0027 ±0.0005; *T*, 0.0022 ±0.0002; *M*, 0.0019 ±0.0004; and *I*, 0.0014 ±0.0003. It will be seen from these results that *G* has the highest average h and the greatest m.v.; that *T*'s average h ranks second and his m.v. fourth; that *M*'s average h and m.v. are both in third position; and that *I*'s average h ranks fourth and his m.v. second. On the basis of these results no generalization may be advanced.

The critical stimulus. The value of the critical stimulus varied in the different series for every *O* except *T*.

For *T* the cylinder of 21,200 d.v. was used in all the series. For *I*, however, the cylinder of 22,200 d.v. was used in Series I, of 18,400 d.v. in Series II, of 17,200 d.v. in Series III, and of 16,800 d.v. in Series IV-XI. The cylinder of 21,000 d.v. was used in the first eight series for *G*, but thereafter, because of the condition of the ear referred to above, values of 19,600, 19,000, and 19,200 d.v. were used respectively in Series IX, X, and XI. The values of the critical stimulus used for *M* varied as follows; in the first three series the cylinder of 18,4000 d.v. was used, in the next four series (Series IV-VII) the cylinder of 18,000 d.v. was used, and in the last three series (Series VIII-X) the cylinder of 17,800 d.v. was used.

Numerous attempts were made over a period of 4 mo. to determine a critical stimulus for *A* and *Sa* but without success. These *Os* were tested over the entire range of the apparatus and their reports were so irregular that a limen could not be calculated from them. It is interesting to note, however, that the majority of their reports were positive even in the upper regions of the scale. *Sa*, for example, on an occasion when the cylinders varying from 22,800 to 24,000 d.v. were used, reported "this whole series seemed to be tone, and at no time was I in doubt of my judgment." Of the 70 judgments returned with this series, 63 were 'yes.' There was, however, *Sa* added, "little discriminable difference between the tones."

It was assumed that neither of these *Os* was able to distinguish in this range between tonal and non-tonal qualities and further work with them was therefore abandoned. It was later learned that they both were 'tone deaf,' in the sense that neither could 'carry a tune.'

Discussion of Results

The decrease in the upper terminal limen is due probably to a change in a number of factors, but prominent among them is the criteria employed by the *O*s in their judgments of tone.

The introspections reveal that the subjective standard of judgment changed as the experiment progressed. Thus while no improvement in sensory acuity was noted, the *O*s did develop a higher degree of discrimination in their judgments of a tone. Every *O* became more conservative and critical in his judgments as the experiments progressed.

As Möller has pointed out,[19] a statement of the limen only gives a little of the available information. Adequate interpretation requires an analytic treatment of the results. For the purpose of this study it has been deemed unnecessary to deal with every individual limen, but it is important to examine the introspections since they shed light upon the general aspects of the problem.

(*1*) *Criteria for tone.* Since no attempt was made, in giving the *O*s their instructions, to dictate the criteria of tone, equivocality is perhaps to be expected. We found that the criteria of judgment varied in the early stages of the experiment, not only from series to series, but even during the course of a single period of observation. *M*, for example, reported in Series II:

"The formation and maintenance of a criterion seemed more difficult this session. The experience was very similar to one I have had in grading examination papers, in which I have at times wondered whether my standard has not changed."

There is, however, more or less general agreement among the *O*s as to the characteristic qualities of the stimuli. A sound of a 'ringing' nature is judged as 'tone,' while a 'click' or a 'thud' is described as 'no tone.'

I, for example, said, "I distinguish a tone by its ringing quality. Whenever I hear a twang or a sharpening, I judge it a tone; when I hear just the click of the ball hitting the cylinder, I judge it not a tone." This is of particular interest since it was written after Series I, which is the highest limen calculated in the entire experiment, Thereafter it required stimuli of considerably fewer vibrations to be characterized as 'tone' on the basis of the 'ringing.' This result can be explained, we believe, in no other way than by a shift of attitude.

T, according to his reports and the observed relative frequencies of judgments, maintained a constant attitude throughout the experiment. He writes:

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[19] Möller, *op. cit.*, 576.

"I seemed to recognize two series of auditory stimuli. One had the content of a thud—this I called 'no tone.' The second type of stimulus carried with it a sort of 'ring'—this I called 'tone'."

G similarly characterizes 'tone' by "a ring which lasts but a very short time after the blow. If the ball strikes the cylinder with a thud, it is merely a noise. In many cases there is just the slightest suspicion of the ring, yet not enough to be classed as a tone."

M uses the same terms. He says, " 'Tone' is something added to the experience. 'No tone' is a dull thud. 'Tone' is that plus something more. The 'something more' is characterized by duration and a kind of 'ring'."

A and *Sa* use similar criteria in differentiating the experiences. *A* says, "The difference between a tone and not-a-tone is determined largely by the presence or absence of a thud. Whenever I hear the dull thud I judge 'no tone.' The absence of the thud determines 'tone,' but there seems also, in the case of 'tone' to be a 'ring'." *Sa* reports that "a metallic clang with an after-ring seems to be my criterion for tone. 'No tone' is a 'flat' sensation of short duration, lacking the after-ring."

The 'ring' and the 'thud' remain the basic differentia during the entire experiment. But the specific location in the scale where these perceptions occur is by no means constant. This variability is undoubtedly a function of the stimulus attitude.

(*2*) *Intensity*. The objective variations in intensity were, as shown in Table I, negligible, yet the *O*s reported intensive differences in their experiences.

I writes, after Series X, "I particularly noticed a phenomenon which I have noticed before, that the intensity of the sound seem to vary considerably. By intensity I mean the series of experiences which vary from loud to soft which are expressed in music by *forte* and *piano*. The 'tones' seem in general to be louder than the 'no tones,' though I cannot make an accurate statement about this relation."

M (Series VII): "I detected very great differences in intensity, apparently between various stimuli which are judged tone. In some the tonal intensity seems very high, in others faint."

G (Series VII): "Occasionally one of the tone sensations would ring out with more intensity than any of the others before. On the other hand, some of the 'no tones' would come as a gentle rap, entirely unlike any other 'no tones'."

A: "I noticed today that there was a louder tone sensation than I had heard before."

Neither *T* nor *Sa* report intensity differences.

Since the reports were made at the close of an experimental period, it was impossible to identify the particular stimulus that produced a judgment of greater intensity. Occasionally, however, an *O* would voluntarily indicate after the reception of a stimulus, an intensity difference. A repetition of several stimuli (including the one judged more intense) never resulted in the per-

ception of a difference. We feel warranted, therefore, in ascribing the intensive differences reported to subjective factors.

(3) *Auditory after-effects.* After-experiences were reported by some of our *Os*, as shown by the following excerpts from the introspections.

T (Series I): "[One] type of stimulus carried with it a sort of ring, or what seemed to be a momentary continuation of the initial impact. I think of it as an after-effect as distinguished from the abruptness of the first type ('no-tone'). This I called tone, for it seemed to carry more than the initial impact."

M (Series I): "Tone seems longer in duration than 'not tone.' The 'not tone' is a click or a thud which is brief and quickly gone. Tone 're-echoes,' as it were. Whether these echoes which seem to add to the duration of the sensory experience are aspects of the sensation primarily, or after-images, I am not able to say." After Series X, he writes, "the presence of pitch seems to carry with it a gradual, uniform falling away of sound."

Sa: "The tonal sensation always lasted longer; it is a ringing sensation followed by an echo-like effect."

Obviously differentiation of tone and not tone depends, to a large extent, upon the relative duration of the perception. The writer inclines to the belief, however, that the 'after-experiences' reported by the *Os* are not after-images analogous to the after-images of vision, but that they are either Bishop's 'modified endings,'[20] or experiences derived from the prolonged vibration of the stimulating media, or memory after-images. The results are not at hand to enable us to decide which of these is the true explanation.

(4) *Assurance and doubt.* The *Os* were not instructed to report the degree of certainty with which judgments were made. Consequently, subjective assurance is not indicated in definitely quantitative terms. Nevertheless, it seems certain, from the references made by the *Os* to the "difficulty of the task," to the fact that they "were guessing at first," to the "feeling of uncertainty" regarding their judgments, and from the numerous requests for a repetition of the stimuli, that the difficulty experienced by the *Os* in establishing constant criteria was in a large measure due to uncertainty. This is in agreement with Boring's statement that "doubt is the most constant offender" in the control of attitude.[21]

[20] H. G. Bishop, An experimental investigation of the positive after-image in audition, this JOURNAL, 32, 1921, 325.

[21] E. G. Boring, The control of attitude in psychophysical experiments, *Psychol. Rev.*, 27, 1920, 445.

(5) *Expectation and comparison.* The *Os* made a few references in their protocols to expectation[22] and comparison, consequently the extent to which these factors influenced their judgments cannot be definitely established. It is probable, however, that both of these factors were frequently effective. The occasions are chiefly due, we believe, to the chance arrangement of the stimuli in the stimulus-series,[23] therefore, whenever *E* suspected that either was effective, the order of presenting the stimuli was changed.[24]

The tendency to compare a present experience with one immediately preceding it was reported by several *Os*, as the following excerpts illustrate.

G (Series X): "The alternation of 'tone' and 'no-tone' became so evident that I unconsciously pre-perceived what was coming next."

M (Series II): "The sequence seemed important this time. I believe, if a sensation follows one which is positively 'tone,' that it is less likely to be judged 'tone' than if preceded by a doubtful experience. In other words, the judgment is influenced by the preceding experience."

A: "I felt that my judgment was somewhat influenced by the previous one. If a clear tone was heard, then the next one would invariably be a dull thud, which might have been judged a tone had it been isolated in experience."

These reports accord with Fernberger's conclusions in his experiments with lifted-weights,[25] that a stimulus is as likely to be judged one way as another, depending upon its position in the series.

(6) *Affection.* Three of the *Os*, as the following excerpts show, refer definitely to affective accompaniments of the sensory experiences.

I (Series VIII): "I noticed an affective reaction to the auditory sensations. When a sensation is surely 'tone' it is very pleasant; in doubtful cases there is unpleasantness. The greater the difficulty in reaching a judgment, the greater in general the unpleasantness. A sensation which is surely 'not-tone' is also pleasant, though less intensely so than in the case of one that is surely tone."

Sa (in his first report): "Tone is marked by an unpleasant tingling in my ears."

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[22] *Ibid.*, 445.

[23] Cf. S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 132.

[24] The effect of expectation in pitch discrimination was noted by F. O. Smith (*op. cit.*, 99) who says, "with a strong expectation of hearing the second tone high or low, the organism is set to make the appropriate response and this has a marked influence upon his judgment."

[25] Fernberger, *op. cit.*, 149.

M (Series I): " 'Not-tone' is always more pleasant than 'tone.' The tones are all high and unpleasant, and at times they are almost painful."

The affective aspects of the experience depend, in the case of *I*, not upon the tonal quality, but upon the degree of assurance that attached to his judgments. In the cases of the other two *O*s, tone is either accompanied by unpleasant consequences, *i.e.* a tingling in the ears, or is itself unpleasant. These results corroborate those of Smith—high tones for some of our *O*s are intrinsically unpleasant.[26]

(7) *Attentional factors.* Four of the *O*s referred specifically to attention as a factor of importance in their judgments. Maximal attention was required in the experiments. Whenever attention began to wane or to waver the *O*s reported that their judgments became difficult and uncertain. They further reported that they resorted, in such instances, to various tricks, such as opening or closing their eyes, to sustain their attention. Whenever distractions occurred or *O*'s attention was poor the experiments were repeated or temporarily interrupted.[27]

(8) *Physiological conditions.* The *O*s reported varying states of health during the course of the experiments. With the exception of *G* none of them, however, suffered anything more than a 'cold in the head.' *G*, as has already been mentioned, suffered from an aural disorder of the left ear.

The disorder of *G*'s left ear was first noticed in Series IX when the critical stimulus (which had been constant at 21,000 d.v.) had to be changed to 19,600 d.v. Our original intention was, when we discovered this defect, to exclude the results and to hold up further observations until *G* had recovered. It was decided, however, to report them and to continue the experiments, because the results were considered to be of theoretical significance.

There are two possible explanations for the decrease in *G*'s sensitivity in Series IX, X, and XI. The impacted ear-wax may have lowered the intensity of the stimulus and this may have resulted in the lowering of the limen. This explanation stands, however, in opposition to the results of Möller's study in which she investigated the effect of changes of intensity upon the upper limit of hearing. She found that the upper tonal limen did not vary with intensive

[26] Smith, *op. cit.*, 87.

[27] It was necessary in a few isolated instances to suspend work because of adventitious sounds. The observations made during such periods were discarded.

differences.[28] The impacted ear-wax may, on the other hand, have resulted in the functional loss of *G*'s left ear and this may have resulted in the lowering of the limen. This explanation stands in line with the results of Knudsen[29] and Young.[30] Both of these investigators found that sensibility is greater when both ears function together than when either functions separately. That *G* was, during the last three series of experiments, observing monaurally, is borne out by one of his introspections. He said, after the experiments of Series X, "The condition of the left ear was worse than ever before. None of the stimuli were heard in that ear. After realizing this, attention was directed to the right ear."

It is not clear, however, why the monaural limen should be lower than the binaural. Even though it be granted that the intensity of the stimulus is less in monaural than in binaural hearing, intensity may not, in the light of Möller's results, be used to explain the difference.

I's health may have been an important factor in the early stages of the experiment. After several experimental periods when his results were particularly irregular, he reported feeling "not physically well." After Series III, when he was approaching a standard criterion which he later used throughout the experiment, he wrote, "Today I am feeling better than at most, perhaps all, of the other sessions."

M reported a 'cold' on a day when his observations were too erratic to be computed. *T* likewise reported a 'cold,' during Series VI, which he believed had affected his left ear; his results did not, however, bear that out as they were not noticeably affected.

While we cannot generalize, it appears from these results that the *O*s, in order to return the best possible reports, should be in a good state of physical health.

(*9*) *Other factors:* (*a*) *Kinaesthesis.* *I* was the only one of the regular *O*s to mention kinaesthesis. He described his experience of tone as being "more like a sharp thudding against the ear than an auditory sensation." Both *A* and *Sa* referred in the preliminary experiments to kinaesthetic experiences. *A* reported that "the tone was determined by a feeling of vibration . . . a greater fullness in my ears than in the case of 'no-tone'." *Sa* observed "an unpleasant tingling in my ears." And again that "at times there was a distinct kinaesthetic sensation inside the ear."

Just how much the awareness of muscular strain entered into the *O*'s judgment can not be decided here. The results offer no clue as to the effect of

[28] Möller, *op. cit.*, 577.
[29] V. O. Knudsen, The sensibility of the ear to small differences of intensity and frequency, *Phys. Rev.*, 19, 1923, 84-102.
[30] R. C. Young, Binaural *vs.* monaural sensibility of the human ear to small differences in frequency, this JOURNAL, 37, 1926, 328.

kinaesthesis, nor do the introspective reports provide us with adequate material for a conclusive statement. We are, however, of the opinion that kinaesthesis is of definite importance and that it should not be disregarded.

(b) *Imagery*. On the basis of the introspective reports, imagery is relatively unimportant in these experiments, yet it must have been involved in them. It must have served, for example, as the basis of the factors of expectation and comparison discussed above. While not clearly described as such by any of the *O*s, the comparison of a present auditory experience with a preceding one evidently took place in imaginal terms, *i.e.* the memory of a preceding tone persisted long enough to be compared with a present one. Furthermore, the auditory 'after-effects' described above, may have been, as we remarked, nothing more or less than memory after-images.

(c) *Localization*. Localization, as the term is used here, means the general region of *O's* body at which the sound 'registers.' *G*, for example, stated (Series I) that "consciousness of the sound seemed to be in back of the head." After the last series he made a rough diagram of his head, indicating the various regions where the different qualities of the auditory experience were localized. *A* localized the tones, similar to *G*, in his head, although the localization was probably made in kinaesthetic rather than visual terms since he referred to a "decidedly sharp vibration in my head."

(d) *Volume*. Volume was seldom referred to in the introspective reports.[11] *I* stated in two reports, however, that some tones seemed "fuller" than others. *G*, on one occasion, mentioned that one of the tones had greater volume than the others. *Sa* also noted a variation in volume. It is difficult to say, however, whether these references are to imaginal contents or to actual perceptions of differences in volume.

(e) *Synaesthesis*. *M* alone manifested synaesthetic phenomena. After Series I he made the following statement, "There are many visual synaesthetic experiences. Tone is a dark circle with rays of light streaming from it; not-tone does not possess these radiations."

Summary

A review of the foregoing introspections indicates that even in a situation where the *Aufgabe* is relatively simple, a great deal of complexity may exist. It is well to repeat that a mere statement of the qualitative limen is insufficient for a complete comprehension of the problem. A consideration of all the relevant material available is necessary for the adequate interpretation of responses to the stimuli here employed.

Individual differences are in general greater than the variations of the judgments of the individual *O*s. We find the various *O*s placing emphasis upon different attributes of the mental process that led to their judgments.

[11]Massiveness or extensity of tone is the meaning here implied.

G, for example, made nine references to 'attention,' the focalization of which was consciously made and extremely necessary for a clear-cut judgment. Localization and imagery seemed to be fused in the three references which he made to these factors. His physical condition was also of concern to him, particularly in the later series, when his left ear was impacted. He referred to this condition three times, and every time in connection with the difficulty experienced in discrimination of tonal quality. He made but one reference to kinaesthesis, volume, intensity, after-image, and comparison and expectation.

I, on the other hand, placed greatest importance upon the degree of subjective assurance which accompanied his judgments. He made nine specific references to this factor. In the early stages of the experiment his unstable physical condition was mentioned four times in the reports. It is believed that this fluctuation of *I*'s general health was largely responsible for the long period required to establish a critical stimulus.

It is of interest to note the change that took place during the experiment in his criteria of judgment since it is more marked in his case than in the other *O*s. After Series II (computed two months after Series I) he said, "Today I seemed to have changed my criterion of tone somewhat. There seemed to be three fairly distinct types of sounds; those clearly not tones, those clearly tones, and a sort of middle class which did not have the sharp twang of what I have been calling tones, but had a kind of bell-like ring. These I called tones." Nine months later, after Series III had been completed, he stated, "In judging today I made a conscious effort to stabilize my criterion of tone by deciding to answer 'Yes' to all cases where I thought the odds were even or better; to all others, I answered 'No.' I used this criterion consciously only in doubtful cases." The next series was calculated one week later, and the same criterion was reported. *I* maintained this standard of judgment for the rest of the experiment; and it will be noted that there is but slight variation in the psychometric function from this point on.

I referred twice to attention and once to affection as factors which aided in subjective assurance. Scattered through his reports are single allusions to volume, kinaesthesis, and intensity.

T's emphasis was upon the temporal differences between 'tone' and 'no-tone.' There was no departure from this criterion. One reference to a 'cold in the head' was all that broke the uniformity of his reports. The maintenance of so constant an attitude resulted in the most stable psychometric function of all the *O*s.

M referred to a number of factors contributing to his judgments, but no one of them was emphasized. After Series I he reported, " 'Tone' is differentiated from 'not-tone' by other modalities as much as by audition." From this it would seem that for him the total mental state was important at the time of a judgment. *M*'s criteria of tone varied more than *T*'s or *G*'s, and less than *I*'s. By deciding to assign 'doubtful cases' to the category of 'no-tone,' *M* made his criteria more rigid. He did this at Series IV. From then on there is a more constant decrease in the liminal value for him than for any of the other *O*s. *M* mentioned, in his introspective reports, attention, auditory after-effects, and synaesthesis each twice, and affection, imagery, comparison, physical condition, and intensity each once.

Conclusions

The following conclusions may be drawn from the results of this study.

(1) The effect of practice is to decrease the upper tonal limen.

(2) Within the limits of the psychometric function, the individual *O* becomes more discriminating of qualitative differences.

(3) Marked individual differences are apparent in the qualitative psychometric functions for these four *O*s. Differences are also evident in the rate and degree of decrease of liminal values.

(4) There is a tendency, once a criterion is rigidly established, for the threshold to remain approximately constant.

(5) Introspective data, which supplement the psychometric are necessary for a complete interpretation of the statistical limen.

(6) Practice does not consistently affect the coefficient of precision, although the trend is in general toward stabilization of that value.

(7) A radical change in the qualitative threshold is largely a function of change of attitude.

(8) For one *O*, an incipient pathology of the auditory mechanism results in a decided obtuseness of qualitative sensitivity.

(9) Qualitative judgment depends upon an integration of auditory attributes and complex mental processes, as well as upon structural constituents, *i.e.* perceptual and imaginal contents.

STUDIES IN AFFECTIVE PSYCHOLOGY[1]

By Paul Thomas Young, University of Illinois

VIII. The Scale of Values Method

The original aim of the present study was to work out a technique for testing affective reactions. We hoped to develop norms of like and dislike similar, perhaps, to the well-known word association norms of Kent and Rosanoff. Owing to difficulties with the method the original aim has not been attained.

The following paper describes the method used and some of the difficulties encountered.

(*A*) *Constancy of affective judgments to odors.*[2] *S* was presented with a scale of affective values and instructed to make judgments upon odors. Thirty-two chemically pure organic substances were selected as stimuli and in addition an empty bottle which had been cleansed and sterilized served as a control.[3]

The experiment was conducted in a black-walled dimly illuminated room. An elevator shaft, about 3 × 5 ft., connecting the room with the upper floor of the laboratory, served as a vent. A table was placed in the bottom of this shaft and two fans—one above and one below—kept a current of air moving slowly upward during the experiment. The arrangement practically freed the experimental room from stray odors, diffusing them through the upper laboratory. The bottles were placed on a table in the bottom of the shaft. When not experimenting the door of the shaft was closed thus keeping odors outside the experimental room.

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[1]Continuation of the series, this Journal, 38, 1927, 157-193; 40, 1928, 372-400.

[2]The experimental work upon which the present study is based was done in the spring of 1923 by Miss Minna Libman under the direction of the writer.

[3]Dr. C. S. Marvel of the chemistry department, University of Illinois, assisted in selecting odors which would be chemically constant for the period of the experiment. Chemical designations have been published by Bentley (M. Bentley, Qualitative resemblance among odors, *Psychol. Monog.*, 35, 1926, (no. 163), 146). In the present experiment Nos. 1 to 36 of Bentley's list have been used except the four marked with an asterisk (Nos. 19, 26, 33, 35). These four were discarded on the ground that cutaneous irritation was involved. An empty bottle used for purposes of control was substituted for one of these (No. 33).

The bottles were black and of uniform size. Each bottle and corresponding stopper was labeled with a number. The chance order used in the experiment was as follows: 1, 9, 17, 25, 33, 2, 10, 18, 34, 3, 11, 27, 36, 4, 12, 20, 28, 5, 13, 21, 6, 14, 7, 8, 16, 24, 32, 15, 23, 31, 22, 30, 29.

S was seated on a comfortable chair with his back towards the stimuli. He was given the following instruction:

"The experimenter will place beneath your nostrils a bottle of odorous substance. Take three deep breaths every time an odor is presented.

"Immediately and without reflection indicate how the experience felt. If your attitude was one of indifference, indicate by 0. If pleasure was experienced, indicate by +, and if displeasure was experienced, indicate by −. Study carefully the scale of values on the accompanying sheet so that you may give the *degree* of feeling by one of these numbers."

On a separate sheet the following scale of values was placed before *S*:

+5 very great pleasure
+4 great pleasure
+3 pleasure
+2 slight pleasure
+1 very slight pleasure
0 indifference
−1 very slight displeasure
−2 slight displeasure
−3 displeasure
−4 great displeasure
−5 very great displeasure

The above scale is symmetrical and balanced. Before deciding upon this particular scale of values we considered some of the lengths and types of scale used in other investigations.[4]

Seventeen students in the writer's class in experimental psychology served as *Ss*. Two other *Ss* were used for some of the odors so the number of judgments per odor varies from 17 to 19. The *Ss* were taken one at a time into the experimental room. After reading the instructions and studying the scale of values an odor was presented. Affective judgment was made and immediately recorded. There was no special warning signal since the approach of *E* served as adequate warning. The thirty-two odors and one empty bottle (No. 33) were presented in haphazard order with a pause of half a minute or more between successive presentations. During the actual smelling the *S*'s eyes were closed.

For purposes of illustration seven odors and the empty bottle have been selected. The following table shows the frequency with which each point on the scale was used by the group of *Ss*.

[4]We previously used three degrees of P and three of U plus indifference. In a recent investigation Kenneth used two degrees of P and two of U. Conklin used six degrees of P and six of U plus indifference. P. T. Young, Constancy of affective judgment to odors, *J. Exper. Psychol.*, 6, 1923, 182; J. H. Kenneth, A few odor preferences and their constancy, *J. Exper. Psychol.*, 11, 1928, 56; also *Psychol. Monog.*, 37, 1927, (no. 171); E. S. Conklin, The scale of values method for studies in genetic psychology, *Univ. of Oregon Pub.*, 2, 1923 (no. 1).

The scale-of-values method has also been used in connection with the study of attention. L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 511; K. M. Dallenbach, The measurement of attention, *ibid.*, 24, 1913, 468; The measurement of attention in the field of cutaneous sensation, *ibid.*, 27, 1917, 444 ff.

Odor No.	−5	−4	−3	−2	−1	0	+1	+2	+3	+4	+5
27	5	5	5	2	1						
2	2	2	7	4	3	1					
17		4	3	2	3	4	1				
4		1	2	2	4	2	2	2	3	1	
33				1	2	12	3				
24					3	1	5	6	4		
30					2	1	7	3	4	2	
23						1	5	5	4	2	2

The odors have been arranged in a series from the most U (unpleasant) to the most P (pleasant). Every one of the other odors can find a place somewhere in this series. Some are wholly on the U side, others wholly on the P side, others in between. The empty bottle (No. 33) presented in the context of odors is the most indifferent of the series—and yet there are three judgments of P and three of U for it.

The result illustrates a well-known fact that some odors induce U, others induce P, whereas others are more or less indifferent. The spread or variability in judgments is not uniform. No. 4 with a wide spread has intentionally been placed next to No. 33 to show this contrast. From the statistical standpoint odors might be ranked in "degree of indifference" just as definitely as in degree of P or U. From this point of view the empty bottle used as a control is the most indifferent member of the series.

Next we desired to see whether the affective judgments would be constant from day to day. Two *S*s, a man (*A*) and a woman (*B*), made affective judgments for 12 successive experimental days. The odors, instructions, procedure, etc. were as above except that a new haphazard order of presentation was used every day to eliminate learning of the series, expectation, and to distribute by chance the effects of contrast. Two odors and the empty bottle may serve to illustrate the result.

		Successive Days											
S	Odor-No.	1	2	3	4	5	6	7	8	9	10	11	12
A	21	−1	−3	−2	−3	−3	−3	−3	−4	−2	−3	−3	−3
	29	0	+1	+1	+1	+1	+1	+1	+3	+3	0	+2	+3
	33	−2	0	0	−2	0	0	−2	−1	0	0	−1	0
B	13	+4	+5	+5	+4	+4	+3	+4	+4	+2	+5	+5	+5
	1	−3	−4	−5	−4	−4	−5	−4	−4	−5	−3	−5	−3
	33	0	0	0	−3	0	0	0	0	−1	0	0	0

Considering the series of judgments we find definite uniformity. Some are consistently P from day to day, some consistently U, some (not shown) vary about I (indifference).

The total experiment demonstrates the existence of statistical constancy in affective judgment to odors. The result is neither new[5] nor startling but it is theoretically important. The experiment shows that we are in the presence of a genuine problem even though, as we shall see, the method is not ideal.

(*B*) *Difficulties with the scale-of-values method.* (*1*) *Affective judgment equivocal.* The affective judgment is equivocal regarding the existence or non-existence of felt experience. Objects may be judged affectively with little regard to the felt pleasantness and unpleasantness of experience at the time the judgment is made. The evaluation of an object as 'pleasant' or 'unpleasant' tells nothing directly about the felt experience of the subject.

This was demonstrated experimentally by Yokoyama[6] for the method of paired comparisons. He concluded that "conscious P and U drop away from the affective perception in the course of its decay, thus following the law of perceptual contextual sensations," and that "*P* and *U* (*of the method of paired comparisons*) are most universally and definitely statable as meanings." These meanings of P and U should not be confused with affective experiences.[7]

In the above experiment 2 *S*s showed an appreciable degree of constancy in affective judgments to odors during a period of 12 experimental days. Possibly, however, the *judgments* are constant whereas the *felt experience* tends to drop out with habituation. The ambiguity of the affective judgment regarding felt experience makes it difficult to interpret the results.[8]

To make the case clear let us look at it physiologically. When an odor is presented for the first time to a subject it may bring

[5]See the writer's study referred to in foot-note 4.

After the above experiment a few of the odors were selected and given to a group of 57 additional *S*s. The aim of the test was to check the predictive value of the results. With the smaller group No. 30 was reported U by 2 *S*s, as I by 1, and as P by 16. We predicted that it would be reported P by the larger group; the result was U 7, I 0, P 50. With the smaller group No. 16 was reported U by 15 subjects, I by 2, and P by 2. We predicted that it would be reported U by the larger group; the result was U 55, I 0, P 2. Predictions regarding like and dislike are thus possible for a group; but this, of course, does not mean that we can tell how the individual *S* will react in any particular case.

[6]M. Yokoyama, The nature of the affective judgment in the method of paired comparisons, this JOURNAL, 32, 1921, 369.

[7]For discussion of this point see P. T. Young, The coexistence and localization of feelings, *Brit. J. Psychol.*, 15, 1925, 356 f.

[8]This JOURNAL, 40, 1928, 399-400.

about a smile, muscular relaxation, or a mild tendency to vomit or to hold the breath. Along with these bodily changes may be some verbal expression such as "the odor was P" or "I didn't like that." With repeated presentations of the odor the verbal reactions may

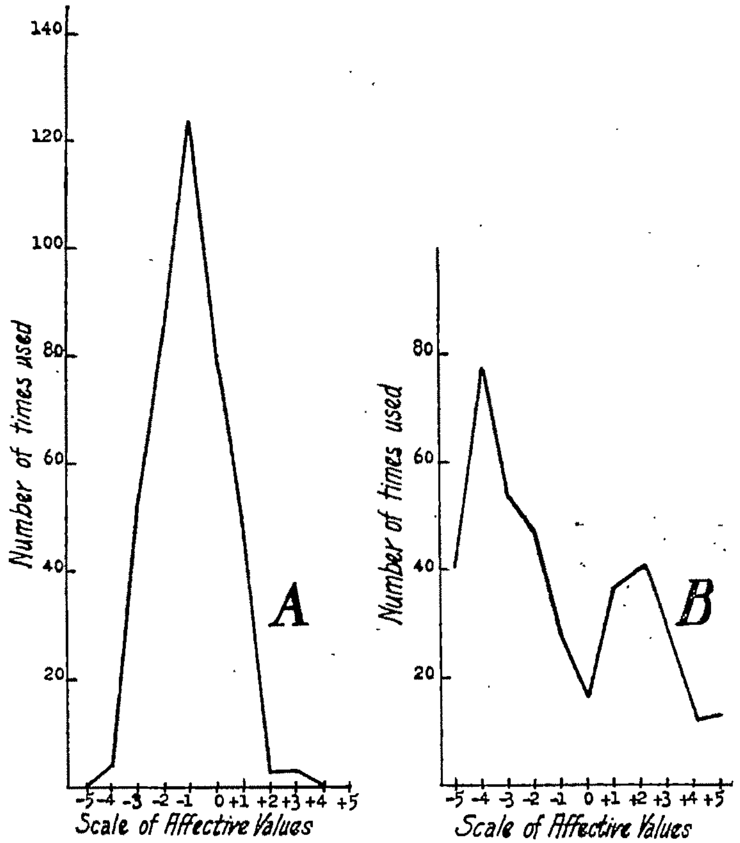

FIG. 1. SHOWING THE DISTRIBUTION OF *A*'S AND *B*'S AFFECTIVE JUDGMENTS

persist while the smiling, relaxing, vomiting, holding of the breath, etc. disappear. Thus the verbal equivalent may remain in the absence of the other reactions. In every-day life we know that this happens and that such a statement as "the object was pleasant" is equivocal regarding the emotional and affective reactions of the *S* at the time the statement is made.

This equivocal nature of the affective judgment is a difficulty common to all forms of the so-called method of impression; scale

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of values, paired comparisons, order of merit, single stimulus, method of choice, etc. The methods are of little value in the study of felt experience.

(2) *Effect of S's attitude.* A second difficulty with the foregoing experiment is that every judgment depends to some extent upon the *S*'s attitude towards the scale of values. To illustrate the effect of attitude let us consider the above experiment in which two *S*s, *A* and *B*, made affective judgments upon the 33 stimulus-objects for 12 successive experimental days. With what frequency was each point on the scale of values used by these 2 *S*s for the total experiment? Fig. 1 answers the question graphically, showing the distribution of the 396 affective judgments given by each *S*.

For *A* the curve is unimodal and the extremes of the scale are rarely if ever used; the most frequently used category is -1. For *B*, on the other hand, the curve is bimodal with a distinct drop at 0. The modes are -4 and $+2$; the extremes of the scale are used with significant frequencies.

How shall we interpret the difference between the curves? If we were to assume that the curves show something about the affective processes, we should proceed as follows. Both curves show a predominance of U over P which indicates that, with the selection of stimuli in the experiment, U experiences occurred more frequently than P. *A* did not experience intense P and U during the experiment but found most odors indifferent or slightly U. *B* was more sensitive to the odors than *A*, *i.e.* more apt to experience either P or U and especially more apt to experience the relatively intense P and U.

Although such an interpretation in terms of affective process is possible, it is nevertheless equivocal. A second type of interpretation follows. The curves indicate that the *S*s apprehended the scale of values differently. *A* probably considered the scale to be a single continuous series whereas *B* looked upon it as two scales, one for P and one for U with indifference between them. The extremes "very great pleasure" and "very great displeasure" were interpreted differently by the *S*s. For *A* the interpretation of $+5$ and -5 was such that affective processes of these intensities did not occur in the experiment. For *B* the scale was apprehended in such a way that the extremes occurred with significant

frequencies. The difference between the curves is a matter of the *S*'s attitude—of the way in which the scale is apprehended. Since the use of the scale is an individual and purely subjective matter we have no way of knowing whether the intensity of P felt by *A* and designated +3 corresponds to the intensity felt by *B* and designated +3.

Thus there are at least two ways to interpret the curves. If we assume that the *S*s are alike in their affective processes, we may then conclude that the curves show a difference of attitude towards the scale of values. If, on the other hand, we assume that the *S*s are similar in their attitude towards the scale, we may conclude that they differ in their affective processes. The latter interpretation is rendered more uncertain by the possibility that an affective judgment may be made in the absence of affective experience.

Unfortunately we have no statement from the *S*s regarding their attitude towards the scale of values. We may appeal, however, to other experimental evidence. Conklin[9] in 1923 published a study of the scale-of-values method. He used a scale of 13 points:

1	Greatest possible pleasure	565
2	Very, very great pleasure	359
3	Very great pleasure	530
4	Great pleasure	1025
5	More than a little pleasure	633
6	Just the slightest pleasure	785
7	Neutral, neither	1196
8	Just the slightest displeasure	851
9	More than a little displeasure	779
10	Great displeasure	1232
11	Very great displeasure	581
12	Very, very great displeasure	479
13	Greatest possible displeasure	1179

The figures at the right show the total frequencies for every category of judgment by a group of 1699 public school pupils when judging propositions such as "to do ridiculous initiation stunts in public." One is impressed immediately by the high frequency for the values 4, 7, 10, 13. Regarding this matter Conklin writes: "It becomes evident upon examination of these distributions and graphs that there is a free use of terms 1, 4, 7, 10, and 13. The frequent use of these indicates that they describe clearly distinguishable varieties of experience." The writer would like to suggest that these high values do not indicate "clearly distinguishable varieties of experience" but rather a general preference for certain categories of judgment on the scale of values. The scale is symmetrical and the points 1, 4, 7, 10, 13 are easily regarded as round numbers and convenient points of orientation towards the scale.

[9]Reference in footnote 4.

Conklin used the same scale to study belief and disbelief in propositions presented to the same *Ss*—the term "belief" being substituted for "pleasure" and "disbelief" for "displeasure." The curves for belief and disbelief also show marked peaks at the points 1, 4, 7, 10, 13 similar in every respect to those on the curves for pleasure and displeasure. Are we then to assume that the "experiences" of belief and disbelief indicated by points 1, 4, 7, 10, 13 are of more frequent occurrence than those indicated by other points? If so, it is a curious fact that the frequency distribution for belief-disbelief "experience" corresponds so closely to that for pleasure-displeasure.

Conklin's result reminds one of Coover's[10] well-known study of mental habit in relation to judgment. Bringing together available evidence from a wide variety of fields Coover demonstrated a preference for round numbers. In the twelfth and thirteenth census reports, for example, the figures indicate that persons give their ages most frequently in multiples of 5 and 10. Also when students' grades are given on a percentile basis the figures indicate that the teachers have a distinct preference for multiples of 5 and 10. Coover found that judges in giving criminal sentence have a marked preference for round numbers when the sentence is in years, and for quarters of a year when the sentence is in months. Further, in the estimation of star magnitudes a preference for round numbers is manifest. For example, one bit of evidence gives 1239 stars of the 6.5 magnitude and only 159 of the 6.6. Similar numerical preferences were found in the estimation of meteorological conditions and in guessing.

It is not surprising, therefore, that certain categories of affective judgment on Conklin's scale of values should be used with much greater frequency than others. Further evidence of number-preference may be indicated by the following experiment.

A random list of 43 common foods[11] was prepared and the list read slowly to the experimental group of 19 *Ss*. The group was instructed to estimate their degree of liking or of disliking for the food suggested, in terms of the above 11-point scale which was printed on the blackboard. In explaining the scale the *Ss* were told to use +3 and −3 for average liking and disliking. Possibly this explanation had some effect upon the result.

Fig. 2 shows the frequency with which each point on the scale was used by the total group. About 80% of the judgments are on the P side of the scale.

[10] J. E. Coover, *Experiments in Psychical Research at Leland Stanford University*, 1917, Part III, Mental habit and inductive probability, 229 f.

[11] The foods suggested are recorded below. Qualifications in parenthesis were made at the time the test was given in answer to the questions of the group. The general instruction was to consider the food as well-cooked but plain, without sauce or dressing, and to estimate immediately the degree of like or dislike for the suggested food: fried parsnips, stewed onions, raw oysters, fried oysters, fresh milk (not warm), raw carrots, uncooked apple, orange, banana, pineapple (raw), vanilla ice cream, mashed potato, raw tomato, cooked tomato, crisp bacon, soft (not crisp) bacon, roast beef (rare), roast beef (well done), mutton chops, boiled cabbage, saur kraut (cooked), cauliflower, boiled turnip, raw young onions, radishes, fried chicken, stewed chicken, baked chicken, fried fish, baked fish, boiled egg (soft), fried egg, raw egg, lettuce, lemonade, buttermilk, coffee with cream and sugar, plain coffee (nothing added), tea with cream and sugar, plain tea, cocoa (sweetened), dry toast, buttered toast.

The significance of this fact is obvious. Common foods are generally liked; if disliked, they would not be in common use. The curve is bimodal with one mode at +3 and the other at −3 and the lowest point curiously enough at −1. The relatively high frequencies for +3 and −3 correspond to Conklin's result

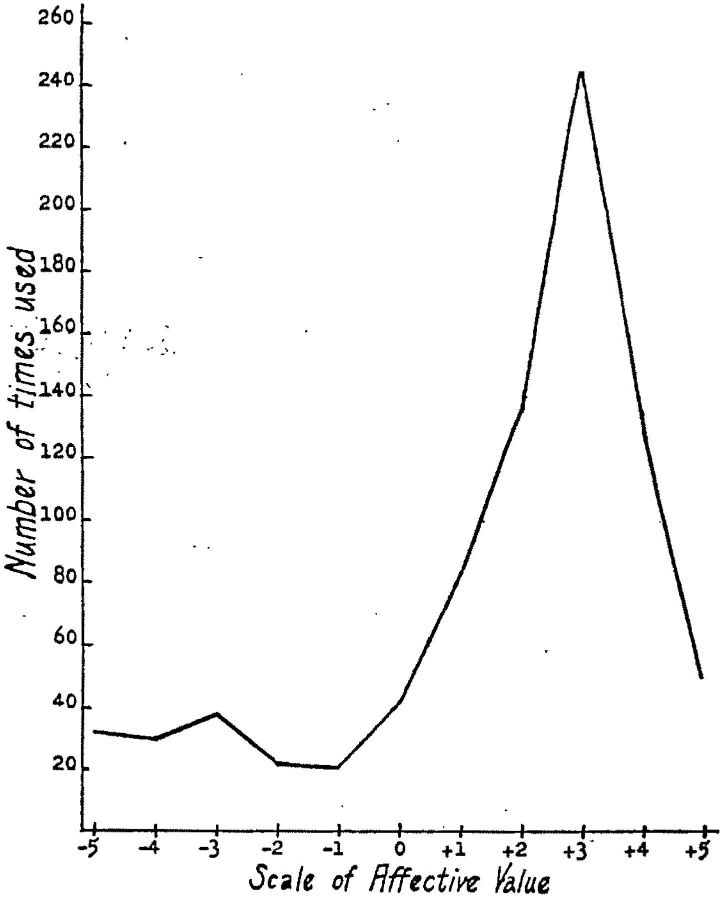

FIG. 2. SHOWING THE DISTRIBUTION OF THE *S*s' USE OF THE VARIOUS POINTS OF THE AFFECTIVE SCALE

that the central points of the scale for P and for U have relatively the greatest frequency.

In view of the above evidence we conclude that the *S*'s attitude towards the scale of values is one factor which determines the result. The way in which he apprehends the scale and his preference for certain categories of judgment are factors involved in this method.

(*C*) *Utility of the scale-of-values method.* (*1*) *Rating of objects.* A great deal of affective psychology has concerned itself

with the evaluation of objects in terms of P and U. Typical questions are "Which of these bits of colored paper is the most P?" "How may these odors, tones, drawings, geometrical figures, etc. be rated as regards P and U?" Similar questions have been formulated on the level of objects and in terms of objects.

The evaluation of objects may have some practical importance. A merchant may want to know in advance how this or that fabric will be liked by the public. A customer may face the problem of rating affectively a series of photographic proofs. In these and similar cases some practical method for determining relative like and dislike is needed.

But the psychologist questions whether such gross rating of objects has any scientific importance. Suppose all the objects in the world were rated affectively by all the persons in existence, what would be gained? This would throw little light upon the nature of felt experience, the nature of affective reactions in the organism, and other matters of psychological importance.

(*2*) *Study of affective reactions.* *C*, a woman, made successive affective judgments upon the 33 stimulus-objects under conditions identical with those described above for *A* and *B*. On the fourth day of the series *C* was distinctly in a U mood. She was dull, cross and her general behavior corresponded to the verbal report of mood. On this day U odors were judged more intensely U, P odors were less P, weak P became U, the empty bottle was judged as −3. In a word, the U mood accompanied a decided swing of all judgments towards U. This may be shown statistically by adding together all of the U-values and also all of the P-values for every day of the experiment.

Day No.	1	2	3	4	5	6	7	8	9	10	11	12
U	−68	−60	−54	−86	−67	−77	−67	−67	−58	−63	−69	−66
P	+24	+40	+33	+ 8	+25	+19	+27	+29	+20	+25	+33	+32

The writer hesitates to interpret the result but the fact is clear that a distinctly U-mood on day 4 coincided with a decided swing toward U affective judgments.

The above result shows that it may be possible to study daily variations in affective reaction and individual differences within a group. The main requirement is keeping the stimuli and objective conditions constant.

SUMMARY

The scale-of-values method may yield statistically uniform results. The interpretation of these uniformities is doubtful because (a) the affective judgment is equivocal regarding the existence of felt experience, and (b) the *S*'s attitude towards the scale is a factor determining judgment.

The method finds application in two ways; (a) in the affective evaluation of objects and (b) in the study of affective reactions of the *S*. The former may be of practical value but is of little psychological importance; the latter opens the way to the study of individual and group differences in affective reaction.

IX. THE POINT OF VIEW OF AFFECTIVE PSYCHOLOGY

The problem. A good way to approach affective psychology is by making an actual demonstration. Prepare (a) a bottle of dead fish, (b) several perfumes, (c) a lump of sugar, (d) an intense quinine solution, (e) a soft warm object, and (f) a needle for puncturing the skin. When these and similar objects are presented to the nose, tongue or skin, as the case may be, *S* will normally report that (a) (d) (f) are displeasing and (b) (c) (e) are pleasing. The demonstration presents most convincingly a central problem of affective psychology. Some situations evoke an experience described as pleasing, agreeable, acceptable; others an experience described as displeasing, disagreeable, unacceptable. The genuineness of the difference is guaranteed by the statistical constancy of the reports when the demonstration is given to a group.

The demonstration may be viewed in two ways, either from the point of view of the *S* or from the point of view of the demonstrator. From the first point of view there is a *felt experience*—a pleasing or displeasing character to the total experience—which is verbally reported. From the second point of view there are certain *affective reactions*—smiling, frowning, withdrawing, relaxing—which the demonstrator may observe. Ordinarily the affective reactions may be distinguished from others not markedly affective, such as counting out loud, opening and closing the eyes, moving the fingers, etc.

The writer believes that both aspects reveal a single natural process going on within the organism. Any one can observe

both aspects of the affective process by going through the demonstration first as *S* and then as demonstrator. The present paper is limited to the first of these aspects.

The nature of felt experience. Two views of the nature of felt experience are rejected. First, there is the view that P and U are attributes of sensation. This has been discredited by Külpe.[12] Secondly, there is the view that P and U are statable as meanings. This view was expressed by M. Yokoyama.[13]

Two views of the nature of felt experience demand serious consideration; the sensational and the traditional non-sensational views. These will constitute the main topic in the following discussion.[14]

(*A*) *The sensational view of felt experience.* (*1*) *The method of sensory observation.* An analysis of the method of observation in the natural sciences brings to light the following facts.[15] (1) Every observation involves some phenomenon which constitutes its object. Men of science have a bias towards visible phenomena, but this is not a necessary state of affairs since pains, pressures, tones, noises, tastes, smells, etc. are all observable as truly as visible objects.[16] (2) Every observation involves a set, or determination, on the part of the observer. This, as we have pointed out,[17] may be conceived in either of two ways: (a) as a physiological adjustment of the neuromuscular system determining bodily reaction, or (b) as a logical proposition stated in

[12]O. Külpe, *Outlines of Psychology*, (E. B. Titchener, tr.) 1893, 227 f.

[13]This JOURNAL, 32, 1921, 357-369. The meaning hypothesis is rejected on the following grounds: (a) The term 'meaning' is ambiguous and ill-defined. Psychologists would do well to follow Professor Calkins' advice and "to abjure entirely the use of the slippery conception, 'meaning'" (this JOURNAL, 39, 1927, 7-22). (b) Studies of the writer (see reference in footnote 7) have repeatedly pointed out the difference between purely cognitive and affective processes. It is important to distinguish between affective and purely cognitive processes rather than to blur the distinction by the use of such a term as 'meaning.'

[14]An historical survey will not be attempted. The reader would do well to see E. B. Titchener's *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, Chap. 3, in which C. Stumpf's view that affections are *Gefühlsempfindungen* is presented and discussed.

[15]Much of the material in the following discussion was presented at the Eighth International Congress of Psychology, Groningen, Holland, September, 1926, in a paper entitled, *An analysis of observation in the field of affective psychology*.

[16]H. L. Hollingworth, Sensuous determinants of psychological attitude, *Psychol. Rev.*, 35, 1928, 93-117.

[17]P. T. Young, The phenomena of organic set, *Psychol. Rev.*, 32, 1925, 477-478. Set is also implied in such words as 'purpose in observing,' 'attitude,' 'preparatory adjustment,' '*Aufgabe*,' '*Einstellung*.'

words or other symbols without reference to the bodily basis. (3) The bodily process of observing may be reduced to simpler bodily processes in the receptors, neurones, muscles, and other structures. This view of observing is incomplete because it leaves out of account the phenomenon observed. (4) Scientific observation is generally followed by a report which expresses in words or other symbols the phenomenon observed. Record is made of the object, event or relation which is observed.

Within psychology observation is often of the above type. For example, when Fernberger[18] observed the hallucinatory images after taking the drug *peyote* (*Anhalonium Lewinii*) his manner of observing was identical with that found in the other sciences. In the numerous investigations upon sensory processes one can find illustrations without limit of this objective type of sensory observation.[19]

(*8*) *Titchener's view of the affective processes.* Titchener has stated that the method of psychology is observation. "And observation," he writes, "implies three things: a certain attitude towards phenomena, a vivid experience of the particular phenomenon which is the object of observation, and an adequate report of this experience in words."[20]

[18]S. W. Fernberger, Observations on taking peyote, this JOURNAL, 34, 1923, 267.

[19]Granting that the method of observation is the same inside and outside of psychology, the question arises "How does psychology differ from physiology, physics, chemistry, and the other natural sciences?" In the writer's view the traditional gulf between physical and psychological science exists only for those who have made it. The main difference between psychology and the physical sciences is the place where explanation is sought. The psychologist looks always to the body of the observing organism to explain phenomena. Ernst Mach summed up the matter very well when he wrote: "A color is a physical object so long as we consider its dependence upon its luminous source, upon other colors, upon heat, upon space, and so forth. Regarding, however, its dependence upon the retina . . ., it becomes a psychological object, a sensation. Not the subject, but the direction of our investigation, is different in the two domains." E. Mach, *Contributions to the Analysis of the Sensations*, (C. M. Williams, tr.) 1910, 14-15.

[20]The word 'introspection' has been avoided because it is frequently used as a reproach or condemnation of something bad in the methodology of psychology. Titchener is often regarded as the arch-introspectionist, but he himself expressed hesitation in the use of the word. I quote: "I have avoided the term 'consciousness.' Experimental psychology made a serious effort to give it a scientific meaning; but the attempt has failed; the word is too slippery, and so is better discarded. The term 'introspection' is, I have no doubt, travelling the same road; and I could easily have avoided it, too; but the time is, perhaps, not quite ripe." (E. B. Titchener, *A Beginner's Psychology*, 1922, x.) Titchener has also stated precisely what he means by introspection. Generally he means scientific observation within psychology in the sense that a discrimination between white and black is an introspection. E. B. Titchener, Prolegomena to a study of introspection, this JOURNAL, 23, 1912, 427-448; The schema of introspection, *ibid.*, 23, 1912, 485-508. See also H. P. Weld, *Psychology as Science*, 1927, 66.

Regarding the nature and place of observation in psychology see a recent paper by C. C. Pratt, Theoretical studies from the Harvard psychological laboratory; faculty psychology, *Psychol. Rev.*, 36, 1929, 147 f.

Now according to Titchener's earlier view of affective experience the chief mark of P and U was lack of clearness or vividness. "Attention to a sensation means always that the sensation becomes clear; attention to an affection is impossible. If it is attempted, the pleasantness or unpleasantness at once eludes us and disappears, and we find ourselves attending to some obtrusive sensation or idea that we had not the slightest desire to observe."[21] Elementary affective processes have the attributes of quality, intensity and duration but lack the attribute of clearness. When Titchener writes that affective processes lack clearness he means precisely what he says although this point has been frequently misunderstood.[22]

Titchener's position contained a fundamental contradiction. To say that P and U are not clear is to say that they are not observable (as observation is defined). How then can the affective processes be brought within a psychology based upon direct observation as its fundamental method? Titchener was aware of the difficulty and attempted to get around it in the following way: "In the case of sensation, the observer is set or disposed, beforehand, to attend to sensation and to report upon sensation; the sensation comes, and is attended to; and the report which follows is determined, under the influence of the preliminary set or disposition, by the nature of the sensation. In the case of feeling, the observer is set to attend to sensation, but to report upon the feeling which accompanies the sensation; the sensation comes and is attended to; and the report then describes, under the influence of the preliminary set, the feeling which accompanied the sensation. That sounds a little paradoxical; but the method is not difficult in practice; and it has the advantage that we can use all manner of sensory stimuli (colours, tones, everything) in our study of feeling."[23]

Despite the attempt to cover up a difficulty Titchener's logic is bad and reminds one of the attempt to eat the cake and keep it too. He admits into his system of psychology non-clear, non-observable affective processes. Hence in the interests of consistency he would do well to change the statement regarding psychological method so as to include processes which are reportable but not attentively observable, namely the affective processes.

Doubtless Titchener's ground for retaining non-sensory elements in his system of psychology rested upon the belief that pleasantness and unpleasantness are similar in many respects to sensory processes and are, perhaps, unde-

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[21]E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 69.

[22]Titchener's position is clearly shown in the following reply to Watt: "Meanwhile, Dr. Watt's own logic has not been adequate to the theory which he attacks. 'Are our feelings so unclear?' he asks. As well might he ask: Are our spaceless sensations so very small? A process can be unclear only if it can also, under other conditions, be clear; and, on my view, the affective processes are neither clear nor unclear, but non-clear; they do not vary at all, as sensations vary, from vivid to dim, from clear to obscure; they show, in experience, neither vividness not obscurity; they lack the attribute of clearness in precisely the same way in which they lack the attribute of space. . . . And the phrase 'total absence of clearness' means for me what it says: which is something very different from 'total obscurity.'" E. B. Titchener, Feeling and thought: a reply, *Mind*, N.S. 20, 1911, 258-260.

[23]E. B. Titchener, *A Beginner's Psychology*, 1915, 80.

veloped sensations. P and U, Titchener states, resemble organic sensation; they have the attributes of quality, intensity, duration; further, they show adaptation, and possibly in other ways are similar to sensation. Titchener is definitely biased in favor of a sensational view of P and U. He favors the view that P and U have peripheral organs. I quote: "The writer hazards the guess that the peripheral organs of affection are the free afferent nerve-endings —what are ordinarily called the free sensory nerve-endings—distributed through the various tissues of the body; and he takes these free endings to represent a lower level of organic development than the specialised receptive organs, or organs of sense. Had mental development been carried further, pleasantness and unpleasantness might have become sensations: in all likelihood would have been differentiated, each of them, into a number of sensory qualities."[24]

We now know that Titchener's sensationalistic leanings finally dominated his theorizing and that toward the close of his life he came to regard P and U as sensory processes. The earlier view of affective processes gave way to a thorough-going and consistent sensationalism. Titchener's belief that affective processes are similar to sensations in several fundamental respects foreshadows a totally different systematic position which in recent years has come out of the Cornell Laboratory.

The modern view finds its clearest expression in a paper by J. P. Nafe[25] and may be described as follows. According to Nafe, Titchener accepted the following list of simple touch qualities: tickle, itch, contact, pressure, prick, clear pain, quick pain, strain, drag, ache, cold, warmth, heat, bright-pressure, dull-pressure. The last two are important in the present connection so we quote from Nafe in detail.

"*Bright-pressure.* Bright-pressure and dull-pressure are the two of Titchener's qualities least known. He describes bright-pressure as a diffuse bodily feel, bright, buoyant; an experience that goes with a general feeling of well being. It is not described as containing an element of pressure, pressure being, in this instance, a general term. The experience could as well have been called a 'felt-brightness' and it does not necessarily occur so diffused as to include the whole body.

"*Dull-pressure.* Dull-pressure is a diffuse bodily feel also, but duller, heavier, more as one feels the day before one comes down with influenza, yet not strong like drag. These terms came into use with a fairly large group but have never been generally accepted."

According to Nafe[26] felt pleasantness when directly observed *is* bright-pressure and unpleasantness *is* dull-pressure.

Writing upon the change in Titchener's formulation Nafe states[27] that the criterion of affective experience had been its unobservability; but by 1921 Titchener "had realized the impossibility of such a condition of affairs without much urge from the outside world." Consequently an experimental problem

[24] E. B. Titchener, *A Text-book of Psychology*, 1909, 261.
[25] J. P. Nafe, The psychology of felt experience, this JOURNAL, 39, 1927, 367-389.
[26] An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.
[27] *Op. cit.*, *ibid.*, 39, 1927, 386.

was formulated and this gave rise to the view expressed by Nafe. "This problem restored the affective qualities as observable phenomena and furthermore put them in the same category with other experience (sensory); but they were found to occur only as undivided parts of other experiences, never alone." Nafe concludes "that affective and emotional experiences are fundamentally similar to other ('sensory') felt experiences." The same view has been expressed by Hoisington.[28]

Thus if we look at the case historically it is obvious that Titchener's systematic position has changed. In 1918, when the writer was experimenting in his laboratory, P and U were regarded as similar in several respects to sensation but different in that they lacked the attribute of clearness. Affective processes were unobservable (as the method of observation was defined) and bright-pressure and dull-pressure were not yet in the air. Ten years later P and U are described as observable processes—P a bright-pressure and U a dull-pressure. The original inconsistency in Titchener's systematic formulation has been resolved by the 'discovery' that P and U are observable sensory processes. Titchener's latest position is a clean-cut sensationalism which is logically self-consistent. The earlier affective processes have vanished!

The change has been in the direction of logical consistency and we may now summarize the present position as follows: The method of science is direct observation.[29] This method is to be held consistently. Direct observation gives only observable processes. They alone constitute the subject matter of a scientific psychology. Non-clear affective processes do not belong within psychology.

Criticism of the sensational view of affective processes. It is an experimental problem to determine precisely what is observed when a sensational attitude[30] is taken towards P and U. If P and U are observable, they are either (a) ultimate elementary processes in the cutaneous-kinesthetic-organic group, or (b) characteristic integrations which may be reduced through analytical observation to elementary components.

The writer is skeptical about the bright- and dull-pressure view for reasons given earlier in this series of studies. There is doubtless an association between P and bright-pressure and between U and dull-pressure; but the existence of an indispensable sensational condition of felt experience has yet to be demonstrated. The statement that P *is* a "bright-pressure" implies that every

[28]L. B. Hoisington, Pleasantness and unpleasantness as modes of bodily experience, *Wittenberg Symposium on Feelings and Emotions*, 1928, 236-246.

[29]See footnote 20.

[30]P. T. Young, Studies in affective psychology, this JOURNAL, 38, 1927, 170-171. In the present connection it may be well to note the view of Lehmann that "Die historische Tatsache aber, dass die Psychologen über die Natur der Gefühle nicht einig werden können, beweist zur Genüge, dass Lust und Unlust keine Empfindungen sind; sonst hätte man sich längst verständigt." A. Lehmann, *Die Hauptgesetze des menschlichen Gefühlslebens*, 1914, 21.

time P is felt sensorial observation will yield a "bright-pressure" and never a "dull-pressure" and never the absence of all pressure experience. The matter needs further investigation in which negative cases are stressed.

Crosland[31] working upon the problem of forgetting studied affection incidentally and found no constant relation between occurrence of affective process and occurrence of any particular kinaesthetic and organic process. In an earlier study of this problem I concluded that there is no sensory *sine qua non* of affection.[32] The situation seems to be the same as that with the so-called expressive methods. There are associations—as between P and vasodilation or U and vasoconstriction—without any one-to-one relationships. There are no hard and fast connections.

This being the case we must produce further experimental evidence before concluding that P and U are directly observable as observation has been defined. At present I do not see how one can claim that P *is* this or that process or integration.

Let us now look at the situation from the logical point of view. We started by defining the method of observation, determined to hold to that method consistently and to accept whatever result it might yield. Prior to experimentation we may know on purely logical grounds that the method of observation is limited to the study of observable processes. From the nature of the case non-observable processes do not and cannot come into the picture. This is a truism.

If we lay it down as a matter of definition that whatever is psychologically observable is a sensory process, we have solved the problem in advance. Whatever the method yields is sensory. The method cannot give any evidence for the existence of non-sensory, non-observable processes.

Thus from the logical point of view Titchener's sensationalism does not nor can it settle the question regarding the existence of non-sensational processes of experience. Sensationalism is merely a determination to hold to a particular point of view and method and to accept whatever results. In itself this is commendable for scientific advance is achieved through specialization.

[31] H. R. Crosland, A qualitative analysis of the process of forgetting, *Psychol. Monog.*, 29, 1921, (no. 130), 143. Crosland also notes (92 f.) that there may be all possible relations between strain and relaxation, on the one hand, and P and U, on the other hand; but in general U most frequently occurred with kinaesthesis or strain and P with decrease of strain or relaxation. This general conclusion agrees with my own on the point (this JOURNAL, 32, 1921, 39 f.). The two studies were done independently and published the same year.

[32] *Op. cit.*, *ibid.*, 32, 1921, 39 f.

(*B*) *The non-sensational view of felt experience.* It was previously stated that phrases such as "I like it," or "I dislike it" may indicate the existence of felt experience without being sensational descriptions of it.[33] A verbal report may inform us of the qualitative character, the intensity, or the temporal course of affective experience. It may be argued, of course, that such phrases do not *describe*[34] the experience but merely indicate a gross pattern which, if fully *described*, would reduce to sensational processes.[35]

Whether the functional report is descriptive or not depends entirely upon what we mean by description. The functional report is not analytical; nor does it presuppose the sensational point of view. It does, however, give significant information about human experience; and it makes possible the study of significant problems which lie entirely outside of sensational psychology.

These problems lie in several fields, in differential psychology (see Part VIII), in physiological psychology (see Part VII), and in social and abnormal psychology.

If we admit that the functional report is a valid psychological method distinct from the method of sensory observation, we must not forget that there are still dangers and limitations. One of the chief difficulties with the functional report is the ambiguity involved. We have repeatedly pointed out that a report may confuse the merely cognitive evaluation of an object in terms of P and U with the direct report of felt experience. This ambiguity is responsible for much of the disorder prevalent in affective psychology to-day.

Let us now consider the question whether there exist any non-sensational affective processes of experience. Clearly if we start from the bias of sensationalism, the question can be answered in only one way. The determination to limit the data of psychology to those gained by the method of direct sensory ob-

[33]This JOURNAL, 38, 1927, 172.

[34]See Hoisington, *op. cit.*, 236. See also R. H. Wheeler, Analyzed versus unanalyzed experience, *Psychol. Rev.*, 29, 1922, 425-446.

[35]Relative to this proposition it might be said that sensational psychology itself accepts the functional type of report as indicative of unanalyzed experience and at least to this extent recognizes the usefulness of the method. Further, it might be argued that the functional report is a kind of observation and description which, however, is not sensational.

servation solves the problem prior to experiment. The whole of human experience is sensory, if we are determined to view it that way. If, however, we admit the validity of the functional approach, the question appears in a new light. We may claim, if we will, that the functional approach yields a direct report of non-sensory processes and characteristics of experience, and that sensational analysis is beside the point. Or we may claim, on the other hand, that the functional method yields only reports of gross, unanalyzed experience. Practically it doesn't much matter which view of the case we accept. Experimental work from the functional point of view need not await a final answer to these systematic problems. Perhaps there is no final answer.

In general, the writer prefers to make no advance assumptions about experience but to start affective psychology with the concrete demonstration of genuine problems. Various methods, points of view and approaches may be distinguished. Sensationalism is not the whole story. The functional approach is also useful.

X. Some General Conclusions

In 1917-18 the writer experimented upon affective processes in the Cornell Laboratory and tacitly accepted the Titchenerian view that P and U are non-sensory processes of experience. Since that time further experimentation has led to considerable skepticism regarding the methods and significance of the earlier work. After several seemingly futile experiments the present series of studies was commenced in the hope that light would be thrown upon the methodological difficulties.

Looking back over the studies one principle stands out prominently, the relativism of facts of observation to methods and postulates. Two observers—a behaviorist and a Titchenerian psychologist—were placed in the same experimental situation (Part III) and each reported consistently with his own theoretical biases. Again, two groups of Titchenerians—one in 1918 and another ten years later—in the same laboratory gave wholly different results. What follows? The only fair conclusion is that the facts of affective psychology are largely *made* before experimentation is commenced, *made* by the training, education, theoretical views of the subjects. The faith of the physical scientist that there are facts of observation independent of the observer is not valid. It is shattered by the finding that to a great

extent facts of observation may be made to order. One can train a group of observers to report A=B and another group to report A≠B.

The situation needs to be viewed as a problem in the psychology of opinion. In the everyday world men differ in political, ethical, religious, social, philosophical, scientific and other opinions. Here and there two individuals or a group may agree but a universal agreement is out of the question. In psychology the case is the same. The attitudes, points of view, theoretical beliefs and hypotheses of psychologists have *made* affective psychology what it is to-day. Even the 'facts' from which we start have to a large extent been *made* in advance.

The two roads. We come now to the parting of the ways. (1) We can hold dogmatically and persistently to some particular point of view, method, system of postulates, and accept the facts which our bias yields, rejecting all else as valueless, or (2) we can attempt to define psychology in such a way that all biases, attitudes, points of view are on the same level as parts of nature. In the latter case we admit freely that facts of observation are completely relative to biases of the observer.

Along the first road we have the satisfaction of distinguishing between true and false in terms of distinctions which have been made in part prior to experimentation. Along the second road the futility of attempting to discover 'truth' by present methods is granted; facts of observation are completely relative to a postulational system. The first road defines sharply the method of sensory observation and holds to it through thick and thin. This determination yields a sensationalism in which uniformities are discovered, facts ordered, and principles formulated. The second road reveals at the start that the method of observation has limitations when used as an instrument in search for 'truth.' The attempt to determine what is 'true' and what is 'false' along present lines is abandoned.[38]

The recognition of the principle that facts of observation are completely relative to the past history of the observer brings to light a fundamental principle of nature. Discrepancies within psychology are viewed exactly as we view discrepancies in the larger world where opinions clash. Psychologists after all are

[38]Cf. *op. cit.*, this JOURNAL, 38, 1927, 188 f.

human beings. From a relativistic point of view one can view calmly the prevailing situation in contemporary affective psychology.

Available methods. If one desires to free psychology so far as possible from the opinion of psychologists and to rest the science upon the ultimates of human experience, the question of methodology arises. Apart from the above remarks the present series of studies has thrown some light on the methodology of affective psychology. A brief summary may be in order.

Part VI demonstrated that problems of relative preference can be studied with animal subjects.[37] Employing this method one could easily work for a life-time upon problems of like and dislike. The method is behavioristic and physiological. It has the advantages of physical science, but it tells nothing directly about felt experience.

Part IX presented the affective problem in human psychology by means of a demonstration.[38] If we accept the functional type of report, various lines of investigation in differential, physiological, social, abnormal, and applied psychology are in plain view before us.

As regards sensationalism the future is good. The sensational approach to problems of feeling and emotion presupposes certain postulates and methods. It presupposes that the observers have an understanding of the psychological vocabulary of the day, that they have had a specialized type of training. The subjects are for the most part trained psychologists. There is no doubt that the careful extension of sensational psychology to the problems of feeling and emotion will bring to light matters of considerable interest and theoretical importance.

Our study has not carried the affective problems into the fields of the abnormal and the social in psychology. In these fields the functional approach is best, for the patients and subjects are not trained psychologists. Ordinarily in the fields of the abnormal and of social psychology no one troubles about the truth or falsity of some particular attitude. It is common to start from the verbal and other expressions of the subject and to account for them genetically. The psychological competence of the subject does not come much into the picture; but the competence of the investigator is a matter of great importance.

[37] *Op. cit., ibid.*, 40, 1928, 372-394.
[38] *Supra*, 27.

ON MAYER'S "RESIDUAL SONOROUS SENSATION"

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In 1874 Mayer published an account of a short series of experiments in which he attempted to measure the duration of what we would today call the positive after-image of tone.[1] In these experiments he served both as experimenter and observer. The following year he repeated the experiments using two observers whom he considered to be more competent than himself for this work because they were musicians.[2] He waited, so he says, for 19 years in the hope that other experimenters would repeat his work. Finally, in 1894 he ceased waiting and extended his researches himself.[3] In this latter series the extension was in two directions: he introduced variations of his original method and he employed more tones in his series of stimuli. In all the experiments of the three periods he relied upon the same fundamental devices. The first was a source of tone which was conducted to a specially constructed disk resembling a siren. This disk served as interrupter of the tone. The second was another conduction tube which picked up the interrupted tone and delivered it to the ear of the observer. The minor variations of this fundamental technique did not materially alter the results. For this reason it is not necessary here to do more than indicate the general method followed by Mayer.

For sources of tone he used tuning-forks with resonators. The resonator was mounted beside the fork with the nipple of the resonator extending laterally toward the siren disk. The siren was brought very close to the resonator, thus imprisoning the tone as much as possible within the conduction system. On the side of the siren opposite to the mouth of the resonator the conduction tube was led away to the ear of the *O* from a point as near to the siren as possible. When the hole in the siren lay in the path of the tone, conduction was unobstructed and the tone arrived at the ear of the *O* at full intensity. When the solid wood of the siren, a moment later, moved into the path, the tone was

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[1] A. M. Mayer, The determination of the law connecting the pitch of a sound with the duration of its residual sensation, *Amer. J. Sci. & Arts*, 8, 1874, 241-255.

[2] Mayer, A redetermination of the constants of the law connecting the pitch of a sound with the duration of its residual sensation, *ibid.*, 9, 1875, 267-269.

[3] Mayer, The law connecting the pitch of a sound with the duration of its residual sensation, *ibid.*, 47, 1894, 1-28.

cut off greatly. Mayer speaks of the obstruction as if it were entirely impenetrable—except in one passage in which he speaks with more exactness.[4] Here he remarks that the tone was not completely blocked but was only greatly weakened in intensity. In the individual experiments the rate of rotation of the siren was gradually increased from the initial rate, at which the tone clearly fluttered at the separate interruptions, to the critical rate at which the tone had just lost its last trace of flutter and had become smooth and continuous. At this stage the "residual" experience, as Mayer called it, was just long enough in duration to fill in the very brief interval of approximate silence between stimulations without loss of sensible intensity. From the determined rate of rotation and the known space between holes in the siren, he calculated the actual duration of the auditory after-image. His interest in the physics of the phenomenon led him to seek a formula by means of which the duration of the after-image for any tone might be calculated. This he achieved, with doubtful validity, under the conditions of his experiments.

Repetition of Mayer's Experiments

Procedure. In an effort to interpret Mayer's results in the light of our own experiments in which we found no auditory after-image,[5] we repeated his experiments both as he did them and with refinements of his technique. We duplicated his siren which was made of wood and cardboard fastened together in a two-ply disk. The holes were 1 cm. in diameter and the interspaces between holes 2 cm. The diameter of the nipple of the resonator was 0.5 cm. and the diameter of the conduction tube was 1 cm.—dimensions stipulated by Mayer. Unlike him we had only one siren which ran at different rates as a substitute for a set of sirens with varying numbers of holes. He rotated his siren by hand by means of pulleys and a belt. In all experiments the rotation of the hand was at a constant and identical rate. The rate of interruption was determined by varying either the number of holes in the siren or the diameter of the pulleys. We rotated our siren by an Edison electric phonograph motor whose speed could be increased or decreased at will and whose rate at any speed of rotation was presumably no less constant than Mayer's manual rotation. We succeeded in bringing the nipple of the resonator and the end of the conduction tube very close to their respective faces of the siren but we were unable to equal Mayer's precision. Our disk was very slightly warped and the consequence of this was that during rotation it varied from a position of just not touching the tube or resonator to a maximum separation from either of approximately 0.5 mm. Mayer's more perfect siren, he says, was kept at 0.3 mm. from the resonator. Upon investigation we found that the tone was not perceptibly louder on account of the additional orifice of 0.2 mm. Indeed, at full intensity, the sound penetrated the siren disk itself when both nipple and conduction tube were held tightly against it. For this reason we feel that the slight difference in precision between the two experimental arrangements is negligible. Mayer and his *O*s were in the same room with the instruments. On account of the escape of tone from the conduction system, we felt that it

[4] *Op. cit.*, 47, 1894, 11.

[5] H. G. Bishop, An experimental investigation of the positive after-image in audition, this Journal, 32, 1921, 305-325.

was necessary to correct this inaccuracy in technique which Mayer disregarded. This was achieved by passing the conduction tube through a heavy stone wall so that the *O* was in one room and the apparatus in another. On account of their convenience of operation, we substituted Stern variators for forks. In other respects than those noted our conditions were like those of Mayer.

We referred above to the inexactness of Mayer's statement about the degree of intensity of the auditory experience which was perceptible during the moment when the siren obstructed the sound. Since he referred to this experience as if it were one of silence while in reality it was one of diminished tone, it seemed necessary to correct this additional error in technique. We, accordingly, reduced the intensity of the tone to the point at which there was complete silence when the siren blocked the conduction tube. Under these conditions, the tone was never strong but it was clearly audible when the hole was before the resonator with the siren at rest.

At slow speeds of rotation the character of the intermittent tone was similar to the interference phenomenon of beats. With increasing rate of interruption and with correspondingly increasing rate of beating, there arose an undertone of noise which finally blotted out the beating completely. So long, however, as any trace of the tone could be heard it was still beating. Long before we attained the number of interruptions which Mayer found to be necessary to give a smooth tone, this accessory noise obliterated the tone. Such results, failing to verify Mayer's claim that the tone became continuous, force one to the conclusion that his experiments emphasize not the alternation of sound and silence but rather the combined effect of the auditory experience of the moment consisting of the interrupted sound which came through the siren, the diminished intensity of sound which came directly to the *O*s from the instruments, and any additional sounds generated by the siren itself. One must conclude, furthermore, that Mayer did not measure the length of tonal after-image as he had intended but made his judgments upon the basis of a complicated auditory situation. He was not unaware of this as the following taken from the article of 1875 shows, "I will now describe the character of the successive sensations experienced when starting from rest we gradually increase the velocity of rotation . . . until the separate beats blend into a smooth, continuous sensation. When the disk is stationary, with one of the openings opposite the mouth of the resonator, . . . the ear will experience a simple sonorous sensation."[6] At the slower rotation, he goes on to say, two additional tones appear, one slightly above and one slightly below the beating tone. At increasing rates of rotation these tones diverge finally reaching a separation of the major sixth. "At the same moment, a resultant sound appears, formed by the union of the sound of the fork with the upper and lower of the secondary tones. This resultant is the second octave below the note given by the fork. On further increasing the velocity of the disk, the two secondary sounds and the resultant disappear, and the ear has alone the sensation of the simple sound produced by the beats of the fork which, at this stage of the experiment, blend into a smooth continuous sensation."

[6] *Op. cit.*, 268 f.

It was obvious, in the face of the conflict between what we found with the *Os* and the instruments in separate rooms and such a report as this, that we must repeat Mayer's experiments as he did them with instruments and *Os* in the same room, if we would duplicate his results. We, accordingly, arranged the apparatus to suit his conditions, continuing to use the variator as a source of tone.[7] As an aid to the *Os* (Misses Doris Dewey and Florence Manson, seniors, with psychological and musical training, and the writer), we placed an harmonium at hand by reference to which an *O* could check the pitch of any component tone which he detected at any time in the complex of sound. We began the experiment with the siren rotating at a somewhat slower rate and ended it with a somewhat more rapid rate of rotation than was stipulated by Mayer. During the stimulation the *Os* listened to the full complex as well as to the character of the stimulus-tone itself.

Results. We were able to verify the findings of Mayer to some extent. With him, we found that the tonal experience was beating in character and that it was complex. The primary tone lost its roughness and, at about the rate of interruption reported by Mayer, was smooth and continuous in character. We did not agree with him, however, that it was the only tone present at the incidence of smoothness. We could hear secondary tones in the complex when the primary had become smooth. In order to deal more fully with this disagreement it will be necessary to examine the physical and musical aspects of the whole auditory experience from the moment the siren begins to revolve until the end of the observation when the primary has become smooth.

The physical aspect is stated explicitly by Rayleigh.[8] He shows that we may expect to find, as components of the complex, the primary tone itself and any or all of four secondary tones which he designates as follows: if n represents the frequency of vibration of the primary tone and m the rate of interruption of the primary tone, then the complex will be composed, under the right conditions, of the primary, n, and the four secondary tones $n+m$, $n+2m$, $n-m$, and $n-2m$. As the rate of interruption increases, it is obvious that the primary tone will remain constant in pitch and that two of the secondaries will rise and two will fall in pitch. The point at which this progressive change will be broken off during the experiment will depend upon the auditory and musical character of the whole complex which has been attained at the moment the *O* decides that the primary is no longer interrupted.

[7]These experiments were conducted in the Psychological Laboratory of Cornell University.

[8]Lord Rayleigh, Acoustical observations, *Phil. Mag.* 9, 1880, 278-283.

If Mayer is correct, the *O* will judge the proper moment by the sole criterion of smoothness of the primary. If we are correct, smoothness will occur because of the musical character of the whole auditory experience at the critical moment. In other words, the judgment of smoothness is a matter of ultimate consonance, an ultimate consonance which was preceded by an earlier dissonance, during the single observation. A fuller discussion will make this cryptic proposition more clear.

During a single observation the auditory experience will advance from the initial, smooth note, *n*, to tonal complexes marked at first by very slow beats but subsequently by beats which increase in rate as the pitch of the secondary tones changes. Stated musically, the change will pass from a single tone at first through the progressively widening intervals of subminor second, minor second, major second to approximately harmonic or fully harmonic intervals, unless the observation happens to be broken off along the way. In the nature of the case the observation would not be broken off while the primary tone was heard to beat. If all four of the secondary tones were to materialize in the complex, one would have a variegated and constantly changing tonal experience, while the experiment was in progress. Quite obviously the musical unitariness of the whole would follow the laws of tonal fusion, consonance, and dissonance. Those combinations of tone which were relatively free from beats and which were, therefore, harmonious would be those combinations at which one would naturally leave off the observation because the beating had, for the first time, ceased. The hitherto kaleidoscopic change in auditory experience would, at the moment, have the appearance of coming to rest. Although the beating due to the interruptions and the well known beats of interference are unlike physically, they are similar as auditory experiences. We know that under the proper conditions the beats will attach to one or the other or both of the generators and, in such a case, even when produced by uninterrupted and continuously sounding generators, the generators will, one or both, be heard to beat. Since the beats will change in rate as the pitch of the continuously rising upper secondaries and the continuously falling lower secondaries changes, they will eventually become so rapid that they will fail to mask the secondary tones. When this occurs, the changing pitch of the secondaries will make them easy to detect, while the diminishing beats will be

relatively less conspicuous. At the proper moment, which will soon arrive, the full consonance or the approximately full consonance within the whole auditory complex will be judged as a smooth primary, in Mayer's type of observation, and in our type will be perceived as a smooth primary plus whatever consonant secondaries could be detected. The truly composite clang of a sounding string which is regularly perceived as a tonal unity supports this interpretation of the observation of the sounding complex. In these experiments in which the primary is reënforced to a considerable extent by transmission through the siren at all times, it is only natural that it should seem even more unitary than a clang. Since these observations must, we believe, be interpreted musically, we shall now examine them as musical experiences.

TABLE I

PRIMARY TONES AND CALCULATED SECONDARY TONES

m	n	$n-2m$	$n-m$	$n+m$	$n+2m$
36	128 c	56(64)	92	164(160)	200(192)
62	256 c′	132	194(192)	318(320)	380(384)
73	320 e′	174	247(256)	393(384)	466
88	384 g′	210	296	472(480)	560(576)
108	512 c″	296	404(409)	620(640)	728
126	640 e″	388	514(512)	766(768)	892
143	768 g″	482	625(614)	911(910)	1054
170	1024 c‴	684(682)	854(853)	1194	1364

TABLE II

MUSICAL NOTES HEARD IN THE TONAL COMPLEX

c	C c e g
c′	g c′ (e′ flat, 307-e′) g′
e′	c′ e′ g′
g′	g′ (b′ flat, 455-b′) d″
c″	(a′ flat- a′, 426) c″ (e″ flat, 614-e″)
e″	c″ e″ g″
g″	(e″ flat-e″, 640) g″ b″ flat
c‴	f″ a″ c‴

In Table I will be found the primary tones with their musical designation and their four secondaries calculated by the formulae given above. We have placed numbers in parentheses beside the calculated values of certain secondaries. These are the frequencies of vibration of the notes actually heard in the complex and identi-

fied by reference to the harmonium. The general agreement between calculated value and corresponding musical note is as close as might be expected.

Table II contains the musical names for the frequency values of all the tones which we heard in the complex as they are given in Table I. The several notes placed in parentheses are border cases and were judged as either the one or the other, but never both. The number in the parentheses is the frequency of the note omitted in Table I.

Examination of these results shows that never were all the secondaries detected. We discovered, in this connection, what Mayer did not report, namely, that the intensity of the secondary tone was so faint sometimes that it was lost at any but the most favorable position of the conduction tube in the ears. If we had searched for them all we might, perhaps, have found them. That, however, was not our problem.

In our interpretation of these results, it is their significance as musical intervals which is important. They cannot be unequivocally offered as measures of "residual" auditory sensation. In order, therefore, to render them more significant as musical phenomena we have assumed certain informative values of m and have computed secondary tones which theoretically might have manifested themselves in the complex. If m is assumed to be $n/20$, m will then be sufficiently small to produce values of the secondaries which will be rather near to one another and also to n itself. This assumption is significant because these values, when stated as musical intervals, should be a set of dissonant intervals. We find such to be the case. Expressing the intervals as decimals based upon any convenient value of n such as 20, we have the values of Table III. The first column shows the secondaries concerned. The second column shows the quotient derived from the ratio of the secondaries in the first column. The third column gives the name of the musical interval to which the interval of the secondaries in the same line of the first column most nearly corresponds. The fourth column gives the exact value of the interval named in the same line of the third column. For example, if $n=20$ and $m=n/20$, then $m=1$ and $n+2m=22$. It follows that $n:n+2m=1:22/20$ which is 1.1. In other words, if n has the value of 1, $n+2m$ will have the value of 1.1. The excellent agreement between the second and fourth columns shows how nearly

the values of m and n, as assumed, and calculated according to the combinations of the first column of the table yield standard musical intervals.

TABLE III

RATIOS SHOWING DISSONANT TONAL COMPLEX

n+m:n+2m	1.047+	Minor Second	1.066+
n:n+2m	1.100	Minor Second	1.066+
n−m:n+2m	1.157+	Acute Major Second	1.125
n−2m:n+2m	1.222+	Minor Third	1.200
n:n+m	1.050	Minor Second	1.066+
n−m:n+m	1.105+	Grave Major Second	1.111+
n−2m:n+m	1.166+	Augmented Second	1.171+
n−m:n	1.052+	Minor Second	1.066+
n−2m:n	1.111+	Grave Major Second	1.111+
n−2m:n−m	1.055+	Minor Second	1.066+

Table III demonstrates that relatively small values of m, *i.e.* a low rate of interruption must inevitably be discordant. The only consonant interval shown is the minor Third. In the nature of the case, however, it may not occur alone since the primary will always be present. This being true, no possible combination of notes at $m=n/20$ can be consonant. Acording to our argument, therefore, Mayer's observations could not end here. Greater rapidity of interruption than $m=n/20$ must be resorted to if one is to arrive at consonance.

We were guided in our choice of intervals, which should either be fully consonant or more consonant than those in Table III, by Mayer's results as they are stated in Table IV. In the first line are the values of n, in the second line the values of m, and in the third, the values of m/n expressed reciprocally. Following the

TABLE IV

MAYER'S RESULTS EXPRESSED RECIPROCALLY

128	256	320	384	512	640	768	1024
36	62	73	88	108	126	143	170
1/3.5	1/4.1	1/4.4	1/4.3	1/4.7	1/5.0	1/5.3	1/6.0

implicit suggestion of these values in Table IV, we calculated other tables, below, using successively, $m=n/10$, $n/6$, $n/5$, and $n/4$. The values of $n/6$, $n/5$, and $n/4$ are suggested directly by Table IV. We chose $n/10$ because it is intermediate between $n/20$ which is too low and $n/6$ which is too high for the threshold of consonance. At $n/10$ we hoped to find either barely consonant

intervals or a few consonant and a majority of dissonant intervals, as a demonstration of the threshold of consonance at which, or just beyond which, the observation in a single experiment might end. Inspection of Table V, which shows the array of intervals at this value of *m*, reveals the fact that we are here close to or at the threshold we sought. Except for the Fifth which is perfectly consonant (but it could not exist alone) and the minor Second which is fully dissonant, the intervals are about as much consonant as dissonant. Mayer could, we believe, not quite break off an observation at $n/10$. Translating Table V into intervals in the key of C natural we would have *A*-flat, *B* (modified), *C*, *D* (modified), *E*-flat. The observation, obviously, could not end if all the notes were heard but it could end if the *B* and *D* were absent.

TABLE V

Approximate Consonance-Dissonance Threshold

n+m:n+2m	1.090+	Grave Major Second	1.111+
n:n+2m	1.200	Minor Third	1.200
n−m:n+2m	1.333+	Fourth	1.333+
n−2m:n+2m	1.500	Fifth	1.500
n:n+m	1.099+	Grave Major Second	1.111+
n−m:n+m	1.222+	Minor Third	1.200
n−2m:n+m	1.375	Fourth	1.333+
n−m:n	1.111+	Grave Major Second	1.111+
n−2m:n	1.250	Major Third	1.250
n−2m:n−m	1.125	Acute Major Second	1.125

When *m* has become relatively greater, reaching the magnitude of $n/6$ (Table VI), $n/5$ (Table VII), and $n/4$ (Table VIII), the musical character of the intervals is such that dissonance would be improbable. Differences between these tables will be discussed presently.

Table VI indicates that considerable consonance could be found either if the notes heard were consonant, of themselves, at all regions of the scale, or if the notes happened to be consonant at the particular region in which the primary note was actually produced. The musical effect would be a matter of the notes composing the chord and their absolute pitch. The chord indicated by Table VI is, f'', a'', c''', d''' (modified), f''', since n is 1024 and m is 170 or $n/6$. We found the first three notes of this chord (Table II) and Mayer, presumably, might have had them. If he did, the smoothness of the consonance would not allow beats at

the region of 1024. Its musical character at other regions, such as at c of 128, is not one of consonance but of dissonance. In this region consonance can be found only with greater intervals between the notes in the chord. In terms of interruptions, m must be more than $n/6$ when n is of sufficiently low pitch. Table IV shows that Mayer's experimental value for 128 meets this requirement.

TABLE VI

INTERVALS CONSONANT AT HIGH PITCH

n+m:n+2m	1.142+	Diminished Third	1.137+
n:n+2m	1.333+	Fourth	1.333+
n−m:n+2m	1.600	Minor Sixth	1.600
n−2m:n+2m	2.000	Octave	2.000
n:n+m	1.166+	Augmented Second	1.171+
n−m:n+m	1.400	Diminished Fifth	1.400
n−2m:n+m	1.750	Minor Seventh	1.777+
n−m:n	1.200	Minor Third	1.200
n−2m:n	1.500	Fifth	1.500
n−2m:n−m	1.250	Major Third	1.250

What is true regarding interval and absolute pitch on the one hand and consonance and relative value of m on the other in Table VI is also true in Tables VII and VIII.

TABLE VII

INTERVALS CONSONANT AT INTERMEDIATE PITCH

n+m:n+2m	1.166+	Augmented Second	1.171+
n:n+2m	1.400	Fifth	1.500
n−m:n+2m	1.750	Minor Seventh	1.777+
n−2m:n+2m	2.333+	Minor Tenth	2.400
n:n+m	1.200	Minor Third	1.200
n−m:n+m	1.500	Fifth	1.500
n−2m:n+m	2.000	Octave	2.000
n−m:n	1.250	Major Third	1.250
n−2m:n	1.666+	Major Sixth	1.666+
n−2m:n−m	1.333+	Fourth	1.333+

The chord from Table VII applies almost equally well, since it is based on $m=n/5$, to the three primaries, c'' of 512, e'' of 640, and g'' of 768. The full chord for c'', constructed by means of the values of $n-2m$, $n-m$, n, $n+m$, $n+2m$, is e'-flat, a'-flat, c'', e''-flat, and g''; for e'' it is g', c'', e'', g'', and b''-flat; and for g'' it is b'-flat, e''-flat, g'', b''-flat, and d'''. Table II shows that we found the three notes in the middle of these three chords at Mayer's

stipulated number of interruptions. Their full consonance in the region in which they were used in the experiments and the dissonance of the same intervals lower in the scale support our contention that Mayer may well have judged only consonance.

TABLE VIII

INTERVALS CONSONANT AT LOW PITCH

n+m:n+2m	1.2	Minor Third	1.2
n:n+2m	1.5	Fifth	1.5
n−m:n+2m	2.0	Octave	2.0
n−2m:n+2m	3.0	Twelfth	3.0
n:n+m	1.2+	Major Third	1.2+
n−m:n+m	1.6+	Major Sixth	1.6+
n−2m:n+m	2.5	Major Tenth	2.5
n−m:n	1.3+	Fourth	1.3+
n−2m:n	2.0	Octave	2.0
n−2m:n−m	1.5	Fifth	1.5

The chord from Table VIII ought to apply rather well to the primaries *c* of 128, *c′* of 256, *e′* of 320, and *g′* of 384, on account of the fact that Mayer's values for these tones are approximately $n/4$. Constructed in the same manner as the chords from Table VIII, the full chord for c is *C*, *G*, *c*, *e*, and *g*; for *c′* it is *c*, *g*, *c′*, *e′*, and *g′*; for *e′* it is *e*, *b*, *e′*, *a′*-flat, and *b′*; and for *g′* it is *g*, *d*, *g′*, *b′*, and *d″*. These chords, except for *e′*, contain the notes for the same primaries as they are given in Table II. The abridgment is only slight in the several cases. The note *G* was not heard in the first chord, the note *c* in the second, and the notes *g* and *d* in the fourth. The result for the primary *e′*, the exceptional case, when calculated according to the intervals in Table VII instead of Table VIII is *g*, *c′*, *e′*, *g′*, and *b′*. It is interesting that the only inversion in Table IV should fall at *e′*, 320. The chord for *e′*, reported in Table II, is *c′*, *e′*, *g′*, the middle notes from the chord given above. This chord from Table VII happens to be more appropriate for *e′* than the one from Table VIII. This might have been expected since $m=n/4.4$ is almost as near to the $n/5$ of Table VII as it is to the $n/4$ of Table VIII. The harmonic difference, however, between *b*, *e′*, *a′*-flat and *c′*, *e′*, *g′* is very slight. The former, in this region of the scale, seems to be slightly more consonant than the latter, as it might well be, since it is an interval of nine semitones, whereas the interval from *c′* to *g′* contains only seven semitones. This sole and rather slight discrepancy between the calculated and

the experimentally determined values hardly vitiates our argument that Mayer could have been judging consonance unwittingly. This is especially true on account of the fact that we are using nothing better than approximate values in our calculations.

In conclusion, our argument is simple. Mayer's results, when interpreted musically are a matter of consonance and dissonance. Although absolute rate of interruption varied directly with rate of vibration of the stimulus tones, the relative rate of interruption varied inversely with it. The relatively slow interruptions were sufficiently rapid, in the higher pitches, to produce consonance but could not produce intervals sufficiently great for consonance in the lower pitches. It happens that Mayer found rates suitable for consonance at all the points of the scale from which he chose his stimulus tones. Indeed, the chord produced was the common chord (C, E, G), with some extensions above and below, or a very close approximation to it. The same type of musical perception could, therefore, be employed in all judgments in his experimental series, and we assume that this was the way in which the judgments were actually made. The apparently different rates of interruption are, musically, really identical, if we consider both harmony for itself and the absolute pitch of the chord. Every single experiment in Mayer's whole series of experiments ended when consonance first appeared, and in our experience this stage, for audition, was very much like a single, smooth primary but was not identical with it. Physically, the secondaries ought to have been present; actually, we found some of them but Mayer did not. Unfortunately, it is one set of observations against another, hardly a determinate situation for science.

Summary

(1) Mayer's conditions were not precisely as he reported them. His stimulus-tone was not interrupted by intervals of silence, as he thought, but by changes in intensity.

(2) When we reduced the intensity of our tone, we found that we were unable to obtain an alternation of sound and silence. The tone disappeared in noise before Mayer's rate of interruption was attained.

(3) We found, under Mayer's conditions, that the stimulus-tone was smooth as he reported at his rates of interruption, but

that there were other and new tones present wh
obtained that were not reported by him.

(4) The new tones formed consonant cho
interruption that Mayer used.

(5) The unity of the clang accounts, we h
failure to note the new tones.

(6) If Mayer heard the new tones withou
his results can readily be explained as the
smoothness of consonance.

PHYSICAL ASYMMETRIES AND DISORIENTATION

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Despite the interest and attention which for centuries have been given to the problem of orientation, a good deal of vagueness still surrounds the major issues. Mystic capacities are still invoked to explain the more complex and less certain features of the process. Writers on the subject are not content merely to speak of adjustments to objects and relations in time and space, but are inclined to feel that, over and above these specific adjustments, there is a more general capacity or orientation sense which controls the individual adjustments. The hope still lingers that we may find an internal supra-sensory faculty with special powers of direction and control. Of such a power we have no direct experience. This being the case, it cannot devolve upon us to prove or disprove its existence, and we should be free to work on the more natural assumption that the adjustments in question depend upon cues registered by the ordinary senses.

Most of the investigation in the past has been concerned with the invertebrates and, more especially, the insects. Very little work has been done on the higher orders, and hardly any in the case of man. The greater perfection of the capacity in the lower forms and the greater opportunity for research have, no doubt, been of chief importance in determining the direction which interest in the problem has taken.

In the case of man the best work has been done on the labyrinthine and equilibrating mechanisms. Here, as in many other instances, the most valuable and illuminating data have been derived from pathological studies and from direct observation of the effects of eliminating the supposedly significant factor. Following this suggestion, the present study was concerned with a series of experiments in which the visual sense, generally recognized as most important for orientation in man, was eliminated as a contributing factor.

Everyone must have vivid impressions of the disturbing effects of blindfolding in such games as blind-man's buff, and the state

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of complete disorientation which develops in the course of the game. The all-important rôle of the visual sense has been emphasized also by recent experiments in aviation. Magnus states that "the exclusion of visual impressions when the aeroplane is passing through mist or clouds makes orientation practically impossible. Often when the aviator emerges from the clouds and can once more see the ground, he finds himself completely disoriented with respect to the earth."[1]

In the experiments reported in this article blindfolded *S*s were required to walk given distances under prescribed conditions. Elimination of the visual sense produced varying amounts of disorientation, expressing itself in a disposition to veer from the desired or expected line of movement. In following up these results interest naturally centered about an attempt to account for the nature of the veering. While the data thus secured may have only indirect bearing upon the problem of orientation, it is hoped, nevertheless, that they may throw some light on the process and possibly indicate the direction which the solution of the larger issues ultimately will take.

Method and Procedure

Experiment 1: Walking straight-ahead. A football field presenting a fairly even and uniform surface of green turf was selected for the experiment. With the aid of a surveyor the field was mapped out as shown in Fig. 1. Small cardboard signs were placed at the points indicated in the diagram. These signs, marking the degrees of deviation from the median line, enabled *E* to plot the course of *S* with a fair amount of accuracy. One hundred twenty-five *S*s, students of psychology, participated in the tests. Before starting, *S* was brought into position at 'O' (see Fig. 1) and asked to orient himself with respect to 'P,' his objective in walking. Upon being blindfolded he would start immediately for 'P' guided from behind for the first few steps in order to insure correct initial orientation. He was permitted to set his own pace. *E*, following close behind, plotted the course of *S* on a specially prepared chart. The limit of the field reached, *S* was requested to make several pivotal turns before being led back to 'O' to repeat the performance. The pivotal turns served to eliminate any possible cues as to the nature of the veering. Six such records were secured for each *S*.

Experiment 2: Walking backward. The bee-line which the *S* attempted to make in walking from 'O' to the limit of the field usually turned out to be a more or less uniform curve to the right or to the left. In view of this, and in view of the consistency characterizing successive performances of the same *S*, it was considered desirable to secure a check on the nature of the deflectional

[1]Rudolph Magnus, Physiology of posture, *Lancet*, 2, 1926, 588.

tendency by having *S* walk backward. If a structural factor were responsible for the deflection, then the disposition to veer should be reversed when *S* walks backward, since this change in position reverses the plane of the body relative to the objective. Except for these variations the procedure was the same as in the first experiment. Seventy-five *S*s who had shown greatest consistency in right-veering and left-veering were selected for this experiment.

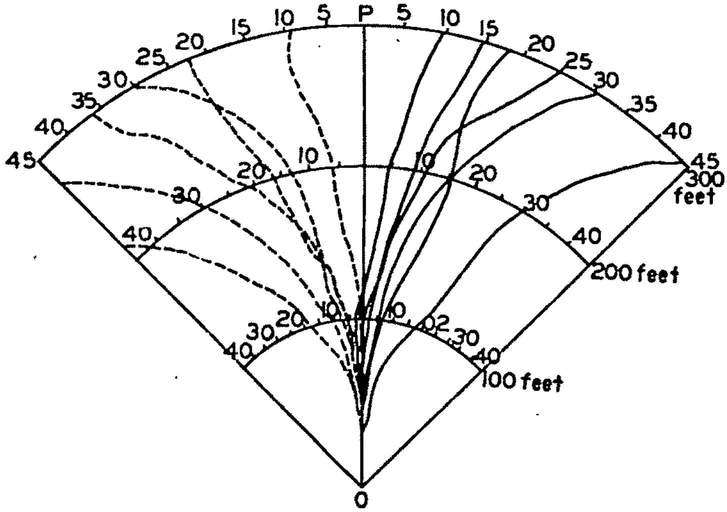

FIG. 1. DIAGRAM OF THE EXPERIMENTAL FIELD
The full lines to the right (walking forward) and the dotted lines to the left (walking backward) indicate the performance of a left-dominant *S* in *Experiments 1* and *2*.

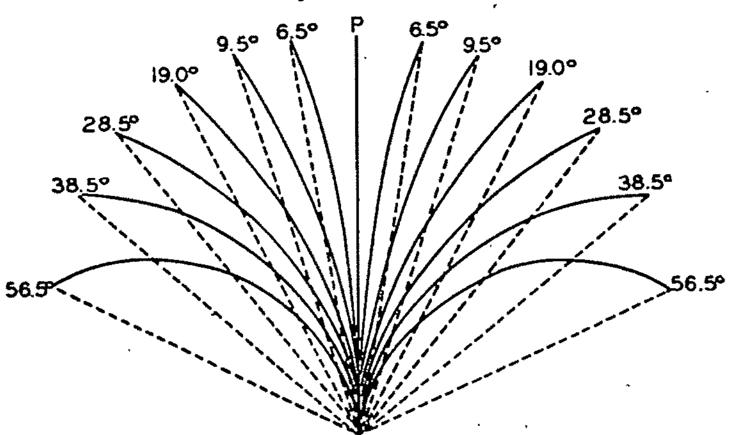

FIG. 2. DIAGRAM OF THE ARCS OVER WHICH THE *S*S WERE LED
The angular deviation of each arc from the line 'O-P' is indicated at the extremity of each arc.

Experiment 3: Walking over prescribed arcs. On the same field 200-foot arcs tangent to the same line were described by radii of 100, 150, 200, 300, 600, and 900 ft. respectively. Six such arcs were drawn to the right and six to the left. Fig. 2 is a diagram of the field and shows the amount of deflection of each arc from the line 'O-P.' Tracing these arcs in liquid lime made them available as courses over which the *S* might be led. Throughout this experiment a mechanical device was interposed between the *E* and the *S* so as to keep the latter on the course and at an appropriate distance from the *E*. *S* was told that he would be led over these arcs and that his problem would be to judge whether he was being taken to the right, the left, or straight-ahead. Starting with the outer arcs and alternating with the straight path, *E* was able gradually to eliminate those arcs in the case of which the deflectional tendency was easily detected; and finally, by a process of elimination was able to determine the approximate norm of deflection of a given *S*. This experiment served as a further check upon the functional dispositions displayed in *Experiments 1* and *2*.

Measurement of Bilateral Symmetry

A preliminary survey of the results showed not only a certain amount of consistency for most *S*s in successive performances in a given experiment, but equal consistency as between experiments. Inasmuch as the most likely explanation of the presence or absence of these functional asymmetries was to be found in some significant structural asymmetry, measurement of all *S*s engaging in the three experiments was undertaken in the following characteristics:

(*1*) *Handedness.* Depending upon dextral usage, the *S*s were classified as dextral, sinistral, or ambidexterous, or, to use the expressions later introduced for the sake of having a common basis for comparison, as *right-dominant, left-dominant*, or *non-dominant.*

(*2*) *Eyedness.* Since some authorities consider eyedness a more significant measure of functional unilateral dominance, in fact, maintain that handedness depends upon eyedness, the Parsons' manuscopic test was used to supplement the measure of handedness.

(*3*) *Length of arms.* The measurement of the length of the *S*s' arms was undertaken because of the relation known to exist between asymmetry in this instance and handedness.

(*4*) *Length of legs.* A measure of the relative length of the two legs was first secured by measuring the distance from the anterior-superior spine to the floor for each leg, *S* meanwhile resting his entire weight on the limb being measured. The uncertainty in some of the point on the ilium from which the measurement was taken made it desirable to secure a check by a different method. The *S* was placed outstretched on a table. A straight position was secured by bringing the feet together in such a way that their juncture was in a line with the median line of the trunk. Two square blocks of wood were then forced with equal pressure against the soles of the feet. The difference in the position of the blocks was taken as a measure of the difference in the length of the legs.

(*5*) *Posture.* It has long been known that the right side of the body may be as different from the left as 'identical' twins may be different. They are, in fact, only imperfect twins. It was believed that superior structural development of a given side would tend to make the individual incline slightly to the

other. In measuring for posture the *S* was asked to bring his heels together and otherwise to assume a natural posture. A perpendicular was then erected behind his back from the point where the heels were in contact. Leaning to the right was interpreted as left-dominance, leaning to the left as right-dominance.

(6) *Walking straight-ahead.* This was the first of the experiments described above. Disposition to veer to a given side was taken to mean dominance of the opposite side.

(7) *Walking backward.* Since the position of the limbs relative to the line of movement is reversed in this experiment, right veering would have to mean right-dominance, left veering left-dominance.

(8) *Walking over arcs.* Predominance of errors on right arcs (see *Experiment 3*) was interpreted as left-dominance, predominance of errors on left arcs as right-dominance.

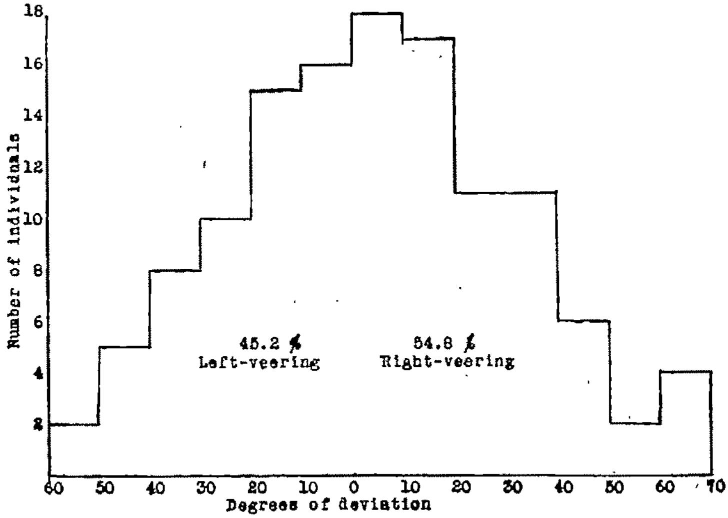

FIG. 3. CHART SHOWING RESULTS OF EXPERIMENTS 1, 2, AND 3
Distribution of 125 *Ss* according to amount of deviation in six successive walking performances while blindfolded.

RESULTS

The results are based upon 3542 trials, or walking performances, of 125 *Ss*, and upon the data derived from measurement of the structural and functional traits enumerated in the previous section. Upon completion of the first experiment every *S*'s average deflection from the median line was computed from his chart, the terminal point of each performance being made the basis for compution. The results of these computations are shown in Fig. 3. As will be seen, more *Ss* veered to the right than to the left. This greater frequency of left-dominance indicated by more frequent right-veering is paralleled by more frequent structural dominance of the left leg, as later measurement showed 39% of

left-dominance, 33% of right-dominance, and 28% of equality. On the other hand, measurement of the arms of the same *Ss* showed 55% of right-dominance, 31% of left-dominance, and 14% of equality. This fact of 'crossed symmetry' (longer arm on one side coupled with longer leg on the other) has been reported by Hasse and Dehner,[2] Guldberg,[3] and others.

Before proceeding to the second experiment the *Ss* were divided into three groups according to the dominant trend of their performance in the first experiment: Group I, 41 *Ss*, right-veering; Group II, 34 *Ss*, left-veering; Group III, 50 *Ss*, in general, forward-moving. Measurement of these groups yielded the following results:

Group I: 78% longer left leg; left leg on the average 0.42 cm. (median 0.52 cm.) longer than the right, with an A.D. of 0.17 cm.

Group II: 83% longer right leg; right leg on the average 0.61 cm. (median 0.63 cm.) longer than the left, with an A.D. of 0.20 cm.

Group III: 65% equality of right and left leg; median gave equality; average favored the left leg by 0.07 cm. with an A.D. of 0.06 cm.

These figures show a striking correspondence between structural and functional dominance. The results of *Experiments 2* and *3* yielded only further confirmation of this relationship, the correspondence being slightly greater than in *Experiment 1*. Groups I and II alone served as *Ss* in the second and third experiments. A complete summary of the results for all three experiments follows.

Walking straight-ahead. Of the *Ss* veering left, indicating *right-dominance*,
- 83% had longer right leg, exceeding the left on the average by 0.61 cm., with an A.D. of 0.20 cm. (0.63 cm. median measure of the same excess);
- 77% veered right in walking backward;
- 83% showed excess errors on left arcs;
- 75% showed right-dominant posture;
- 92% were right-handed;
- 80% were right-eyed.

Of the *Ss* veering right, indicating *left-dominance*,
- 78% had longer left leg, exceeding the right on the av. by 0.42 cm. with an A.D. of 0.17 cm. (0.52 cm. median measure of the same excess);
- 80% veered left in walking backward;

[2] C. Hasse and M. Dehner, Unsere Truppen in körperliche Beziehung, *Arch. f. Anat. u. Entwicklungsgesch.*, 16, 1893, 249-256.

[3] G. A. Guldberg, Über die morphologische und functionelle Asymmetrie der Gliedmassen beim Menschen und bei den höheren Vertebraten, *Biol. Centralbl.*, 16, 1896, 806-813.

91% showed excess errors on right arcs;
73% showed left-dominant posture;
84% were right-handed;
53% were left-eyed.

Walking backward. Of the *Ss* veering right, indicating *right-dominance*,
85% had longer right leg;
77% veered left in walking 'straight-ahead;'
71% showed excess errors on left arcs;
69% showed right-dominant posture;
85% were right-handed;
77% were right-eyed.
Of the *Ss* veering left, indicating *left-dominance*,
79% had longer left leg;
80% veered right in walking 'straight-ahead;'
88% showed excess errors on right arcs;
84% showed left-dominant posture;
84% were right-handed;
62% were right-eyed.

Walking over arcs. Of the *Ss* showing excess errors on left arcs, indicating *right-dominance*,
83% veered left in walking 'straight-ahead;'
71% veered right in walking backwards;
67% had longer right leg;
78% showed right-dominant posture;
89% were right-handed;
89% were right-eyed.
Of the *Ss* showing excess errors on right arcs, indicating *left-dominance*,
91% veered right is walking 'straight-ahead;'
88% veered left in walking backward;
79% had longer left leg;
80% showed left-dominant posture;
91% were right-handed;
54% were left-eyed.

Interpretation of Results

The disposition to turn, veer, or 'circle' appears with a high degree of regularity in organisms both high and low in the scale of life. In some it is manifest at all times, while in others, mostly of the higher order, it appears only when the sensory cues normally guiding spatial adjustments have been eliminated. As a *functional asymmetry* this disposition to veer would seem most naturally to call for explanation in terms of some *structural asymmetry*. But, unfortunately, the naturalness or simplicity of an explanation does not always insure its acceptance. There are always those

who are specially alert to suggestions of a more intriguing nature, and who prefer to inject mystery into their account wherever possible. So it has been even in the phenomena under consideration. When confronted with the seemingly needless yet continuous spiralling of paramecia, or the circular course taken by game when hard pressed, or, again, a similar performance on the part of individuals when lost, certain scientists have been inclined to side-step the simpler and more natural morphological account and to introduce special agencies from the realm of the unobservable.

Asa Schaeffer, Professor of Zoölogy in Kansas University, who has made wide observations on veering tendencies in many species, is convinced that men and animals are possessed of a "spiralling mechanism" or a "steering mechanism that makes them turn in spirals when it gets control of a situation—a sort of 'sixth sense' that most people have never dreamed that they possess."[4] "No forward moving organism," says Schaeffer, "has yet been found that does not move in some form of spiral path when there are no orienting senses to guide it. . . This extraordinary unanimity of observations can hardly be interpreted in any other way than that it is a universal property of moving living matter."[5] Concerning this "spiralling mechanism" he finds "comparatively little if any evolution. . . . The same mechanism operates in man as operates in the amoeba." To avoid being challenged with an outright mysticism, he proceeds to localize his newly discovered agency: "So far as man is concerned the center of mechanism is, of course, in the brain." Possibly fearing that this might lead him into admitting some structural influence, which from the outset he has discarded as "erroneous" and "absurd," he qualifies his statement with the admission that "the effective mechanism may at any given time be located in a single cell in the human brain."

These statements are made by Schaeffer at the conclusion of his work on *Spiral Movement in Man*, which had been preceded by a study of similar movement in the case of lower forms. In his experiments on man he recorded the paths of blind-folded *Ss* while walking and while swimming. On the basis of his studies, he feels justified in concluding that "the spiral turns are not due to asymmetries of the legs or other parts of the locomotor organs"

[4]For a summary of Schaeffer's results see *Literary Digest*, Aug. 11, 1928.
[5]Asa Schaeffer, Spiral movement in man, *J. Morph. and Physiol.*, 45, 1928, 297 ff.

(pp. 349, 375, 376, 393). This categorical statement is presented in spite of the fact that no measurements were made, or, at least, recorded, and in spite of the fact that his results are suggestive of a structural interpretation. He finds, for instance, just as in our own experiment, that right-turning predominates in walking, and, again, that left-turning predominates in swimming. But more frequent right-turning in walking is correlated with the fact (apparently known to Schaeffer, but for some reason overlooked) that a majority of persons have the left leg longer than the right. Again, more frequent left-turning in swimming is correlated with more frequent longer and stronger right arms than left.

In view of these facts there is reason to believe that evidence of a structural influence would have been at hand had measurements been made. In any event, we can hardly regard our own results as indicative of anything but a high degree of interdependence of functional and structural asymmetries. That 80% of the *S*s should show correspondence between structural and functional dominance is hardly a chance matter; and what is more to the point, this correspondence was increased when consideration was limited to those *S*s who showed consistency in more than one experiment. Added corroborative evidence was found in the fact that degree of structural dominance is correlated with degree of functional dominance. This became apparent when the *S*s were grouped according to amount of structural dominance of a given side:

	Structural disparity in leg-length (cm.)	Median deflection in walking (deg.)
Group A	0–0.3	14
Group B	0.4–0.6	25
Group C	0.7–0.9	25
Group D	1.0–1.3	27

But what of the 20% who failed to show correspondence between structural and functional dominance? And what, indeed, of the fact that no S veered in precisely the same manner in successive trials? Since the structural traits must have been constant, the important variant or variants must be found in the sphere of function. The difficulty really is in the matter of control and in the matter of keeping conditions constant throughout the experiment. One cannot always be sure of the full coöperation of the *S*. But even the *S* who tries his best cannot be expected in an experiment of this kind to keep his mental set more than relatively con-

stant over so long a period of time. There are always sensory elements of internal and of external origin which necessarily vary from trial to trial and which consciously or unconsciously influence the mental set and change the conditions throughout the motorium. But the importance of these sensory elements and of the mental set controlling the spatial adjustments brings us back to the problem of orientation.

Disorientation

Most blindfolded *Ss* after walking 300 feet find themselves facing in a direction at variance with their expectations, and after walking 500 feet many are completely disoriented, the veering process having resulted in a reversal of the line of movement. If, indeed, the individual possessed a special sense of direction, what sort of a sense could this be to allow for such anomalies of behavior? As already indicated, we prefer to work on the assumption that no special sense is involved. There is usually more willingness to admit this in the case of human behavior than in the case of some of the lower orders. But even in the lower animals the best opinion is inclined to rule out the possibility of a special sense for which there is no known anatomical basis. Rabaud, an eminent authority in the field, has recently brought together the results of the best experimental work on the subject, including investigation of the more unusual performances of insects and of homing and migratory birds. By way of conclusion he remarks that "the facts based upon well-conducted experiments or observations constantly reduce the phenomena of orientation and of place recognition to processes of memory bringing into play the organs of the ordinary senses."[6]

As for the senses involved in our own spatial adjustments, the visual is clearly dominant, the auditory and kinaesthetic assuming lesser rôles. In orientation of position and posture Magnus[7] considers the tactile sense almost as important as the visual and kinaesthetic. According to this author, the importance of the cerebellum and the labyrinth for attitudinal and righting adjustments has been greatly exaggerated.

But allowing that these senses and the impressions registered by them are adequate to account for orientation, how shall we

[6]E. Rabaud, *How Animals Find Their Way About*, 1928, 1-123.
[7]Magnus, *op. cit.*, 588 f.

account for the relatively stable and definite mental content usually described as our 'sense of direction'? How account for the equally definite experience of 'being turned around' with its resultant shift in getting again orientated?

The writer secured the coöperation of 5 *S*s in an attempt to gain introspective data which might throw some light on this question. Introspective reports were called for immediately following the removal of the blind-fold in situations in which the surroundings and the line of movement were not those expected. Analysis of these reports indicated that two forms of awaremess were readily distinguishable by the *S*: awareness of the relation between bodily position and the environmental lay-out, and awareness of the relative position of objects within this lay-out. Taken together, these forms of awareness constitute a mental configuration or mental set described by the individual as his "sense of direction." This mental set is relatively constant, and it must be so in order that objects within the field may shift without disturbing the contextual lay-out of the field.

The experience of being 'turned around' results when sensory impressions are registered which cannot readily be fitted into the general scheme or which belong to a context different from the one in control. The momentary confusion occasioned by these unexpected impressions institutes a process of reorganization which results in an elaboration of a new mental set with the attendant feeling of being 'turned right' again. Such, at least, seems to be the only interpretation possible with the facts at present available.

Summary

Exclusion of the visual sense, which usually guides spatial adjustments, results in varying amounts of disorientation, expressing itself in a disposition to veer or in a disparity between actual and intended movement. The blindfolded *S* attempting to walk in a straight line typically veers to the right or to the left. The direction of the veering is hardly a chance matter, since the same *S* placed under the same conditions tends to veer in the same direction in successive trials. Consistent right-veering may be regarded as a case of functional dominance of the left side of the body, consistent left-veering as a case of functional dominance of the right side. We ascribe such (functional) unilateral dominance

to some structural unilateral dominance, and present the following facts in support of our interpretation.

(1) The consistency of right-veering or left-veering of the same *S* in successive trials,

(2) The reversal of the veering process when walking backward, in the case of four out of every 5 *S*s.

(3) The difficulty experienced by *S*s in distinguishing the direction of the veering when led over a course corresponding to the normal disposition to veer.

(4) The correspondence between functional and structural dominance (superior structural development of the right leg in left-veering subjects and *vice versa*) in four out of every 5 *S*s.

(5) The increase in correspondence between structural and functional dominance when only those *S*s are considered who showed consistency in all performances.

The individual's 'sense of direction' may best be conceived as a mental set or a contextual form of awareness pertaining to the relation between bodily position and the environmental lay-out. The experience of being 'turned around' results when sensory impressions are registered which are out of accord with existing mental context. This institutes a process of reorganization which results in a contextual shift and a feeling of being 'turned right' again.

A STUDY OF THE FACTORS WHICH CAUSE INDIVIDUAL DIFFERENCES IN THE SIZE OF THE FORM-FIELD

By C. E. Ferree, G. Rand, and M. M. Monroe[1]

In recent years papers have been published by the senior authors showing the factors which influence the extent and shape of the form and color fields in a given individual, and describing the apparatus and controls which are needed to eliminate or minimize the effect of these factors and to secure accuracy and precision of result in perimetry.[2] In two papers by the present writers a method of treatment of the remainder of the factors was outlined,[3] namely, those which vary from individual to individual. It was also indicated in these papers that as fast as possible data would be given showing the influence of these factors on the size of the form and color fields. It is the purpose of the present paper to show the relation of three of them, errors of refraction, age and sex, to the size of the form-field for a stimulus subtending a visual angle of 1°. The influence of these factors can not be shown directly by experimental variation, as was the case with the factors which vary for a given individual. The procedure used has been to collect the data under standard conditions of control for a group of non-pathological individuals sampled to include the factors under investigation, and to determine their influence by statistical study so far as was possible in the situation presented.

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[1]Ferree and Rand, Research Laboratory of Physiological Optics, Wilmer Ophthalmological Institute, Johns Hopkins Medical School; M. M. Monroe, Graduate School of Medicine, University of Pennsylvania.

[2]C. E. Ferree and G. Rand, Chromatic thresholds of sensation from center to periphery of the retina and their bearing on color theory, *Psychol. Rev.*, 26, 1919, 16-41, 150-163; The absolute limits of color sensitivity and the effect of intensity of light on the apparent limits, *ibid.*, 27, 1920, 1-24; The limits of color sensitivity: effect of preëxposure and surrounding field, *ibid.*, 377-398; A new laboratory and clinic perimeter, *J. Exper. Psychol.*, 5, 1922, 46-67; The effect of variations of the intensity of the perimeter arm on the determinations of the color fields, *Psychol. Rev.*, 29, 1922, 457-473; Perimetery: variable factors influencing the breadth of the color fields, *Amer. J. Ophthal.*, 5, 1922, 886-895; The effect of size of stimulus and brightness of preëxposure and surrounding field on the extent and shape of the color fields, *J. Gen. Psychol.*, (in press).

[3]C. E. Ferree, G. Rand, and M. M. Monroe, Studies in perimetry: I. Preliminary work on a diagnostic scale for the form field, *Amer. J. Ophthal.*, 9, 1926, 95-104; II. Preliminary work on a diagnostic scale for the color fields, *ibid.*, 12, 1929, 269-285.

The results obtained by such a procedure are not so clear-cut and incisive as are those obtained by the experimental method, but the procedure used is the only one available in cases in which experimental analysis is not possible.

The fields were taken on the Ferree-Rand perimeter with a strict observance of all the precautions prescribed for the use of this perimeter. With this instrument all of the external conditions of the test can be held constant and reproduced at will. The fields were determined with a white stimulus on a black background. The coefficients of reflection of stimulus and background were respectively 78% and 4%. The illumination at every point in the field was kept constant at 7 foot-candles. In every instance the refractive condition of the eye was carefully determined and an ophthalmoscopic examination made. No cases showing a pathological condition, however slight, were included in the series. Ample rest periods were allowed between observations, and care was exercised that no field should be taken when the *S* was suffering from general fatigue or was otherwise unfit for accurate work. All determinations were carefully checked, but the *S* was not given extensive preliminary training. It was not the purpose of the writers to make examinations for the determination of the influence of the factors in question under conditions differing in this respect from those which obtain in the ordinary practice of perimetry. The results obtained may, therefore, be considered a fair example of the effects that may be expected in an average group of untrained *Ss* who show no pathology.

Fields were determined in eight meridional quadrants (0°, 45°, 90°, 135°, 180°, 225°, 270°, and 315°). The eyes examined included 75 cases of hyperopia and hyperopic astigmatism, 30 cases of myopia and myopic astigmatism; 40 cases of presbyopia; 5 cases of mixed astigmatism; and 50 cases showing no error of refraction or an error no greater than 1 diopter of hyperopia or ¼ diopter of hyperopic astigmatism. This last group contained no cases of myopia or myopic astigmatism of a detectable amount. For convenience of treatment it will be referred to as emmetropic. The *Ss* ranged in age from 8 to 56 years. There were 84 males and 116 females.

In making comparisons of the kind considered in this paper a single value or index is needed to represent size of field. Two types of index have been discussed and used in our former papers: the average breadth of field in the eight meridional quadrants; and the area of field as measured with a planimeter on a map drawn to a predetermined scale. The former of these has been selected for use in the present paper.

Graphic representations of results are given in Figs. 1-3. In Fig. 1 average breadth of field in degrees is plotted against frequency of occurrence. The total range of average breadth of field for the 200 cases was from 59.9°–74.4°. For the purpose of grading and representation in the plot the cases were separated into 9 groups, each group covering a range of 2°. The cases are designated with regard to condition of refraction according to a key shown in the chart.

The results represented in Fig. 1 may be summarized as follows:

(1) In general the emmetropes and hyperopes have the wider fields; the myopes and presbyopes, the narrower fields.

made here such a detailed specification of condition of refraction was quite impossible. (b) The placement of the image on the retina. For example, in cases of hyperopia due to deficient curvature of the cornea one might expect as a phenomenon of refraction that the image for a given excentricity of stimulus in the field would be formed nearer to the center of the retina than for the same position of the stimulus in cases of emmetropia and myopia. The tendency of this factor would be to widen the field for hyperopia. Conversely, in cases of myopia due to excessive curvature

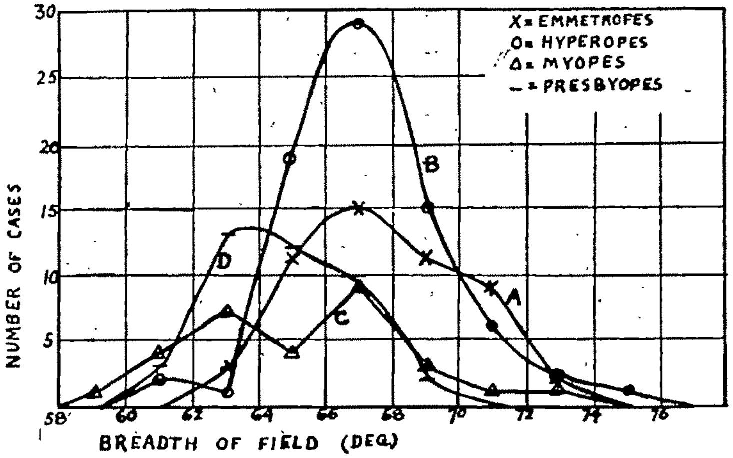

FIG. 2. SHOWING THE DISTRIBUTION OF 200 NON-PATHOLOGICAL CASES FOR EVERY REFRACTION GROUP

Curve A shows the distribution for the emmetropes; B, for the hyperopes; C, the myopes; and D, the presbyopes. The distributions are based on the average breadth of field in the eight principal meridional quadrants.

of the cornea, one might expect for a given excentricity of the stimulus in the field that the image would be formed relatively farther from the center of the retina. The influence of this factor would tend to narrow the field in myopia. (c) Characteristic differences in shape of eyeball, in degree of development and sensitivity of the nervous structure and in health of the eye. (d) Age as a dominant factor in presbyopia. With the exception of (d) the data presented in this paper do not justify any serious attempt at a differential evaluation of the possibilities suggested. It may be noted, however, that the sizes of field obtained in the greater number of cases of hyperopia and myopia are in the direction that might be expected from factor (b), curvature of cornea.

Table I shows for the entire group of 200 cases and for the different re fraction groups, the average value of average breadth of field, the standard deviation (σ), the coefficient of variability ($100\sigma/M$), the range for each group, and the range for the middle 50% of cases in each group. It is seen in this table that the averages are larger for the emmetropic and hyperopic groups; and that the scatter is greatest for the myopic and least for the presbyopic group.

TABLE I

SHOWING FOR THE ENTIRE GROUP OF 200 NON-PATHOLOGICAL CASES AND FOR EACH REFRACTION GROUP, THE AVERAGE VALUE OF AVERAGE BREADTH OF FORM-FIELD, THE STANDARD DEVIATION (σ), THE COEFFICIENT OF VARIABILITY ($100\sigma/M$), THE RANGE FOR ALL CASES IN EACH GROUP, AND THE RANGE FOR THE MIDDLE 50% OF CASES

Group	Average value of average breadth of form field	σ	$\frac{100\sigma}{M}$	Range of size of field: Total number of cases: Limiting values	Deg.	%*	Middle 50 per-cent of cases: Limiting values	Deg.	%*
Entire group (200 cases)	66.45°	2.62	3.9	74.4–59.9	14.5	24	68.1–64.6	3.5	5
Emmetropes (50 cases)	67.59°	2.56	3.8	73.0–62.9	10.1	15	69.8–65.3	4.5	7
Hyperopes (75 cases)	67.1°	2.57	3.8	74.4–60.6	13.8	20	68.3–65.6	2.7	4
Myopes (30 cases)	65.14°	3.10	4.8	72.0–59.9	12.1	19	67.0–62.9	4.1	6
Presbyopes (40 cases)	64.85°	2.14	3.3	69.1–61.0	8.1	13	66.1–63.3	2.8	4

*In computing the percentages for range the average value of the group in question was used as the base.

Tables II and III have been compiled to show the relation between age and average breadth of form-field in the eight principal quadrants. The range of size of field is treated in 5 groups of 4° each, the range of age in years in 6 groups of 10 years each. The rows of Table II show the frequency of occurrence of each group of size of field for each age-group; the columns, the frequency of occurrence of each age group for each size of field. In Table III is shown the percentage of occurrence of each group of size of field for each age group.

An inspection of these tables shows that there is some tendency for size of field and age to vary concomitantly, the larger fields occurring at the younger ages. The correlation between age and size of field computed by the product-moment formula of Pearson for linear relationship is -0.31 ± 0.04. There is, however, a slight non-linearity of relationship shown in the table, due to a pronounced shift towards smaller fields near 40 years of age. That is, above 40 years the mode occurs in group 66–69.9° (average 68°), while below 40 years the mode occurs in the group 62–65.9° (average 64°). Between 30 and 40 years a transitional stage is seen, where there is more nearly an equal number of cases

in the groups whose averages are respectively 68° and 64°. This shift of the mode downward at age 40, and the transitional stage between 30 and 40, will be shown in a later paper to occur also for the color fields. The average size of field for the age-group 10-19 is 67°; for the age group 20-29 is 67.5°; for the age group 30-39 is 66.8°; for the age group 40-49 is 64.8° and for the age group 50-59, 64.7°.

TABLE II

SHOWING THE NUMBER OF CASES IN EACH GROUP OF SIZE OF FORM-FIELD FOR EVERY AGE-GROUP

Age	Average breadth of form-field in degrees					
	58–61.9	62–65.9	66–68.9	70–73.9	74–77.9	Total
0-9			1			1
10-19		14	28	5		47
20-29	4	19	36	12	1	72
30-39	2	10	17	4		33
40-49	4	16	10			30
50-59		14	3			17
TOTAL	10	73	95	21	1	200

TABLE III

SHOWING THE PERCENTAGE OF CASES IN EACH GROUP OF SIZE OF FORM-FIELD FOR EACH AGE-GROUP

Age Group	Average breadth of form-field in degrees					
	58–61.9	62–65.9	66–69.9	70–73.9	74–77.9	Total
0-9			0.5			0.5
10-19		7.0	14.0	2.5		23.5
20-29	2.0	9.5	18.0	6.0	0.5	36.0
30-39	1.0	5.0	5.0	2.0		16.5
40-49	2.0	8.0				15.0
50-59		7.0	1.5			8.5
TOTAL	5.0	36.5	47.5	10.5	0.5	100.0

In Fig. 3 the data have been plotted to show separately the distribution of cases below and above 40 years of age. Curve A gives the distribution of the 153 cases below 40 years of age; Curve B the distribution of the 47 cases above 40 years of age. For ease of comparison the data for Curve B have been replotted in Curve C on a magnified scale. The factor of magnification used was 3.25, the ratio of the number of cases in the two groups.

It may be further noted that no very large fields (more than 70°) occur above the age of 40, and no very small fields (less than 62°) below the age of 20. These characteristics also are in general found for the color as well as the form-fields.

The narrower fields of the presbyopic group are probably largely a phenomenon of age rather than an effect of presbyopia. That is, presbyopia, loss of sensitivity, and the general failure of all the powers of the eye are in the main coördinate rather than interdependent phenomena of age. In the more advanced cases of presbyopia, however, the defective focussing for near seeing due

to loss of accommodation doubtless exerts a direct influence on the results of perimetry when the stimulus is viewed at a distance of 33 cm. Other age phenomena, extraneous to the functional changes in the sensory mechanism, which may exert an influence on the perimetric results are opacities of the cornea, lens and vitreous; changes in pupil size, etc.

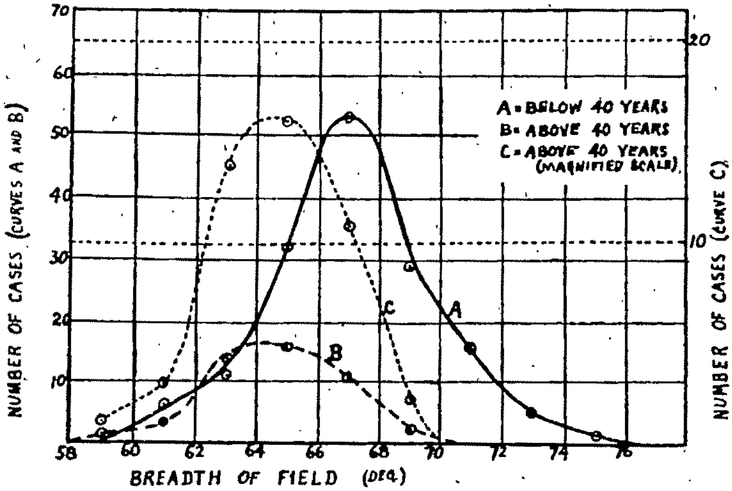

FIG. 3. SHOWING THE DISTRIBUTION OF THE NON-PATHOLOGICAL CASES BELOW AND ABOVE 40 YEARS OF AGE

Curve A shows the distribution of 153 cases below 40 years of age, Curve B the distribution of 47 cases above 40 years of age. Curve C shows, for comparative purposes, the cases above 40 years of age plotted on a magnified scale, the factor of magnification being 3.25—the ratio of the number of cases in the two age-groups.

The results have further been studied to show what relation, if any, exists between sex and the size of the form field. The 200 cases studied included 84 males and 116 females. The median value of size of field rated in terms of average breadth is 66.4°. For the males the average breadth of field for 36 cases or 43% of the number tested falls above, and for 48 cases or 57% of the number tested, below this value; for the females, the average breadth of field for 64 cases or 55% of the number tested falls above, and for 52 cases or 45% of the number tested, below this value. The average breadth of field for the males is 66.04°; for the females 66.75°.

Table IV gives the number of cases of each sex tested in the various refraction groups and the average breadth of field in

degrees for each. It is seen that the averages for the females are slightly but consistently larger than for the males in each group.[4] The difference is too small to be significant in practical perimetry, but it represents a possible point for further study. Whether it represents an actual difference in spread of sensitivity is problematic. The result may be due to other causes. Among these may be mentioned difference in facial conformation: the deeper set eyes, the more prominent nose, overhanging brows, etc. of the males. Some information as to the importance of the factor could be obtained perhaps from a study of the fields for the two sexes meridian by meridian. Sex differences and their analyses will be made the subject of more extensive study, should the data obtained in later work show that the point is of sufficient importance.[5]

TABLE IV

SHOWING THE NUMBER OF CASES OF EACH SEX STUDIED IN EVERY REFRACTION GROUP AND THE AVERAGE VALUE OF AVERAGE BREADTH OF FIELD

Refraction Group	Sex	Number of Cases	Average Value of Average Breadth of Field
Emmetropes	M	22	67.42°
	F	28	67.73°
Hyperopes	M	28	66.42°
	F	47	67.54°
Myopes	M	12	64.98°
	F	18	65.25°
Presbyopes	M	17	64.54°
	F	23	65.09°
Mixed Astigmatisms	M	5	65.50°
	F	0	———

In conclusion it may be stated that, (1) Errors of refraction are an important factor influencing the variability of size of the form-field from individual to individual. (2) After 40 years, age is also an important factor. Earlier than this the effect of age is negligible. (3) Sex apparently is not a factor of significant importance.

[4]In this connection the following quotation taken from a letter written to one of the writers by Dr. Henry H. Donaldson of the Wistar Institute of Anatomy and Biology under date of March 26, 1926, is suggestive: "In studying the albino rat we have found the eye bulbs of the female to be heavier than those of the male of like body weight, and the area of the cross section of the optic nerve to be also greater in the female. This last would possibly suggest a slightly greater extension of the retina in the female."

THE ANALYSIS AND SYNTHESIS OF BURNING HEAT[1]

By S. C. Ferrall, University of Illinois, and
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The studies of heat made during the past third of a century have definitely shown: (1) that the experience is aroused by the stimulation of near-lying cold and warm spots;[2] (2) that it is a simple, unique quality;[3] (3) that it has no qualitative resemblance to either warmth or cold;[4] (4) that pain is not a necessary or intrinsic component;[5] and (5) that the quality of heat lies in the pressure-pain continuum,[6] closer to pressure than to pain.[7]

The early investigators used areal stimuli and temperatures that were adequate for warmth and cold (normal and paradoxical), and frequently for pain. As a consequence, the authors were at odds regarding the ultimate

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[1]From the Psychological Laboratory, University of Illinois. The experiments were performed during the Summer Session of 1928.

[2]Torsten Thunberg, Förnimmelserna vid till samma ställe lokaliserad, samtidigt pagaende köld- öch varmerenting, *Upsala Läkaref. Förh.*, 1, 1896, 489 ff. Cf. also W. A. Nagel's *Handbuch der Physiologie des Menschen*, III, 1905, 707.

Sydney Alrutz, Om förnimmelsen 'hett,' *Upsala Läkaref. Förh.*, 2, 1897, 340-359; The sensation 'hot,' *Mind*, 141-144; Studien auf dem Gebiete der Temperatursinne, II. Die Hitzeempfindung, *Skan. Arch. f. Physiol.*, 10, 1900, 340-352.

Friedrich Kiesow, Zur Psychophysiologie der Mundhöhle, *Phil. Stud.*, 14, 1898, 567-588; Zur Analyse der Temperaturempfindungen, *Zsch. f. Psychol.*, 26, 1901, 231-246.

F. Cutolo, Jr., A preliminary study of the psychology of heat, this Journal, 29, 1918, 442-448.

J. H. Alston, The spatial condition of the fusion of warmth and cold in heat, *ibid.*, 31, 1920, 303-312.

E. B. Twitmyer and S. W. Fernberger, A demonstration heat grille, *ibid.*, 38, 1927, 119.

N. C. Burnett and K. M. Dallenbach, The experience of heat, *ibid.*, 38, 1927, 418-431; Heat intensity, *ibid.*, 40, 1928, 484-494.

W. B. Gritman and K. M. Dallenbach, The formula for the intensive gradation of heat, *ibid.*, 41, 1929, 460-464.

[3]Alrutz, *opp. citt.*; Cutolo, *op. cit.*, 445 ff.; Alston, *op. cit.*, 311; Burnett and Dallenbach, *op. cit.*, 423.

[4]Alrutz, *opp. citt.*; Cutolo, *op. cit.*, 445, 447; Burnett and Dallenbach, *op. cit.*, 420 ff.

[5]Alrutz, *opp. citt.*; Cutolo, *op. cit.*, 447 ff.; Alston, *op. cit.* 311; Burnett and Dallenbach, *op. cit.*, 420, 423.

[6]Cutolo, *op. cit.*, 445 ff.; Alston, *op. cit.* 311; Burnett and Dallenbach, *op. cit.*, 423.

[7]Burnett and Dallenbach, *loc. cit.*

nature of the experience aroused. Thunberg and Kiesow held to the view that it was a complex, *i.e.* a fusion of the cutaneous qualities; Alrutz, who ignored the adventitous pains, that it was unique and further unanalyzable.

The more recent investigators worked with punctiform[8] as well as areal stimuli,[9] and with temperatures that were selected so as to avoid the distracting influence of pain. They did not succeed in avoiding the concomitant arousal of pain with every observer in every experiment, but they did succeed often enough to establish the truth of Alrutz's contention. They obtained at times warm heats, cold heats, and burning and painful heats (just as it is possible with the proper stimuli to obtain warm sweets, cold sweets, and burning and painful sweets), but at other times, and most frequently, the quality of heat stood alone (as does the quality of sweet). When their stimuli were totally inadequate to ache, pain, and paradoxical cold, but adequate to warmth and normal cold, they obtained reports and introspective descriptions which clearly indicated that heat was a simple and unanalyzable quality lying in the pressure-pain continuum.

All the investigators reported, at times, the experience of burning or painful heat. It is probable, in the light of the recent experiments, that these experiences are merely admixtures of heat and pain; but we cannot definitely say so before subjecting them to descriptive analysis.

Object. The object of the present study was to describe the experience of burning heat. Our problem was two-fold: we had first the analytic problem in which we sought to determine, by introspective analysis, the qualities involved in the experience; and secondly, the synthetic problem by which we sought to test the accuracy of our analysis by synthesizing the experience, for, as Bentley says, "no better test of the accuracy of an analysis is possible than the reinstatement of the whole through synthesis of the products of dissection."[10]

METHOD AND PROCEDURE

Our study was divided into five series of experiments: Series I, a preliminary series, taken to determine our *O*'s limens for heat; Series II, synthetic heat; Series III, burning heat; Series IV, synthetic burning heat; and Series V, control series. Though the series are reported separately, Series II, III, and IV were actually conducted in haphazard order. The advantages of this procedure are: (1) it enabled the *O*s to compare, without knowing

[8]Cutolo, *op. cit.*, 443-446; Alston, *op. cit.*
[9]Cutolo, *opp. citt.*, 446 f.; Burnett and Dallenbach, *opp. citt.*; Gritman and Dallenbach, *op. cit.*
[10]I. M. Bentley, The synthetic experiment, this JOURNAL, 1900, 405.

it, the experiments of the different series; (2) it enabled us to judge more accurately their results; (3) it enabled *E* to make more rapid adjustments of the apparatus and thus to run off the experiments more rapidly; and (4) it enabled *D* to work without knowledge of the special conditions of the given experiments.

Apparatus. A temperature grill, identical with the one used by Burnett and Dallenbach,[11] was used in the present study. It consists of two systems of copper tubes bent and interlaced so as to form a grid or grill. Water was forced through the systems by the pressure of the University mains. The temperature of each system was controlled by passing the water through copper coils immersed in warm or cold water baths, and by controlling the water's rate of flow through the system. If the temperature of the warm system was too high, the rate was increased; if too low, the rate was decreased; and conversely with the rate in the cold system.

A small electrical binding post was soldered to each system for the purposes of the experiments in Series IV and V.

The temperature of the experimental room varied during the course of the experiments between 30° and 32°C. The *Os* found it uncomfortably warm, but, unfortunately, it could not be changed. The high temperatures did not, as we found, materially affect the results; they at most merely raised the *Os'* physiological zeros.

Observers. Our *Os* were Mr. E. E. Anderson (*A*), a senior in the University; Mr. J. G. Jenkins (*J*), an instructor in psychology; and the senior author (*D*). *A* was altogether untrained in introspection, and *J* in the observation of heat. They worked without knowledge of problem or method. *D* had served as *O* in two studies on heat, and worked in the present study with knowledge both of problem and procedure. He was not, however, informed of the progress of the investigation, and did not know the time-order of the different experimental series. The *Os* observed 5 days a week from 1-2 P. M.

Instructions. The following instructions were given to the *Os* at the beginning of every experimental hour.

"At the signal 'Now,' place the ventral (dorsal) surface of your right (left) forearm upon the grill. Relax, and keep the pressure of your arm as constant as possible. Remove your arm at the second signal 'Now,' and write your report."

Procedure. After *E* had brought each system of the grill to the desired temperature—and had in Series IV and V properly adjusted the electrical current—he called the *Os*, who were waiting in the experimental room, one at a time. *O* was given specific instructions regarding the arm and surface to be stimulated, seated himself beside the grill, and at the signal 'Now' placed his arm upon it. *E* gave the second signal 10 sec. later, *O* removed his arm and withdrew to write his introspective report, while the next *O* was taken in turn.

We gave the *Os* 8 experiments, on the average, during the course of a single hour. The number varied, however, from 5 to 11, depending upon the ease or

[11] *Op. cit.*, 419.

difficulty that *E* experienced in bringing the cold and warm systems to the desired temperature. None of the four experimental surfaces (the ventral and dorsal surfaces of the right and left arms) was stimulated more than once in 20 min.

The *Os*, in accordance with instructions, relaxed the muscles of the arm stimulated so that its whole weight bore upon the grill. This precaution of keeping the pressure of the arm on the grill as constant as possible from experiment to experiment was taken because, according to the results of Burnett and Dallenbach, the intensity of the heat experience is "conditioned in part by the pressure of the arm upon the grill."[12]

SERIES I. PRELIMINARY EXPERIMENTS

We first determined the *Os'* limens for heat, *i.e.* the warm temperatures that in half the cases aroused heat, so that we might, in the synthetic experiments which were to follow, use warm temperatures that were inadequate to heat and pain. Since the earlier experimenters did not determine the limens for their *Os*, one cannot tell whether or not the warm stimuli used in their experiments were adequate for heat and pain. It has generally and incorrectly been assumed that 45°C. is the critical temperature, and that values between this temperature and physiological zero arouse, if anything, only experiences of warmth.[13]

Procedure. We put the copper coils of both systems in the warm water bath. By adjusting the rate of the flow of the water, we brought the systems to the same temperature, 37°C., and then raised the temperature, by steps of 2°, to 45°C. Every *O* gave 2 reports at every step—10 reports in all.

Results. *J*'s limen for heat stood at 43°C., *A*'s and *D*'s at approximately 45°C. We consequently worked in the synthetic experiments with temperatures below these limits.[14]

SERIES II. SYNTHETIC HEAT

We reproduced, in Series II, the earlier synthetic experiments on heat—eliciting the experience of heat with warm and cold stimuli that are themselves inadequate to ache, pain, and paradoxical cold—not in the hope of gaining anything new, but for two other reasons: (1) to set off, by contrast as it were, the experiences of heat aroused (in Series III and IV) by stimuli ade-

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[12]Burnett and Dallenbach, *op. cit.*, 425. Bishop's heat grill (H. G. Bishop, An improved heat grill, this JOURNAL, 38, 1927, 648 f.), which, in our opinion, is the best that has been devised, would have insured a constant pressure and obviated this difficulty.

[13]Burnett and Dallenbach had one *O* (*op. cit.*, 424 f.) who reported heat every time he was stimulated with a temperature of 43°C. In a series of supplementary experiments they found that this *O*'s critical temperature was below 43°—the exact point was not, however, established.

[14]These critical temperatures are probably higher, because of the temperature of the experimental room (which varied between 30°-32°C.), than would have obtained under more favorable and usual conditions.

quate to pain; and (2) to obtain descriptions of heat aroused by stimuli inadequate to pain, that could for comparative purposes be used as norms.

Procedure. We varied the temperature of the cold system of the grill from 9°-25°C. for all the *Os*, and of the warm system from 40°-42.5° for *J*, and from 38°-44° for *A* and *D*. *A* did 21 experiments in this series, *D* 18, and *J* 16.

Results. *A* reported 3 heats, 5 intense warmths, and 13 moderate or mild warmths; *D*, 15 heats and 3 warmths; and *J*, 2 burning heats,[15] 10 heats, and 4 warmths.

(*1*) *The quality of heat.* The *Os'* descriptions of their experiences of heat corroborated, as the following excerpts show,[16] the view that heat is a unique and further unanalyzable quality.

D: (10°-43°) "Cold at first then hot . . . a smarting pressury experience. It is not painful;" (12°-43.5°) Heat . . . a pressury smart, but withal pleasant."
J: (10°-41°) "Heat of high intensity at first . . . a pricky experience."

(*2*) *Temporal aspects.* In the experiments in which heat was aroused—the only ones we are interested in at present—all of the *Os* reported, as the following excerpts from their introspections show, qualitative and intensive changes.

A: (14°-42°) "Cold, immediately followed by hot, then by a sensation of cold again;" (12°-43°) "Cold at first . . . then, after about a second, heat which gradually declined and disappeared, and then cold again."
D: (14°-38°) "Cold at first, then hot. The intensity of the heat diminished rapidly (probably due to the adaptation of the warmth) until the experience was indifferent, then it became cool, and then in the latter part of the experiment, cold.
J: (12°-42°) "A flash of cold, followed by a mild heat which adapted to cold before the end of the period."

With the exception of pressure, which was a constant accompaniment, cold was the quality most frequently experienced first.[17] This was followed by heat, which frequently gave way in turn to cold again.[18] Exceptions to this

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[15]The temperature combination used, when *J* reported the burning heats, was 12°-42.5°C. It is very likely that the warm temperature encroached upon *J*'s thermal pain limen, for he reported burning heat both times that 42.5° was used. Too few experiments were taken in the preliminary series to do more than to establish roughly the critical point. We could not, however, if we were to complete the main experiments in the short time at our disposal, devote more time to the preliminary series.

[16]*A*, the untrained *O*, did not attempt a description of his experiences in this series.

[17]This corroborates the results obtained by Cutolo (*op. cit.*, 444), Alston (*op. cit.*, 305), and Burnett and Dallenbach (*op. cit.*, 426).

[18]The sequence, cold, heat, cold, may be due to the shorter latent period for cold (cf. A. Goldscheider, *Arch. f.* [*Anat. u.*] *Physiol.*, 1887, 468-472; and F. Kiesow and M. Ponzo, *Arch. f. d. ges. Psychol.*, 16, 1910, 376-396) and to the shorter adaptation time for warmth.

general rule occurred most frequently in *J*'s results. He reported twice as the following introspections show, that heat appeared without a prelude and changed to warmth.

J: (10°-41°) "Heat of high intensity at first, faded rapidly to a diffuse warmth;" (13°-42°) "Intense heat, followed by milder heat which adapted to warmth, and this became weaker and almost disappeared before the end of the period."

Our results clearly indicate that the temporal course of the experience must be taken into account. If it is ignored and the *O*s' reports are telescoped, *Mischempfindungen* will of course result. Such composites, however, are artifacts of the method, not items of the experience.

Series III. Burning Heat

We wished in the experiments of Series III to obtain descriptive accounts of the experiences aroused by hot stimuli.

Procedure. We brought each system of the grill, as described in Series I, to identical temperatures, which we varied in haphazard order by steps of 1° from 45°-51°C. for *A* and *D*, and from 43°-50°C. for *J*. Because of the excessive stimulation of these temperatures and the long after-effects, experiments of this series were usually postponed until near the end of the experimental hour. Except in a few cases, they were the last experiments upon a given area in a given hour. We gave *A* 15 experiments in this series, *D* 14, and *J* 16.

Results. *A* reported that 46.6% of the experiments aroused burning heat, 40% heat, and 13.3% warmth; *D*, that 78.6% aroused burning heat, 14.3% heat, and 7.1% warmth; and *J*, that 50% aroused burning heat, 43.7% heat, and 6.3% warmth.

(*1*) *Quality:* (*a*) *Warmth.* The quality of warmth was reported with the minimally hot temperatures. The *O*s merely mentioned this quality, without attempting to describe it.

A: (45°-45°) "A mild warmth, which seemed to show very little or no adaptation;" (46°-46°) "Very warm, bordering on heat."
D: (45°-45°) "Warm throughout the course of the experiment."
J: (43°-43°) "A uniform course of fairly high degree of warmth—diffuse and showing slight adaptation."

(*b*) *Heat.* The heat aroused by the hot stimuli was identical in quality, as the following excerpts show, with that aroused by the warm and cold stimuli in Series II.

A: (45°-45°) "Mild heat, gradually faded to a warmth. No pain content was experienced throughout the experience;" (46°-46°) Heat, showing no adaptation. There was an occasional addition of pain. Pain would appear, last for about a second, then disappear for a second or two, and appear again. This fluctuation seemed to be due to poor contact on the dorsal side."
D: (46°-46°) "Hot . . . a smart; a pressury painful prick."
J: (45°-45°) "A flash of heat . . . This temperature experience, so difficult to describe, seems actually to be a bright pressure-pain, localized just under the skin."

(c) *Burning or painful heat.* The critical temperatures for burning heat were 45°C. for *J*, and 46° for *A* and *D*. Every stimulus-temperature above these values aroused for our *Os* experiences of heat, which were, as the following excerpts from the introspections show, predominantly painful.

A: (47°-47°) "Burning heat, no adaptation. Pain and heat content about equal;" (47°-47°) "Painful burning heat. There was a very slight adaptation of the heat content, but none of the pain content;" (51°-51°) "Burning heat. Impossible, because so painful, to hold arm on grill for 10 seconds."

D: (46°-46°) "In the burning hot experience, pain is the most prominent aspect. Quality lies on the pressure-pain continuum, between prick and pain, and nearer to pain than prick;" (47°-47°) "Hot . . . a pricky stingy pressury pain;" (48°-48°) "Hot at first, then burning heat. . . . The difference between hot and burning hot is the prominence of the pain-reminder." (51°-51°) "Hot at first, then burning hot; very painful. Reflex drawing of the arm away from the grill. Experience is too painful for good observation."

J: (47°-47°) "Burn, temperature component disappeared leaving a fairly bright pricking pain;" (48°-48°) "Burn, not as intense as some of the others. There was only a flash of heat then the burn which appeared as a dull, intense pressury-pain;" (50°-50°) "Burn . . . pain is outstanding, the temperature experience is negligible. Whatever temperature is present at first goes out of the focus of attention."

We are not prepared to say, upon the basis of the results of this series, whether burning heat is a *Mischempfindung, i.e.* a mixture or fusion of the qualities of heat and pain, or whether it is, as several of the introspections indicate, a simple quality lying in the pressure-prick-pain continuum nearer to pain than to pressure. But whether we regard it as a fusion, like a simple clang, or as a simple quality, like an orangish red, so much is certain: burning heat is predominantly a pricky, stingy, painful experience. It is also significant to note, in view of Thunberg's and Kiesow's results, that stimulus-temperatures adequate to pain arouse, in contradistinction to those inadequate to it, heat experiences that are predominantly painful.

(2) *Temporal aspects.* The temporal course in the experiments in which burning heat was aroused was not so varied as it was in the other experiments in this series, or as in the heat experiments of Series II. When burning heat was aroused, it usually appeared immediately and did not give way to another quality during the 10-sec. interval of the experiment. *A* reported no exceptions to this general rule. *D*, however, reported that the quality of burning heat was preceded once by warmth (stimulus-temperature 51°) and twice by heat (stimulus-temperature 47° and 51° respectively), and twice that burning heat "just before the end of the experiment changed to warmth." *J* reported two

preludes: "heat" (stimulus-temperature 49°) and "a flash of cold" (stimulus-temperature 47°), but no qualitative shifts after burning heat had appeared.

While the qualitative changes in time were not as pronounced in these experiments as in the heat experiments of Series II—probably because of the greater insistence of the quality of burning heat—they nevertheless emphasize once more the necessity of taking the temporal course of the experience into account.

Series IV. Synthetic Burning Heat

We attempted, in the experiments of Series IV, to synthesize burning heat from stimuli that separately are adequate only to warmth, cold, or pain.

Apparatus and procedure. We varied the temperature of the cold system between 9°-20° for all the *Os*, and of the warm system between 40°-44° for *A* and *D*, and between 38°-42° for *J*. We aroused pain electrically. We used the two grill systems, which were made of copper tubing and isolated from one another by asbestos, as electrodes. We attached an electric wire to the binding post soldered to each system. When *O* placed his arm upon the grill he completed the circuit (if the current was turned on) and received, in the part of the arm simultaneously stimulated by warmth and cold, numerous electrical 'shocks' which were described as tingling, stingy, pricky, or painful. We tried the faradic current, but abandoned it, because of the difficulties it involved, in favor of the 110 v., 60 cycle, alternating current drawn from the University lighting line. We controlled the strength of the current, which was varied, in haphazard order, between 10-15 v., by means of a rheostat and voltmeter. *A* did 48 of these synthetic experiments, *D* 40, and *J* 36.

Results. We obtained, as the following introspections show, reports of painful or burning heat from all the *Os*.

A: (10°-43°-10 v.) "Painful heat of moderate intensity;" (11°-44°-10 v.) "Cold at first, then suddenly replaced by painful burning heat which remained constant throughout the experience;" (17°-40°-11 v.) "Cold, jumped to a painful burning heat which declined gradually to a mild warm."

D: (16°-43.5°-10 v.) "Burn of weak intensity, persisted throughout the course of the experiment;" (16°-44°-11 v.) "Burning painful heat. Either the experience is from a truly hot stimulus or else it is an excellent reproduction, as I cannot tell which it is;" (11°-42°-12 v.) "Burning hot at first, then adapted to hot, to warmth, and at the end of the experiment became cold;" (15°-42.5°-13 v.) "Burning hot at first, then warm, and then, near end of the experimental interval, burning hot again. The quality of stingy burning heat very clear."

J: (16°-42°-10 v.) "Burn at first—continued with slight adaptation to the end;" (20°-40°-11 v.) "First a neutral pressure, then a mild burn;" (14°-42°-12 v.) "Definitely burning heat—mild and diffuse."

A reported 'burning' in 12.5% of the experiments in this series, *D* in 85%, and *J* in 33.3%. *A* and *J* reported fewer cases

in this series than in Series III (normal hot stimuli), where they reported 'burning heat' in 46.6% and 50% respectively of the experiments. *D*, on the contrary, reported more cases of 'burning heat'—85% as compared to 79%—in the present series. *A*'s and *J*'s results were in accordance with our expectations, *D*'s exceeded them.

The difference between the results of *D*, on the one hand, and *A* and *J*, on the other, may be due to (1) 'set' *D* know the object and plan of the experiment, and he may unconsciously have been biased in his reports in the direction of burning heat (*D* admits this possibility, but he is certain that he had not anticipated a particular result, and that the qualities he reported were matters of indifference to him); to (2) differences in experience and training. *D* was highly trained in cutaneous observation in general and in the introspection of heat in particular; J, though an experienced *O*, had never before observed in a cutaneous experiment; and *A* was a novice; to (3) differences in the effectiveness of the stimuli—particularly the electrical stimuli. The cutaneous stimuli were, as the experiments of Series V indicate, more effective for *D* than for either *A* or *J*, possibly because of differences in the amount of pressure of the arm upon the grill or in the amount of perspiration which bathed the arm.[19]

The lower intensities of the electrical current were more effective for all the *O*s than the higher in producing synthetically the experience of burning heat. All the 'burning heats' reported by *A* and *J* occurred when the lower intensities were used. The 'pain' experiences at the high intensities were, moreover, as the following reports show, very different in character from those aroused by the normal stimulus of burning heat.

D: (20°43°-14 v.) "Burning hot . . . The 'stings' are too coarse to duplicate the experiences of a stimulus of 50°. These 'stings' are separate, individual; the stings in the real burning heats are indistinguishable. The 'burning hot' of this experiment was like the real only for a moment." (15°-43°-15 v.) "Burning heat at first blush, then nothing but the pricky painful stings of the electrical current. The 'stings' of the current are too coarse to duplicate, beyond the first moment of the experiment, the experience aroused by a burning hot stimulus."

The difference between the 'stinging' pains aroused in these experiments and those aroused by the normal stimulus is more clearly brought out by the following report given by *D* after being stimulated by a temperature of 51°C.

[19] *A* and *J* each weighed about 130 lbs., *D* about 210; the room temperature varied between 30°-32°C.

D: (51°-51°) "Warm for an instant, then burning heat. The 'pain' or 'sting' is uniformly spread over the stimulated area; it is not punctiform as in the last experiments [with strong electrical stimuli], but is uniform like a smart. In order to reproduce the experience of burning heat synthetically, one must have a 'pain' that is areal and not punctiform."

While the results of this series of experiments clearly show, in confirmation of the analysis of Series III, that burning heat may be aroused by stimuli that singly are adequate only to warmth, cold, or pain (areal sting), they tell us nothing regarding the ultimate nature of the experience.[20] The question, whether burning heat is a *Mischempfindung* or a unique quality that lies in the pressure-prick-pain continuum, still remains an open one.

Series V. Control Experiments

After completing Series II, III, and IV, we gave the *O*s the experiments of Series V to learn, for purposes of comparison, the separate effect of the electrical current.

Procedure. We brought the temperature of each system of the grill to neutrality (33.5°-34.5°), and used, in haphazard order, the strengths of current (10 v.-13 v.) that gave the best 'burning heats' in Series IV. Under these conditions *A* observed in 21 experiments, *D* in 18, and *J* in 24.

Results. We obtained nothing that resembled heat, burn, or burning heat in the slightest degree, or that could be confused or mistaken for it. The *O*s described their experiences as pressury pricks which were mild, intense, biting, and sometimes tingling. The 'pricks' were uniformly reported as punctiform, sometimes evenly distributed over the stimulated surface, at other times scattered in an irregular mosaic.

Corollary Results

We report, as a matter of record, the following results which confirm those obtained by previous investigators. Though the *O*s were not directed in the instructions to do so, they frequently reported the spatial characteristics of their experiences, and the after-sensations aroused.

(*1*) *Spatial characteristics.* The *O*s reported, in every series of experiments, that several qualities were at times experienced together, not as fusions, but as locally distinct experiences which ran their courses separately. One quality might be experienced at one part of the stimulated area, another

[20]In Series III, *D* and *J* gave reports indicating that burning heat is a unique and unanalyzable experience. We hoped to gain further evidence in this series but were disappointed as *J* did not touch upon the point, and *D* referred to it only once. We give the instance. *D:* (10°-43°-12 v.) "Cold at first blush, then burning hot, and then hot without the burn. Burning hot is hot plus areal sting; it is a perception, *not* a simple quality." Whether this holds only for experiences synthetically aroused or applied to all burning heats, we cannot say. It is significant however, that *D* did not limit or qualify his statement, and that he gave no evidence of appreciating that the experience was synthetically aroused. This introspection, moreover, was given after those mentioned in Series III—in the third from the last experiment given in the regular series.

quality at another part, and still a third quality at a third part. For example: D, (10°-43°-12 v.) "Cold along the right edge, warm along the left, and burning hot in the middle."

The experiences were localized on and in the arm. They were described as 'surfacy,' 'cutaneous,' 'on the skin,' or as 'deep,' 'subcutaneous,' 'under the skin.' We are unable, principally because too few experiences were localized, to draw any correlation between the quality of experience and the localization. There is some indication, however, in the results of *J*—who reported this aspect more frequently than either of the other *O*s—that 'heat' is more superficial than 'burning heat,' that burning heat involves more tissues and tends to be localized deeper in the skin than heat.

(2) *After-experiences.* Very pronounced after-experiences were reported by all the *O*s. In Series II (synthetic heat), *A* reported "wetness," "prick," "cold," "warmth;" and *D* reported "wetness." *J* did not mention any in the experiments of this series. In series III (burning heat), *A* reported "warm glow," "areal sting," "pain;" *D* reported "smart," "burning pain," "smarting pain;" and *J* reported "burn," "burning pain." In Series IV (synthetic burning heat), *A* reported "wetness," "cold," "sting," "stinging pain;" *D* reported "wetness," "smart," "burn," "smarting stinging glow;" and *J* reported "burning sting."

The after-experiences aroused in Series II are qualitatively different and, in general, less intense than in Series III. Those aroused in Series IV are intermediate both in quality and in intensity.

Summary

The results of this study may be summarized as follows.

(1) The limen for heat—*i.e.* the point at which paradoxical cold is aroused—varies for different *O*s.

(2) The experiences of heat, aroused by stimuli that are separately adequate only to warm or cold, is a unique quality that lies in the pressure-prick-pain continuum, closer to pressure than to pain.

(3) Burning heat is predominantly a pricky, stingy, painful experience that may be aroused synthetically by stimuli that singly are adequate only to warmth, cold, or pain (areal sting).

(4) The ultimate nature of the experience of burning heat, *i.e.* whether it is a *Mischempfindung* or a unique quality that lies in the pressure-prick-pain continuum nearer to pain than to pressure, is still undecided.

A COMPARATIVE STUDY OF VOLUNTARY AND HYPNOTIC CATALEPSY[1]

By Griffith W. Williams, University of Wisconsin

Introduction

There are two rather widely quoted studies of the effect of catalepsy on the movements and oscillations of the extended arm. One of these is by Rieger who made a comparative study of normal and trance catalepsy with one other person and himself as subjects.[2] He tried to record the movements of the arm by means of a pencil held in the fingers and placed directly in contact with the paper on a revolving drum. He found, however, that the arm made horizontal as well as vertical movements. This, together with the fact that adequate pressure to secure a tracing would have obscured some of its essential features, led him to abandon this method. He then tried the ingenious method of holding a pin between the fingers and having its shadow cast on the revolving paper. A record was made by having the experimenter trace the course of this shadow with an inked brush. He claims that this gives satisfactory results and several of the tracings are reproduced in the reports of his work.

Rieger found that the arm of the hypnotic cataleptic subject dropped 18 cm. in one hour. He does not mention the rate of the dropping of the arm in voluntary rigidity, but his conclusion can be inferred from his statement that it is impossible to hold the arm in the same position for 2 min. in the normal state in spite of the subject's efforts to do so. During the trance a considerable increase in the oscillation of the extended arm was also found at the end of an hour. No feelings of fatigue were reported after experimentation in the trance. He noticed that breathing affected the movements of the arm so that the latter became somewhat rhythmical. He also claimed that the arm-movements were affected by the heartbeat.

A source of error is introduced into the results of Rieger by the fact that the instructions in the trance and normal states were not identical. In the normal state, the subjects were told to hold the arm rigidly in a horizontal position. In the cataleptic state, the arm was lifted into the horizontal position by the experimenter and left there. Such lack of comparability between trance and normal conditions is an error which is by no means unknown in hypnotic experimentation.

Charcot has also investigated this problem.[3] There is no description of his apparatus in the references given here, but its general characteristics can be

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[1]From the Psychological Laboratory, University of Wisconsin. This is the fifth of a series of Wisconsin studies on hypnotic phenomena. The writer wishes to acknowledge his indebtedness for the aid that he received from Professor Clark L. Hull.

[2]C. Rieger, *Der Hypnotismus*, 1884, 18 f.

[3]Reported by A. Binet and C. Féré, *Animal Magnetism*, 1888, 120 f.; and also by J. Crocq, *L'hypnotisme scientifique*, 1900, 26 f.

inferred from a drawing published by Binet and Féré.[4]
Marey's tambour attached to the back of the hand witl
tended position on the level of the shoulder. A weight i
upon, the rubber dam of the tambour so that a quick
will cause a disturbance of the air-pressure within the c

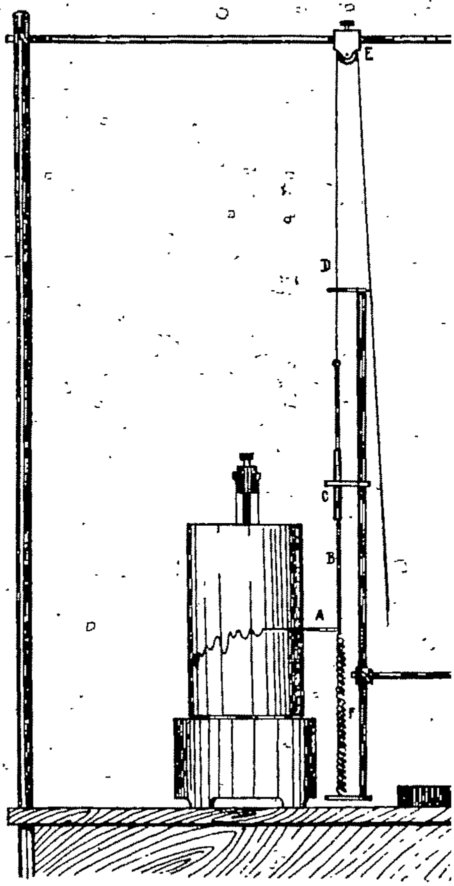

FIG. 1. DIAGRAM OF THE APPARA'
(See text for description)

of the weight. The tambour on the hand is connectec
and recording lever by means of rubber tubing. The lev
makes a tracing on the drum whenever there is a disturl
within the first tambour. This apparatus will record
the hand but will not record the gradual falling of the
ments.

[4] Op. cit., 121.

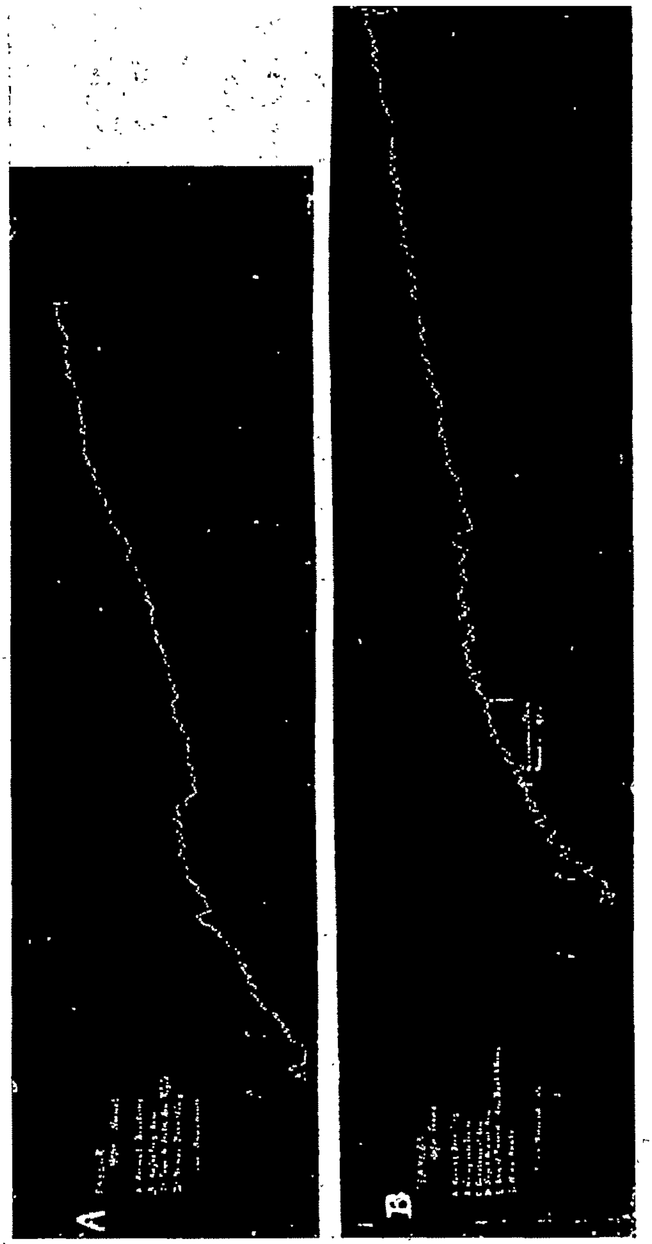

FIG. 2. TRACINGS OF *Re*

A is a normal and B a trance record. Each record shows a tracing of breathing at the top, then a tracing of the arm-movements, a signal-line, and a time-line. The time-lines are broken at intervals of 5-sec.

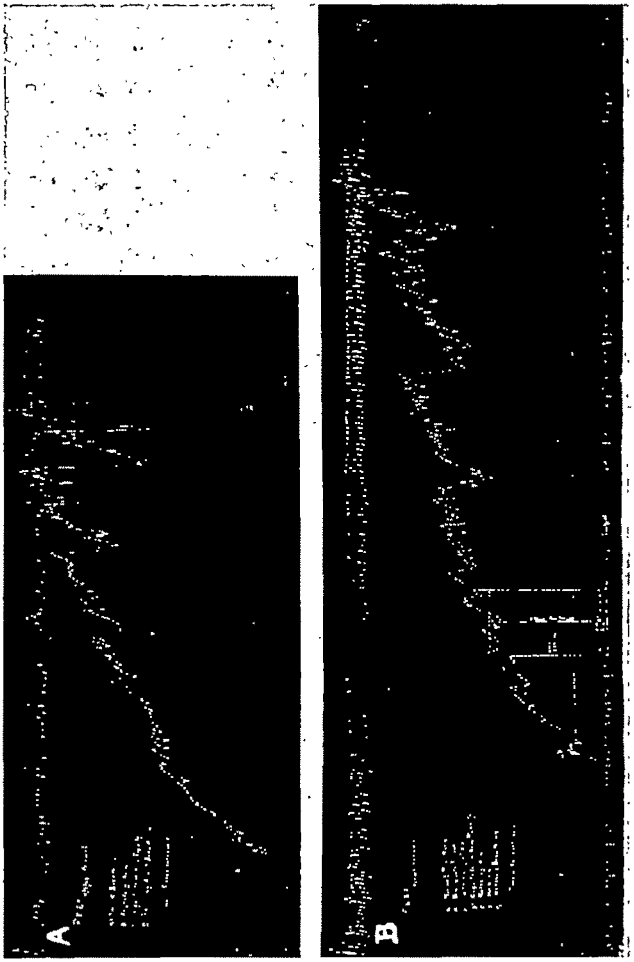

FIG. 3. TRACINGS OF *T*

A is a normal and B a trance record. Each record shows a tracing of breathing at the top, then a tracing of the arm-movements, a signal-line, and a time-line. The time-line is broken at intervals of 5-sec. in A, and of 10-sec. in B

Charcot used only one hypnotic subject, a hystero-epileptic patient. Records obtained from this subject show no trace of oscillation in the cataleptic state. In the normal state, when another subject "attempts to maintain the cataleptic attitude," the records show that oscillation soon begins and increases rather rapidly in amount. Charcot claims that the arm drops slowly and evenly in the cataleptic state although it seems evident that no objective record of this could be made with the apparatus shown in the drawing. He reports no essential difference in the length of the periods during which the arm is held up.

Binet and Féré[5] and Moll[6] quote these results as a satisfactory test for simulation. Bramwell, however, wisely adds the caution that "as most, if not all, of the muscular feats of hypnosis could be reproduced by trained unhypnotized persons, the phenomena are only worthy of note when it can be clearly proved that they are beyond the hypnotic subject's waking powers."[7]

Statement of Problem

The present experiment is an attempt to make a comparative study of the involuntary movements of striped muscle in hypnotic and voluntary catalepsy. The experimental series was so arranged that the subjects themselves served as their own controls and all conditions, except that of being in the trance or normal state, were kept constant.

Method and Procedure

The arm was used because of its convenience in experimentation. The movements studied would thus be the descent of the arm from an extended horizontal position toward a vertical position, and the oscillation of the arm during this period. Inspection of the movements of the arm reveals the fact that both horizontal and vertical oscillations occur, but in the present experiment only the vertical are studied as it was felt that the horizontal ones would show the same general characteristics.

Apparatus and technique. A tracer was constructed in which a fine moveable stylus *A* (Fig. I) is attached to a rod *B* which runs through a metal sleeve *C*. A thread *D* is attached to the rod and runs over a pulley *E*, thence down to the wrist of *S* where it is attached to a leather cuff. The contact of the pointer *A* with the smoked paper is held at a constant but very light pressure by a hairspring *F*. The downward movement of the arm is recorded as an upward tracing on the paper.

The tracings were made on smoked paper 10 inches wide. The horizontal length of the record is determined by the rate of the falling of the arm as the stylus moves off the top of the paper when the arm has fallen a certain distance. Some of the records of two of the *S*s were terminated by the experimenter when the individuals showed such evidence of acute pain as profuse perspiration. These latter were the longest records made. A pneumograph tracing was taken simultaneously in order to discover any relation that might exist between

[5] *Op. cit.*, 133.
[6] A. Moll, *Hypnotism*, 4th ed., 1913, 208.
[7] J. M. Bramwell, *Hypnotism*, 3rd ed., 1921, 80.

breathing and the arm-movements. The *Ss* sat in a chair so that the arm was free to move in any direction. The right arm was used for all *Ss* except one who had received an injury to her right arm in childhood.

The *S* were put into the trance by having them look into one of *E*'s eyes. Verbal suggestions of being in a deep trance were also used when *S*'s eyes had closed. Cataleptic rigidity of the arm was induced by verbal suggestions that the arm was becoming stiff and rigid. The arm was simultaneously lifted to the level of the shoulder by *E*, and the thread was then attached to the cuff

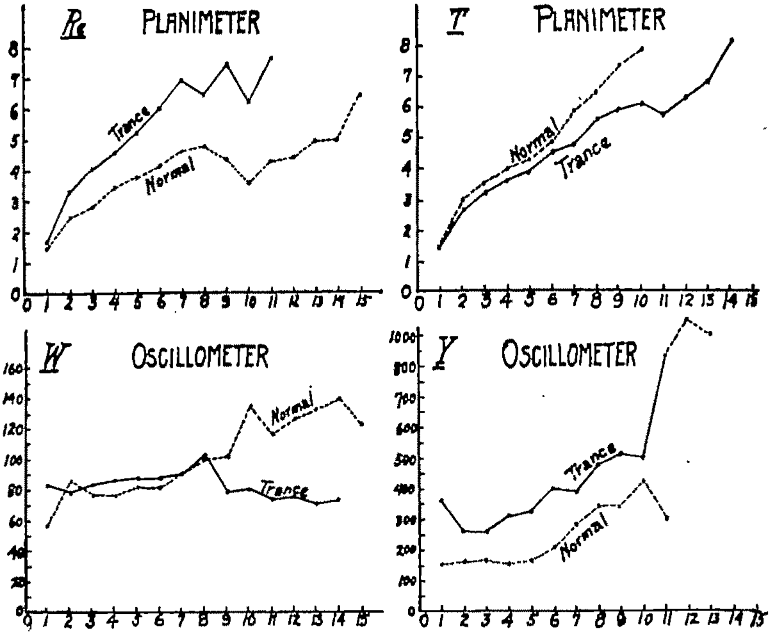

FIG. 4. TYPICAL GRAPHS OF *Re*, *T*, *W*, AND *Y*

Re and *T* show opposite tendencies in the rate of dropping the arm. *W*'s curve resembles closely the composite curve of the group of *Ss*; *Y*'s illustrates an extreme case in which there is more oscillation in the trance than in the normal state.

about *S*'s wrist. In the normal state the arm was similarly lifted, but without the repeated suggestions of catalepsy. A voluntary catalepsy was induced by giving the single instruction, "Hold your arm there rigidily." This instruction was also given after the arm had been lifted in the trance cataleptic state. This was the only instruction given, so that conditions were identical when the arm had been made cataleptic and lifted in the trance state and had been similarly lifted by *E* with voluntary catalepsy in the normal state.

Every *S* served at a regular time of the day and at regular intervals. The time of day, however, varied for the different individuals.

A control series was secured by alternating the experimental series as follows: N(ormal), T(rance), T, N, T, N, N, T. At least two practice periods were given before the eight records summarized in the tables were made. This arrangement of the experimental periods provides both a normal control series and a means of equalizing practice effects.

This apparatus and technique provide a basis for two measurements of the effect of catalepsy on the movements of the arm: (1) a curve of the rate of falling of the arm; and (2) a curve of the amount of vertical oscillation of the arm. Four illustrative records are reproduced in Figs. 2 and 3.

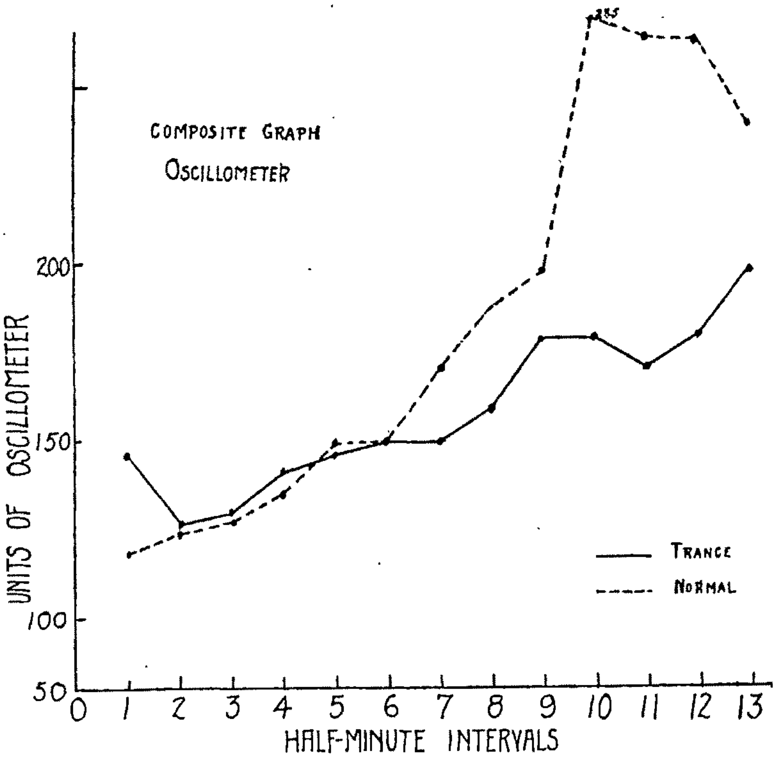

FIG. 5. COMPOSITE GRAPHS

Plotted from the averages shown in Table IV of the entire group of *Ss*

The curve of the falling of the arm was secured from measurements of the tracings made with a planimeter. The records were first divided into 1/2-min. intervals. Verticals were then drawn at these points so that together with the tracing at the top and the time-line at the base, they formed irregular polygons. When the area of one of these polygons is divided by the length of the base line, the average height of the line traced by the falling arm is secured. An illus-

trative example of such a measurement is outlined in Fig. 2, B. In this polygon are indicated (1) the length of the base line, *e.g.* 2.12 in., (2) the area of the polygon, *e.g.* 8.29 sq. in., and (3) the average height of the line of tracing, *e.g.* 3.91 in. This latter is indicated by the height of the horizontal dotted line. Similar measurements were made for all the 1/2-min. intervals in the 64 records.

The records were also measured by Hull's linear oscillometer.[8] By means of this instrument it is possible to summate the vertical oscillatory movements made by the arm. The line of the record is traced with the pointer of this instrument. The upward movements are summated on one dial and downward movements on the other. The units on the dials are approximately 1/40th of an inch. The interval for these measurements was also a half-minute. One of these outlined in Fig. 3B.

Subjects. Eight *S*s (2 men and 6 women, and all students) were used. Six of them were somnambules; such phenomena as automatic writing and posthypnotic amnesia could be produced in them. The other two were not as susceptible; they could be put into a light trance in which catalepsy was easily induced, but not in a state that induced posthypnotic amnesia. None of the *S*s knew the object of the experiment. They also worked without knowledge of their results.

RESULTS

An illustration of how the data were treated is shown in the accompanying tables. Table I shows the detailed planimeter readings for two records. These records are reproduced in Fig. 2 and one of the intervals measured has been outlined. With the 1/2-min. interval on the time-line as base, the area enclosed by this base, two verticals, and the tracing, was measured. The average height of the tracing was then found for each unit measured. These averages are also shown in the table.

When the records of a *S* had been measured and the heights of the tracings ascertained, the totals of the average heights of the tracings of the 4 trance and 4 normal records, respectively, were secured. An average of these was secured as shown in Table III and individual graphs were plotted from these averages. Two typical graphs are reproduced in Fig. 4. *Re*'s graph is typical of those *S*s who show a more rapid falling of the arm in the trance than in the waking state; *T*'s graph is typical of those *S*s whose arm falls more rapidly in the normal state.

There is no satisfactory way of separately measuring the amount of movement due specifically to the falling of the arm as distinguished from its downward oscillation. In the measure-

[8] C. L. Hull, An instrument for summating the oscillations of a line, *J. Exper. Psychol.*, 12, 1929, 359-361.

TABLE I

PLANIMETER MEASUREMENTS OF THE TRACINGS OF *Re* THAT ARE REPRODUCED IN FIG. 2

Record	Type	Variable	Half-minute intervals										
			1	2	3	4	5	6	7	8	9	10	11
A	N	area	3.55	5.80	6.50	8.22	8.16	10.23	10.70	12.11	9.78		
		length	1.50	1.50	1.50	1.50	1.44	1.50	1.60	1.60	1.56		
		av. heights	2.37	3.87	4.33	5.48	5.67	6.82	6.69	7.57	6.27		
B	T	area	2.23	5.14	8.29	8.60	8.87	9.18	11.09	8.93	11.00	11.13	6.29
		length	1.40	1.80	*2.12	1.80	1.74	1.80	1.80	1.80	1.74	1.78	0.82
		av. heights	1.59	2.85	*3.91	4.78	5.10	5.10	6.16	5.13	6.25	6.25	7.67

*This interval is outlined in Fig. 2B.

TABLE II

OSCILLOMETER MEASUREMENTS OF THE TRACINGS OF *T* THAT ARE REPRODUCED IN FIG. 3

Record	Type	Movement	Half-minute intervals									
			1	2	3	4	5	6	7	8	9	10
A	N	downward	107	96	53	31	60	65	72	60	51	
		upward	45	47	22	50	27	38	27	22	28	
B	T	downward	146	92	93	68	67	68	62	63	70	63
		upward	60	*51	52	71	47	42	42	46	53	34

*This interval is outlined in Fig. 3B.

ments shown, this movement is included in the downward amount of oscillation. As this is constant for all records, however, it does not invalidate the comparative conclusions of this study.

The data obtained from measuring the records with the oscillometer were treated in a somewhat similar way. The upward and downward oscillations for every 1/2-min. interval were measured and recorded as shown in Table II. The records from which the measurements in this table were made are shown in Fig. 3, A and B. The upward and downward oscillations were added together for each interval, and the totals averaged for all normal and all trance records respectively. These averages are shown in Table IV. Individual graphs were then plotted from these averages. Two graphs of the relative oscillations in the normal and trance states are reproduced in Fig. 4. *W* shows an approximately equal amount of oscillation in both states to the end of the fourth minute. The graph of *Y* is included to show an extreme case where there is more oscillation in the trance than in the normal state.

Table III is a composite table including the average heights of the tracings of all *S*s determined from planimeter measurements. Separate averages are given for the normal and trance records respectively. It thus provides a summary of all the measurements made with the planimeter. A final composite average for the normal and trance records respectively is given in the last two rows of this table. The oscillometer readings are summarized in the same way in Table IV. The graph (Fig. 5) of these final oscillometer averages is included.

To ascertain whether the differences between the composite averages of the trance and normal records were true differences or merely due to chance, it was necessary to compute the P.E. of the difference between these averages. The computations, therefore, were made for the averages of the ninth, tenth, and eleventh 1/2-min. intervals and the following values obtained: 52.96, 73.43, and 103.56. When these values were substituted in the formula: D/PE_{diff}, in which D is the difference, values of 0.39, 0.33, and 0.28 were obtained respectively. The chances in 100 that the true difference in each of these cases is greater than zero are 60, 59, and 58, respectively. These averages are thus unreliable statistically, and at best, merely indicate a possibility. In general, it means that if this experiment were repeated with an infinite number of *S*, the chances are 6 out of ten that the difference would be

TABLE III

SHOWING FOR EVERY S AND FOR THE GROUP AS A WHOLE THE AVERAGE HEIGHT OF THEIR TRANCE AND NORMAL TRACINGS

The averages for every S were obtained from 4 records each, those for the group from 32 records each.

		Half-minute Intervals																								
S	Type	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
C	T	1.32	2.16	2.84	3.99	5.49	6.94	8.61																		
	N	1.19	1.48	2.29	3.38	4.81	6.55	7.4	57.97	8.72																
Ra	T	1.37	2.44	3.60	4.52	5.33	6.25	6.96	8.00	8.72																
	N	1.61	2.45	3.70	4.61	5.74	6.11	6.72	7.33	7.92	8.14	7.60	8.10													
Y	T	1.51	2.78	3.29	3.66	3.80	4.07	5.09	4.55	5.25	5.12	3.66	3.79	5.47												
	N	1.30	1.72	2.37	2.43	3.81	3.16	4.48	4.19	3.97	5.14	6.99	5.48													
T	T	1.48	2.63	3.31	3.63	3.96	4.53	4.80	5.60	5.92	6.13	5.76	6.26	6.84	8.15											
	N	1.63	2.97	3.55	4.00	4.30	4.87	5.86	6.50	7.34	7.85															
B	T	0.97	1.75	2.03	2.42	2.69	3.01	3.55	4.14	4.56	3.99	4.24	5.07	6.65	7.87											
	N	1.03	1.43	1.99	2.50	3.32	3.69	4.20	5.23	5.68	6.44	6.89	7.06	8.00												
Re	T	1.72	3.55	4.06	4.60	5.28	6.07	6.95	6.51	7.43	6.25	7.67														
	N	1.52	2.43	2.81	3.45	3.82	4.16	4.63	4.77	4.40	3.61	4.34	4.46	4.98	5.04	6.49										
F	T	1.20	1.31	1.33	1.58	1.82	2.03	2.26	2.24	3.03	3.84	4.08	4.78	4.73	3.66	4.09	4.44	6.58	7.25	7.43	6.67	7.73				
	N	0.82	1.32	1.73	2.10	2.77	3.48	4.07	4.53	5.61	6.34	5.44	5.93	5.08	6.31											
W	T	1.15	1.62	2.13	2.64	3.36	4.08	4.91	5.76	6.11	6.61	7.34	7.67	8.71	8.44	8.36										
	N	0.87	0.92	1.02	1.29	1.16	1.38	1.48	1.78	2.20	2.67	2.90	3.20	3.55	3.90	4.19	4.62	4.78	5.16	4.69	4.72	5.53	5.57	5.63	5.79	5.77
Composite	T	1.33	2.25	2.81	3.78	3.97	4.55	5.29	5.23	5.82	5.21	5.44	5.87	6.84	7.10	5.51	4.44	6.58	7.52	7.43	6.67	7.73				
	N	1.25	1.83	2.43	2.97	3.71	4.17	4.63	5.04	5.52	5.66	5.09	4.80	4.65	4.49	3.88	4.62	4.78	5.16	4.69	4.72	5.53	5.57	5.63	5.79	5.77

in the same direction, but 4 out of ten that they would be in the opposite direction. About the weakest reliability that is accepted in experimental science is 0.95, *i.e.* 95 chances out of a hundred of its being true and only 5 in a hundred of its being false.

Comments and Conclusions

It has been generally assumed that the hypnotic trance tends to decrease individual differences, but this experiment illustrates to a marked degree the retention of differences in performance by *S* in the trance. The range of performance represented by the 8 *Ss* in both of the measurements used is very striking and the statistical reliability of any average made from such measurements will be small. A similar wide range of variability in muscular reactions during hypnosis was found in work previously reported by the writer.[9]

The *Ss* reported an absence of feelings of pain in the arm after the taking of the trance records. This was not due to any conscious suggestion given by the experimenter. Rieger[10] reports the later occurrence of fatigue, and Binet and Féré[11] its non-appearance in the trance. The failure to report any feelings of pain in this experiment is analogous, if not identical with these observations. Acute pain was reported during the making of the normal records.

No characteristic difference appears in the gross average length of time during which the arm was held up or in the curve of the falling of the arm. Table III shows that 4 *S* held their arms up longer in the trance and four in the normal state. This seems to confirm Moll's conclusion that "a cataleptic person cannot hold his arms up longer than an imposter."[12] It raises a question, however, concerning the interpretation of the reported absence of feelings of pain. Normally, it would be expected that the arm would be held up longer when there are no feelings of pain. It is possible that there are other factors functioning in the trance which would counterbalance the effects of this analgesia, but it is also possible that the analgesia may merely be an inhibition of verbalized reports of feelings of pain and that otherwise the physiological characteristics of pain are just as acute in the trance as in

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[9] G. W. Williams, The effect of hypnosis on muscular fatigue, *J. Abn. Psychol. & Soc. Psychol.*, 24, 1929, 318–329.

[10] *Op. cit.*, 59, 67.

[11] *Op. cit.*, 133.

[12] *Op. cit.*, 208.

TABLE IV

SHOWING FOR EVERY *S* AND FOR THE GROUP AS A WHOLE THE AVERAGE AMOUNT OF OSCILLATION AS MEASURED IN UNITS OF THE OSCILLOMETER

The averages for every *S* were obtained from 4 records each, those for the group from 32 records each.

		Half-minute Intervals																								
S	Type	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
C	T	168	150	154	142	156	164																			
	N	200	217	253	301	318	273	331	314																	
Ra	T	100	74	74	62	84	76	84	73	69																
	N	103	87	91	102	86	75	66	82	90	102	63	109													
Y	T	357	263	259	307	329	398	390	476	512	502	830	1187	981												
	N	154	161	163	158	166	208	281	344	345	428	305														
T	T	156	129	133	145	115	97	110	119	105	133	118	90	124												
	N	140	153	119	137	126	174	149	139	146																
B	T	110	82	94	97	99	99	114	117	125	130	126	186	222												
	N	89	94	99	96	119	101	114	108	88	125	81	88													
Re	T	173	227	227	263	286	251	277	266	302	570															
	N	185	190	225	211	274	253	323	491	503	576	641	794	687	933											
F	T	100	95	96	109	96	111	114	133	146	146	197	144	209	207	282	243	176	154	210	321					
	N	102	83	72	77	103	116	124	164	196	272	236	181	285	342											
W	T	83	79	83	86	87	87	90	103	78	80	74	75	71	73											
	N	56	85	77	77	82	82	90	99	101	133	116	125	132	138	122	146	134	136	146	116	176	122	132	178	162
Composite	T	156	137	140	151	156	160	160	169	189	189	181	190	208	140	282	243	176	154	210	321					
	N	128	134	137	145	159	160	180	197	208	285	273	274	250	305	122	146	134	136	146	116	176	122	132	178	162

the normal state. These facts suggest a problem that would probably yield fruitful results for investigation.

A curve plotted from the composite averages shown in Table III closely resembles that of *T* (Fig 4) in general characteristics except that the composite curve shows (1) less negative acceleration, and (2) the cataleptic arm falls slightly faster than the normal state. The obtained differences in the averages from which the composite curve was plotted (Table III) were so small that it seemed unnecessary to make any further statistical computations. On the basis of the work done in the present experiment the curve of the falling of the arm may then be regarded as having a very slight negative acceleration and as essentially like the individual curve to which reference has been made.

The curves of oscillation, however, seem to show a clearer distinction between the trance and the normal performance. Five *S*s show less oscillation in the trance, 2 show more, and one shows an equal amount of oscillation in both states. A composite graph shows an equal amount of oscillation up to the end of the sixth 1/2-min. interval and then a marked increase in oscillation in the normal state as compared with the trance. This difference, however, was found upon investigation to be statistically unreliable. Curves were not plotted beyond the thirteenth 1/2-min. interval because the number of units included in the averages is small, as may be seen from an examination of Table IV. The general characteristics of the curve of the amount of oscillation contrast with those of the curve of the falling of the arm. The curves of both the normal and trance oscillation show positive acceleration. This is quite marked in the case of the normal oscillation.

The statistical reliability of the differences of the averages, however, indicates that too much emphasis should not be placed on the shape of the curve. It is probable that much of the present theory of hypnosis would be modified if adequate statistical treatment were applied to the results of experimentation. The convincing appearance of the curve as contrasted with its statistical unreliability raises the question of the value of the work of Charcot, Rieger, and others, when they report experiments on only one subject. Rieger, Binet and Féré, and Moll report oscillation in the normal state as contrasted with no oscillation in the trance. The results of the present experiment merely indicate that there

is a tendency for more oscillation to occur in the normal than in the trance state. The fact that the difference in amount of oscillation does not occur at the beginning of the periods does not seem to have been previously reported.

Breathing has a pronounced effect on the movements of the arm as is shown by an inspection of any of the records. Breathing introduces a rhythmical movement into the tracing made by the arm. It often occurs, *e.g.* in Fig. 2, B, that a large movement of the arm is synchronous with deeper or more uneven breathing. Rieger reports this same characteristic and it seems probable that most of the oscillations shown on the charts published by him are due to breathing and do not represent the independent oscillations of the arm at all.

The following conclusions seem to be warranted by the data presented in this study.

(1) There is a failure to report feelings of pain during the making of the trance records which is not due to any suggestion given by the experimenter. The question of the relation of feelings of pain to work accomplished was, however, not specifically studied in this experiment.

(2) The *Ss* used in this experiment were unable to hold up their arms any longer in the trance than in the waking state.

(3) There is possibly an increase in the amount of oscillation in the normal state as compared with that in the trance state after the first three minutes of experimentation.

(4) The characteristic curve of the falling of the arm shows slight negative acceleration and that of the amount of oscillation a more clearly emphasized positive acceleration.

DEFINITE AND INDEFINITE PREPARATION IN THE VISUAL APPREHENSION EXPERIMENT

By Miles A. Tinker, Dorothy Roberts, and Harriet Jackson
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Most investigators, in determining the span of visual apprehension, have employed some type of tachistoscope for presenting stimuli. To maintain conditions which yield the most valid results various authorities agree that this short exposure technique must be carefully controlled. Evaluation and criticism of the tachistoscopic technique have been made by Becher, Binet, Cattell, Dearborn, Erdmann and Dodge, Dodge, Goldscheider and Müller, Hoffmann, Huey, Kutzner, Schumann, Whipple, Wundt, and Zeitler.[1] Wundt, in his discussion, formulated seven essentials of a good tachistoscope. One of these requirements is that a ready-signal must be given at an appropriate time before the exposure. The question arises what is the appropriate time-interval and how definitely must the observer be prepared in order to respond most efficiently. In reaction-time experiments a preparatory interval of 2 sec. has been found most favorable for yielding a quick response to stimulation. Also definite preparation (a definite time-interval between the ready signal and the exposure) produces faster reaction than indefinite preparation.[2]

In many tachistoscopic studies of visual apprehension the preparatory interval has been 2 sec. and there has always been definite preparation. This interval is convenient to use and it appears adequate for this kind of experimentation. Does the observer, however, need, in order to respond efficiently, as definite a preparation in the visual apprehension as in the reaction-time experiment? Our problem is to determine the relative efficiency of apprehension with definite and indefinite preparation in the visual apprehension experiment when the time of exposing stimuli is held constant.

Method. The tachistoscopic method of presentation was employed to determine the span of visual apprehension for letters in nonsense series under each of the two conditions of the experiment. Two students (*J* and *R*) and one member of the faculty (*T*) served as *S*s in this investigation. Every *S* had had previous experience in short exposure observation.

The stimulus material consisted of 200 groups of capital letters arranged in nonsense series. The letters were about 7 mm. high and each series was printed with black india ink in a horizontal line 4.5 cm. long. The line of letters was centered on a white card 9 by 9 cm. To avoid nonsense syllables only consonants were employed in constructing the groups of letters. There were 7 letters in each series. Additional stimulus cards were prepared for the practice series.

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[1]For references and discussion see G. M. Whipple, *Manual of Mental and Physical Tests*, 1910, 222 ff., 243 f.

[2]Cf. H. Woodrow, The measurement of attention, *Psychol. Monog.*, 17, 1914, (no. 76), 16-65.

Apparatus. A Whipple disk tachistoscope was used for presenting the stimuli. An exposure interval of 120σ was employed. This interval allowed ample time for clear perception of the exposure field, was brief enough to preclude eye-movement, and was a convenient interval to use with the apparatus. The pre-exposure field carried a fixation-mark. The *Ss* sat close to the table upon which the tachistoscope was mounted. Their eyes were on a level with the exposure field and at a distance of approximately 20 in. from the material to be read.

Procedure. Every *S* observed regularly twice a week with a few exceptions when there was only one sitting per week. The experiment consisted of two parts: the preliminary series with definite preparation and the variable series including both definite and indefinite preparation. In the preliminary part of the experiment the first sitting was a practice session. Then there were 4 regular observation periods at each of which 50 series of letters were shown.

In the second part of the experiment every *S* was shown 55 stimulus series at each sitting. The first five cards constituted a warming-up exercise and were given with a definite preparation of 2 sec. (*i.e.* a known interval of 2 sec. between the ready signal and the exposure). These 5 series were not used in computations. The next five cards were also given with a definite preparation of 2 sec. For the remaining 45 series indefinite preparation was employed. The intervals used with indefinite preparation were 2, 4, 8, 12, 16, 20, 24, and 32 sec. long. The timing of these intervals was done with a stop watch. Five trials of each interval were given at a single setting. The order of the intervals with indefinite preparation was haphazard and unknown to *S*. In all there were for every *S* 20 sittings during which 100 experimental series were shown with definite preparation (2 sec.) and 100 at each interval under indefinite preparation. When the series of 200 stimulus cards had been read once the cards were shuffled and used again. This occurred once about every 2 weeks. Every day before the experimentation began the *Ss* read the following instructions (definite preparation):

"The object of this experiment is to discover how many letters can be read during a very brief given exposure. The series of 7 letters will appear at the center of the exposure window. After you have viewed the group of letters repeat the series aloud to *E*. Say 'blank' to designate the omission of a letter.

"At the ready signal fixate your vision on the cross at the center of the exposure window; 2 sec. later *E* will say 'Now' and then after 2 more sec. the stimulus will appear in the center of the region which you are fixating."

For the indefinite preparation the directions were modified informing *S* that, following the "now" signal, the stimulus might appear after any one of the several intervals cited above and that the sequence of the intervals would be varied in an irregular order.

E recorded responses as given by *S*.

Scoring. The responses were scored for the number of letters correctly reproduced in the right order. Any misplaced item was considered wrong.

Experimental Results. The preliminary series of 200 trials with definite preparation was completed in order to allow the *Ss* thoroughly to adapt themselves to the experiment and partially to stabilize the spans of visual apprehension. Table I shows the average span for every consecutive series of 50

responses and for the total of 200 presentations. *R* and *J* increased their spans slightly during this part of the experiment while *T*'s span decreased by a small amount. On the average, however, the results for the group show that the spans of visual apprehension were rather constant for the 4 sittings (column 8 of the table).

TABLE I

SPAN OF VISUAL APPREHENSION IN THE PRELIMINARY SERIES

The average number of items correctly read per exposure for each successive group of 50 trials and for the whole 200 trials with the standard deviations of the distributions for every *S* and for the group.

No. of Readings	*R*		*T*		*J*		All *S*s	
	Av.	σdis.	Av.	σdis.	Av.	σdis.	Av.	σdis.
1st 50	3.22	0.37	3.28	0.36	4.04	0.92	3.51	0.55
2nd 50	3.30	0.49	3.02	0.38	3.92	0.95	3.41	0.61
3rd 50	3.34	0.42	3.18	0.63	4.24	0.66	3.59	0.57
4th 50	3.42	0.56	2.94	0.38	4.32	0.54	3.56	0.49
All 200	3.32	0.70	3.11	0.67	4.13	0.89	3.52	0.75

TABLE II

THE INFLUENCE OF DEFINITE AND INDEFINITE PREPARATION ON THE SPAN OF VISUAL APPREHENSION

The average number of items correctly read per exposure and the standard deviations of the distributions for every *S* and for the group. 100 readings for every *S* at each interval.

Preparation	Interval in Sec.	Subjects						All *S*s	
		R		*T*		*J*			
		Av.	σdis.	Av.	σdis.	Av.	σdis.	Av.	σdis.
Def.	2	3.70	0.93	3.58	0.74	4.43	1.00	3.90	0.95
Indefinite	2	3.63	0.95	3.67	0.75	4.30	1.02	3.87	0.98
	4	3.70	1.14	3.43	0.73	4.52	0.89	3.88	1.03
	8	3.87	0.96	3.46	0.75	4.45	0.95	3.93	0.99
	12	3.71	1.08	3.36	0.70	4.45	1.15	3.84	1.10
	16	3.83	1.10	3.53	0.74	4.42	1.05	3.93	1.05
	20	3.76	0.89	3.52	0.88	4.14	0.86	3.81	0.93
	24	3.86	1.09	3.44	0.71	4.38	0.94	3.89	1.05
	32	3.80	1.04	3.48	0.71	4.36	0.94	3.88	0.99
	All	3.77	1.03	3.49	0.74	4.38	0.98	3.88	1.02

Results from the second part of the experiment are given in Table II. Averages under the conditions of definite preparation are shown in the first row. These figures are to be compared with the results in the rest of the table. Examination of the averages for individual *S*s reveals some variation between definite and indefinite preparation in span of apprehension and between various intervals under indefinite preparation. For *R* the largest difference between averages is 0.24, which is found by comparing the 8 sec. with the 2 sec. interval under indefinite preparation. This difference is of doubtful

statistical significance since D/σ_{diff} yields only 1.79. When the average for *R*, under conditions of definite preparation, is compared with the average of all intervals with indefinite preparation, the difference is 0.07 and may be attributed to chance.

For *T*, however, the largest difference of 0.31 between the 2 sec. and the 12 sec. interval with indefinite preparation has high statistical significance, since $D/\sigma_{diff} = 3.04$. The next greatest difference, however, yields a D/σ_{diff} of only 2.31 which indicates comparatively low reliability for predictive purposes.

Results for *J* show no differences which can be assigned to other than chance factors. The largest difference of 0.22 between the 2 sec. and the 4 sec. intervals with indefinite preparation gives a D/σ_{diff} of 1.63. For both *T* and *J* the differences between averages under definite preparation and averages for all intervals with indefinite preparation are chance differences.

If the group averages for all three *Ss* are considered, similar trends are discovered. The largest difference of 0.12 between the 8 sec. and the 20 sec. intervals with indefinite preparation yields $D/\sigma_{diff} = 1.54$, which indicates that the difference has little or no reliability for prediction. For these group comparisons (column 9) the average under definite preparation is almost identical with the average span for all intervals with indefinite preparation (3.90 and 3.88). The various total averages in the lower section of column 9 of the table show striking agreement. Only two of the 9 averages with indefinite preparation vary more than 0.03 of a point from the average span of 3.90 under definite preparation.

Examination of the standard deviations of the distributions will show that they are relatively constant for the various intervals with indefinite preparation, but that they are slightly greater for indefinite than for definite preparation (1.02 *vs.* 0.95). This difference is of comparatively slight statistical significance.

With definite preparation the range in span was 2 to 7 letters; for indefinite preparation 1 to 7. Ordinarily the *Ss* adjusted themselves quickly and efficiently to the conditions of indefinite preparation. Occasionally, however, due to wandering of the attention, the *Ss* obtained scores of only 1. On the other hand, scores of 6 were approximately twice as frequent with indefinite as with definite preparation. In general the conditions of indefinite preparation tended to foster in the *S* a relaxing and receptive attitude which seemed to aid apprehension.

The evidence presented above indicates that visual apprehension is equally efficient under either definite or indefinite preparation and that with indefinite preparation any single time interval is as effective for efficient apprehension as any other within the limits of 2 to 32 sec. Why is indefinite preparation less efficient than definite preparation in the reaction-time experiment, while the two conditions of preparation are equally effective in visual apprehension? There is an important distinction between the two experiments which may be cited as a partial explanation. The reaction-time experiment calls for an immediate motor response on perception of the stimulus and the quickness with

which the response is initiated is the measure involved. In visual apprehension the responses is a delayed reaction. Although the stimuli are directly apprehended or apprehended by re-imaging the stimulus material, the response is given at the convenience of *S*.

The results described in this report have a certain bearing on the control of tachistoscopic procedure in work with visual apprehension where the time of exposure is approximately 1/10 sec. The findings indicate primarily that no timing device is needed for controlling the length of the preparatory interval. Any interval which is convenient to work with, such as 2 sec., may be employed. A rough approximation of this interval, as determined by *E*'s counting "one, two," or by some similar device, will yield satisfactory and reliable results. It is not necessary to have *S*'s attention at a high level at the instant of the exposure, though such a condition may be highly desirable where the time of exposure is very brief, *i.e.* 5σ.

Summary and Conclusions

(1) This study was undertaken to determine the relative efficiency of apprehension with definite and indefinite preparation in the visual apprehension experiment.

(2) In a preliminary experiment the span of visual apprehension was determined for the three *S*s with a definite preparatory interval of 2 sec.

(3) In the second part of the experiment spans were obtained with a definite preparation of 2 sec. and with indefinite preparation for intervals of 2, 4, 8, 12, 16, 20, 24, and 32 sec.

(4) No reliable differences were discovered between spans with definite and spans with indefinite preparation nor between spans with preparatory intervals of different lengths under the conditions of indefinite preparation.

(5) The results warrant the conclusion that the use of either definite or indefinite preparation yields equally efficient apprehension in the visual apprehension experiment when the time of exposure is approximately 1/10 sec., the definite preparation is 2 sec., and the indefinite preparation varies between the limits of 2 and 32 sec.

TIME-LIMIT *VS.* WORK-LIMIT METHODS

By DONALD G. PATERSON and MILES A. TINKER, University of Minnesota

It would seem desirable to discover through experiment whether or not the time-limit method and the work-limit method of administering speed tests are equivalent and interchangeable. For the most part this question has been discussed in connection with the relative advantages and disadvantages of the group *vs.* the individual procedure in test administration. The difficulties of utilizing the work-limit method in group testing have usually been recognized with the implication that the group testing method suffers as a consequence of using the time-limit method. Theoretical considerations indicate that rate of work or speed of performance might be measured either by amount of work per unit of time (time-limit) or by the time required per unit of work (work-limit). The practical difficulties, however, of arranging test materials of equal difficulty throughout the test are held by some to be so great as to place in question results obtained by the use of the time-limit method. Such reasoning as this led Whipple to say: "There is no doubt, therefore, that the work-limit method is to be preferred to the time-limit method: it is better, in other words, that every *S* should be asked to perform the same work and to measure his efficiency in terms of elapsed time than to require every *S* to work for the same time and to measure his efficiency in 'ground covered.' But the time-limit method is compulsory in all tests of this order undertaken by groups, so that this constitutes yet another serious objection to the group-method."[1] In a footnote Whipple states that he has been able to use the work-limit method successfully with groups of reliable *Ss* by the aid of a newly devised time-clock.

In a search through testing manuals we have been unable to discover any actual evidence regarding the essential identity of results obtained from the two methods under consideration. Authors seem to take it for granted that the two methods yield equivalent results and then proceed to discuss the practical advantages and disadvantages of each method in connection with a variety of testing situations.

In view of the absence of experimental data it was considered desirable to study experimentally this question through the use of a speed of reading test known to have been carefully constructed to yield two duplicate forms containing paragraphs each equal in difficulty to every other.[2] The problem is to determine the 'true' correlation (obtained r corrected for attenuation) between speed of reading scores obtained by the time-limit method and by the work-limit method. College students enrolled in elementary and advanced psycho-

[1] G. M. Whipple, *Manual of Mental and Physical Tests*, 1914, I, 8-9.

[2] J. C. Chapman and S. Cook, The principle of the single variable in a speed of reading cross-out test, *J. Educ. Res.*, 8, 1923, 389-396. Forms A and B of the Chapman-Cook Speed of Reading Tests were used.

logy classes were used as *Ss*. In all, 1090 students were tested. They were subdivided into the following four experimental groups:

Group I	Form A, Time-limit method.
(n = 185)	Form B, Work-limit method.
Group II	Form A, Work-limit method.
(n = 183)	Form B, Time-limit method.
Group III	Form A, Work-limit method.
(n = 162)	Form B, Work-limit method.
Group IV	Form A, Time-limit method.
(n = 560)	Form B, Time-limit method.

Data from Groups I and II permit the computation of coefficients of correlation between paired scores for Forms A and B. These coefficients constitute the *intercorrelations* between the two methods of testing. Groups III and IV permit the computation of similar correlations. These latter correlations are the *reliability coefficients* for the two methods of testing. Since the formula for correcting correlation coefficients attenuated by the presence of chance errors necessitates the mere division of the average intercorrelation by the average reliability coefficient our method of analysis is simple and straightforward. In practice, the correction formula employs the geometric mean. In our problem the numerator is the square root of the product of the two intercorrelations and the denominator is the square root of the product of the two reliability coefficients.[3]

Details of administration and scoring the tests followed our previous practice.[4] The time-limit employed was 1¾ min. In conducting the tests with the work-limit method a letter code was used, a different letter being placed on the blackboard every 10 sec. The students were instructed to record on their papers whatever letter happened to be on the blackboard at the instant they completed their tests. The purpose of the experiment was explained before the tests were given.

Groups I, II, and III were tested during the Spring Quarter of 1928-29. The results for Group IV were obtained in connection with another experiment (the effect of length of line on speed of reading) during the Fall Quarter of 1928-29. Evidence will be given later indicating that the results for Group IV may legitimately be included as a part of the present experiment.

Results. Table I contains all of the basic data for our comparisons. Variations in the means and standard deviations of the distributions for the different experimental groups probably reflect minor differences of sampling since classes composed of sophomores were included along with classes of advanced undergraduates and graduates.

The coefficients of variability are reported to show the effect of method upon the dispersion of reading scores. It is obvious that no definite trend exists in this respect. Thus the time-limit and the work-limit methods seem to be equally effective in the measurement of individual differences in speed of reading.

[3] H. E. Garrett, *Statistics in Psychology and Education*, 1926, 211-213.

[4] M. A. Tinker and D. G. Paterson, Studies of typographical factors influencing speed of reading, I. Type Form, *J. Appl. Psychol.*, 12, 1928, 359-368; II. Size of type, *ibid.*, 13, 1929, 120-130; III. Length of line, *ibid.*, 13, 1929, 205-219.

TABLE I

BASIC DATA REGARDING THE TIME-LIMIT AND WORK-LIMIT METHODS OF ADMINISTERING THE CHAPMAN-COOK SPEED OF READING TESTS TO COLLEGE STUDENTS

			Mean	S.D.	C. of Var.	r	N
I.	Intercorrelations						
	Form A	Time-limit	21.12	5.02	0.24		
	Form B	Work-limit	3′8″	50″	0.27	0.87 ± 0.01	185
	Form A	Work-limit	2′50″	41″	0.24		
	Form B	Time-limit	20.30	4.29	0.21	0.84 ± 0.01	183
II.	Reliability Coefficients						
	Form A	Work-limit	2′36″	42″	0.27		
	Form B	Work-limit	2′48″	46″	0.27	0.86 ± 0.02	162
	Form A	Time-limit	19.05	4.34	0.23		
	Form B	Time-limit	17.87	3.73	0.21	*0.84 ± 0.01	560

Main interest centers in the column containing the coefficients of correlation. For Groups I, II, and III these are remarkably similar; such slight differences as exist are within the limits of the P.E. With respect to the coefficient of correlation for Group IV, it is to be noted that the standard deviations of the distributions for this group are smaller than in the preceding groups. This is because Group IV was composed of sophomores alone whereas the other three groups were composed of students with a much greater range of talent. This fact necessitates the correction of the coefficient of correlation obtained for this group ($r = +0.78 \pm 0.01$) by employing Kelley's formula[5] ($\sigma/\Sigma = \sqrt{1-R}/\sqrt{1-r}$) in order to make the obtained r comparable with the rs for the other three groups with respect to heterogeneity of subjects. The application of this formula yields a corrected r of $+0.84 \pm 0.01$.[6]

The similarity in magnitude of the two intercorrelations (+0.87 and +0.84 for Groups I and II) and the two reliability coefficients (+0.86 and +0.84 for Groups III and IV) indicates quite clearly that the two testing methods agree with each other as closely as each agrees with itself. Thus application of the correction for attenuation formula $r_{TW} = \sqrt{r_{T_1W_2} \cdot r_{W_1T_2}}/\sqrt{r_{T_1T_2} \cdot r_{W_1W_2}}$ results in a 'true' correlation of +1.00.[7] In other words the time-limit method and the work-limit method produce speed of reading scores which are perfectly correlated within the limits of the reliability of each. Hence, Whipple's strong

*r of $+0.84 \pm 0.01$ was derived from an obtained r of $+0.78 \pm 0.01$. See text for explanation.

[5]T. L. Kelley, The reliability of test scores, *J. Educ. Res.*, 5, 1921, 370-379.

[6]The use of a reliability coefficient for the time-limit method obtained when Form B differed from Form A in typographical arrangement might be questioned. Limitation in number of available subjects forced us to follow this procedure. However, the consistency with which Form A correlated with Form B in a series of eleven experimental groups (80 students in each group), in some of which Form B differed in type size, and in others in length of line, suggests that the reliability of the time-limit method as *method* is being revealed. These eleven correlations ranged from +0.70 to +0.85; the average was +0.80.

[7]H. E. Garrett, *op. cit.*, 211-213. Subscripts used above: T = Time-limit method, W = Work-limit method, 1 and 2 refer to the two forms of the test.

preference for the work-limit method fails to receive any objective substantiation in this experiment. If these results should be verified by other experiments in which various types of speed tests are utilized and administered either by the individual or by the group method of testing, it would follow that the time-limit method and the work-limit method are theoretically and empirically equivalent. Decisions regarding which method to employ would then be wholly a matter of convenience in relation to the particular kinds of tests and subjects to be employed.[8]

Summary and conclusions. (1) In view of the absence of experimental evidence regarding the equivalence of the time-limit and work-limit methods of test administration, it seemed desirable to experimentally study the question.

(2) The Chapman-Cook Speed of Reading Tests were administered to 1090 college students so as to yield intercorrelations between the two methods of testing as well as the reliability coefficients for each method.

(3) The work-limit method was found to agree with the time-limit method as closely as each method agrees with itself. When the 'true' correlation between the two methods was computed, by use of the attenuation correction formula, r was found to be +1.00.

(4) Within the limits of the conditions of this experiment, the time-limit method and the work-limit method are equivalent.

[8] This conclusion does not solve the perplexing problem of whether time per unit of work or work per unit of time should be employed in experiments concerning the effect of practice on individual differences. It is obvious that both units of measurement may be derived from either the time-limit or the work-limit method of test administration. Cf. J. Peterson, Thurstone's measures of variability in learning, *Psychol. Bull.*, 15, 1918, 452-455.

APPARATUS

An Improved Automatograph

By W. N. Kellogg, Indiana University

Considerable difficulty is often experienced in obtaining good results with the automatograph of the type described by Titchener[1] and Myers[2] because of the fact that (a) if the screws against which the stylus is supposed to slide are adjusted too tightly, free vertical movement of the stylus is hampered. On the

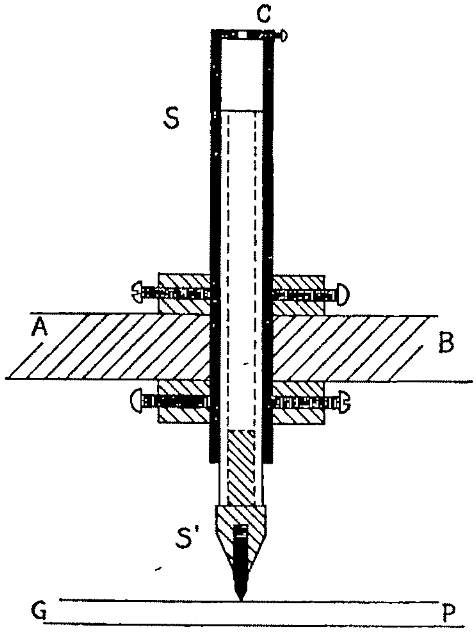

Fig. 1. An Improved Automatograph

other hand, if these screws are too loose, the horizontal play of the stylus makes clicks, when the direction of movement is changed, which are magnified by the resonance of the automatograph board so that *S* becomes aware of the slightest tremor. (b) The stylus frequently fails to run smoothly upon the recording paper, again forcibly apprising *S* of movements which would otherwise be unperceived. In the attempt to obviate one or the other of these drawbacks we have tried a glass stylus with ink writing point, a special stylus constructed of

[1]E. B. Titchener, *Experimental Psychology, Student's Manual, Qualitative,* 1901, 95 f.

[2]C. S. Myers *A Textbook of Experimental Psychology,* Pt. ii, 1911, 101 f.

polished wood which would not resound, various kinds of recording pens and inks, including the fountain pen furnished by Stoelting, and tracing on smoked papers with various forms of recorders. Success was only partial, however, until an entirely different type of stylus and writing principle were adopted. These are illustrated in the accompanying diagram (Fig. 1).

A-B represents the automatograph board with the usual wooden blocks above and below it through which run the 8 screws (4 of which are shown) that center the stylus. The stylus hole is drilled to fit a piece of heavy brass tubing (18 mm. in outside diameter and 14 cm. long) which is rigidly secured in the hole. A small disk with air hole is fastened in the top of this tubing at C. A similar piece of heavy tubing, S-S′ (2.5 mm. in thickness), which serves as the sliding element of the stylus, is machined to move easily within the outer tube *but without horizontal play*. The writing end at S′ is cut from a piece of hard wood; a machine screw is then inserted in the point of the cone and the end of the screw is turned down on a lathe to produce a fine but rounded point (black in the diagram). The wooden end is thereupon plugged into the lower opening of the smaller brass tube to complete the stylus.

Recording is accomplished by causing the stylus to write directly upon a heavy piece of plate glass (GP) which has previously been coated with Bon Ami. The weight of the stylus is sufficient to keep it continuously upon the plate and the use of glass instead of paper as a writing surface insures perfect smoothness and the elimination of 'catches.' If desired, the records can easily be made permanent by placing a piece of sensitized paper beneath them and printing after the usual manner of making blue prints, or by putting the glass upon a black background and photographing.

A New Electrode for the Hathaway Galvanic Reflex Apparatus

By Christian A. Ruckmick, University of Iowa

While the Hathaway apparatus is still in process of improvement, in the author's judgment it has one outstanding feature in that it eliminates battery and polarization effects in the electrodes because it sends through the portion of the body to which the electrodes are attached an alternating instead of a direct current. This does away with a cumbersome and bothersome type of electrodes and permits the substitution of a simple and convenient form, since metallic connection may be safely made directly with the skin. The electrodes furnished with the apparatus, however, possess certain disadvantages because the pressure and probably the area of contact cannot readily be kept constant for any one *S*, especially not for any length of time. If between two readings on the apparatus *S* is required to perform exercises or to busy himself otherwise away from the apparatus, succeeding attachments of the electrodes cannot conceivably be made in such a way as to insure uniform pressure or equal area of contact throughout the series.

The author has consequently devised the electrodes shown in Fig. 2. To a bandage of soft leather (E) 15 cm. long by 6 cm. wide are attached two straps (C) 12 cm. long by 1.5 wide, which fit into buckles (B) attached to the opposite end of the bandage by short straps extending out about 4 cm. from that

end. The long straps (C) are sewed on the under side of the bandage (as illustrated) at a point about 3 cm. back of the free edge of the bandage at E. This allows a certain amount of overlapping of the edges of the bandage when accommodated to small hands. The bandage is cut out at D to fit around the thumb.

G is a flap sewed on the bandage at one end with a free edge at the other end. Between it and the bandage a piece of sponge rubber, about 1.5 cm. thick in the middle and tapering towards both edges in the lengthwise direction, is inserted. The metallic electrodes consist of very thin flexible copper 32 mm. square (A,A′) with rounded corners, mounted about 3 mm. apart and rather nearer the upper edge of the flap to bring them into a central position on the palm when the bandage is turned over face down on the skin. They are attached by

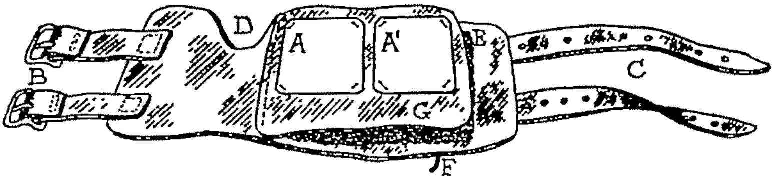

Fig. 2. New Electrode for the Hathaway Galvanic Reflex Apparatus

means of waxed linen thread at the corners. F shows one of the connecting leads of fine woven copper wire, filled with solder at the end to prevent fraying. These emerge through slits in the bandage and serve also to keep the rubber sponge in place. The area of the electrodes equals that of the electrodes furnished with the apparatus.

When the bandage is strapped to the hand with electrodes resting on the palm it is comfortably worn even when the straps are fastened tightly enough to insure a full contact. The rubber sponge then acts as a cushion. The usual process of wiping both electrodes and the palm of the hand with ethyl alcohol is followed. After a few minutes a saturated condition of humidity, due to natural perspiration from the palm, is reached and maintained.

The wires to the apparatus terminate in clips which are fastened to the leads from the electrodes. This allows *S* to move about between successive readings without disturbing the original area, place, and pressure of contact. The bandage has been tried out with success and is adjustable to the smallest as well as the largest hands found among students in the laboratory.

A Tridimensional Maze

By D. P. Boder, Lewis Institute, Chicago

The tridimensional maze described here was devised for the study of motor learning. It is made of 3/8-in. brass pipes—ordinary gas fixtures—which are screwed and soldered together in the form shown in the accompanying diagram (Fig. 3). The pipes are carefully fitted so that the joints are smooth on the inside. A 3/16-in. steel ball-bearing may therefore be rolled around freely within the system without catching or becoming lodged. Openings are left, through which the ball may fall, at A, B, C, D, E, and F.

S drops the ball at A and tries by tilting and tipping the maze in this and that direction to 'run' the ball through the pipes and to bring it out at some one of the holes previously designated as the goal. Problems may be set of varying degrees of difficulty. *S* may be instructed to roll the ball out at B—a simple task requiring only a few movements; or he may be instructed to roll the ball out at C, D, E, or F—tasks of increasing difficulty, requiring more and more carefully executed movements and delicate adjustments. *S* must know in the more complex problems where the ball is within the maze, and also how to coördinate his movements so as to avoid the culs-de-sac, *i.e.* the wrong tubes and holes.

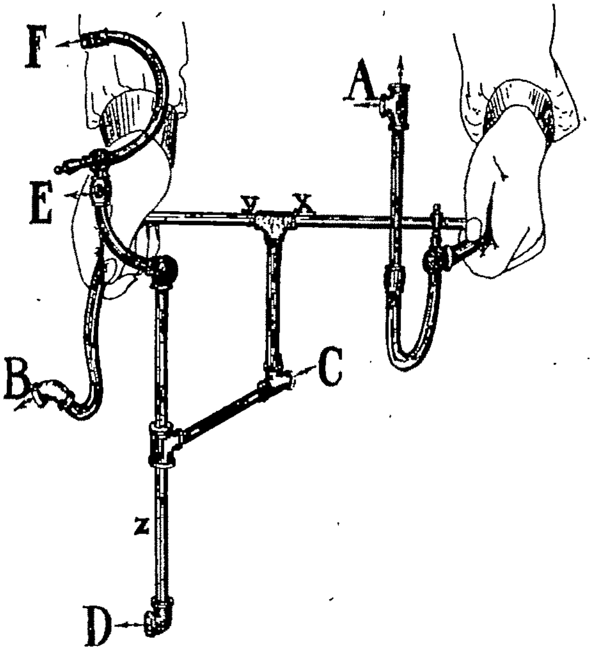

Fig. 3. Tridimensional Maze

Passage of the ball into the wrong tube may be corrected, but out of one of the wrong holes means starting again at A. Performance may be measured (1) by the number of attempts made to attain success, (2) by the time required for the completion of the problem, or (3) by a combination of these two methods.

The maze is so constructed that it may easily be modified. *S* may be required to alter the position of his hands in holding the apparatus, or sections of pipe may be added, or subtracted, or interchanged. Pipe z, for example, may be removed and placed at F, or pipes x and y may be interchanged. Any change in the apparatus, either in the position of *S*'s hands or in the form of the maze, causes a shift in the center of gravity and necessitates relearning. The interchange of pipes x and y, since the ball is to be placed in the maze at A, changes also the direction of the primary 'run' and thus further breaks up the kinaesthetic pattern.

Demonstrational and Experimental Devices

By Albert Walton, University of Washington

I. A Vertical Maze for Lecture Room Demonstration

The need for ocular group-demonstration of an animal in the various stages of learning has been met by the construction of the *vertical maze*, the essential items of which are shown in Fig. 4. It is 44 in. high and 42 in. long and is built of ½-in. pine. Since the pathway of the maze is only 4 in. deep and enclosed in screening with a ½-in. mesh, the progress of the animal is visible from start to finish and from all parts of a large hall. We have used it on an elevated stage before classes of five hundred students. The revolving cage is weighted so that an opening to the wheel is directly before the animal. The exit from the wheel is at the top of the other side. The rat turns the wheel through a half-revolution and leaves as soon as the exit has come down to floor level, when the weight returns the cage to its former position ready to admit another rat.

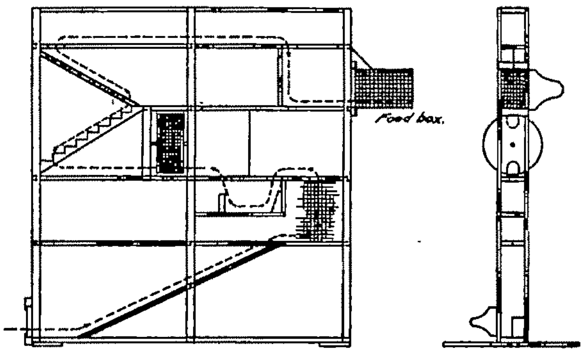

Fig. 4. Framework of Vertical Maze
Covered with a 1/2 in. hardware cloth. Dotted line shows true path to food box.

The food box is a wire cage hung on a maze by a single hook and is so long that when the rat goes to the outer end for the bait a door can be closed behind him, the cage lifted off, and the rat returned to his nest box. A trained rat will make the run in about 10 sec.

After two or three trained rats have run the maze, two or more naïve rats are introduced to find their way as they may. They seldom reach the goal during the lecture period, illustrating in their behavior all of the factors which have been treated abstractly in text and lecture.

The training of the rats for demonstration purposes involves little expenditure of time or energy. The maze is taken to the animal laboratory a week before the demonstration and is placed so that the rats can enter it directly from their nest box. By placing all food which they receive during the week in the food box of the maze while keeping the drinking water at the nest box their frequent passing from end to end of the maze is insured and they quickly acquire a mastery of the true path.

For cleaning purposes and to obviate the necessity of waiting for the naïve rats to solve the problem, one side of the maze is made removable, the screen being tacked to a frame instead of to the maze itself. The whole maze is light enough to be easily carried by one man and as it is a self-contained unit it requires no assembling but is always ready for use.

II. Conditioning Illustrated by an Automatic Mechanical Device

For purposes of demonstration in introductory psychology we have devised and constructed a self-contained, self-acting mechanical toy which completely illustrates the setting up of a conditioned response to a substitute stimulus.

The operation involves merely the bringing of a toy rabbit (substitute stimulus) into the 'field of vision' of a small doll which thereupon raises both

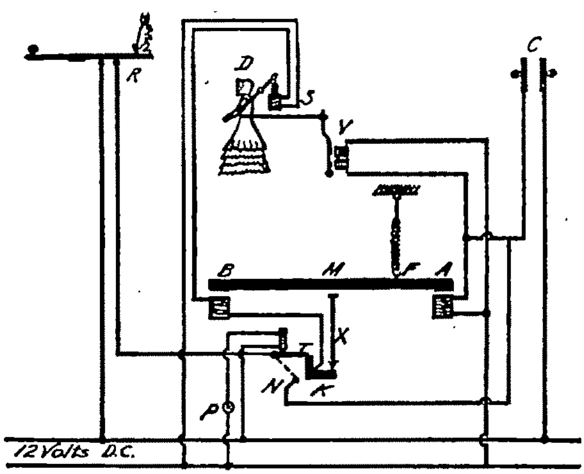

Fig. 5. Wiring Diagram for Conditioned Reflex Machine

arms toward the rabbit as an 'approach reaction.' If, on the other hand, a loud noise (original stimulus) be caused by banging together two blocks of wood the doll is caused to tremble violently. The doll follows the Pavlovian dictum that no conditioning takes place when the substitute stimulus is presented after the original or adequate stimulus. If the loud sound be made first and either discontinued or allowed to persist and then the rabbit is presented, no conditioning takes place. Even if the doll is still trembling when the rabbit approaches, the trembling is somewhat reduced (probably by reason of the 'distraction' involved), but there has been no conditioning. If the rabbit be again presented alone the old approach, reaction is made just as before. If, however, while the approach response is being called out, the loud sound be made, the arms fall to the side and trembling ensues. The rabbit being withdrawn and presented again now elicits no 'approach response,' but does bring on the fear reaction of trembling. Fortunately, unconditioning can be effected simply by pressing a push-button.

Only the rabbit and the doll appear in the performance, the synapses and ganglia involved being behind a partition. The effect on the class before which the demonstration is made is one of pleased surprise which aids in fixing the principles involved in their own response mechanisms by virtue of the very method which the device illustrates.

The mechanism involved is shown with all wiring connections in Fig. 5. The central device is the horizontal arm *BMFA* which is balanced on the floating, spring-suspended fulcrum *F*. When the loud sound is made by the two blocks at *C*, the circuit is closed through the vibrator *V* and the magnet coil under *A*. The doll trembles and arm *BMFA* is tilted by being drawn down at end *A*. End *B*, due to the unequal length of the two arms BF and FA, is raised out of the field of the magnet under *B* so that if the rabbit *R* be moved, in such a way as to close the contact under it, magnet *B* has no effect.

If, now, instead of presenting the stimuli in this order, the rabbit be moved toward the doll first, the contactor on the under side of the slide on which the rabbit is mounted closes the gap at *R* and current is sent through the trip *T* and the bell-crank *K* to the magnet under *B* and the solenoid *S* in series with it. End *B* is drawn down and the doll's arms are raised toward the rabbit.

Due, however, to the shortness of lever arm *FA*, the end *A* is not raised out of the field of its magnet, so that, if the blocks be now banged together at *C*, magnet *A* will draw its end down as before. With either end of the bar up and the other down its center remains stationary or may be slightly raised. In any event it does not touch the tripping bar *X*. If both ends are drawn down at once, as is done when *B* is depressed first, the mid-point *M* is drawn down on *X*, which is depressed enough to move the bell-crank *K*, allowing the trip *T* to drop off the bell-crank down onto a new contact *N*. Current from *R* is now diverted from *B* and *S* to *A* and *V*; the doll drops its arms and begins to tremble. From this event on, either clapping the blocks at *C* together or closing the rabbit contact *R* will actuate *V*, solenoid *S* which formerly raised the doll's arms being cut out of circuit by the breaking of the contact between the trip *T* and the bell-crank *K*.

Pressing push-button *P* resets the trip T and the apparatus is ready to repeat. In the apparatus as built, the bar *X* is dispensed with, the midpoint of bar *BMFA* striking the bell-crank *K* directly, an arrangement not easily shown in diagram since it is mounted at right angles to the plane of the sketch. The tension in spring *F* is regulated by thumb screw adjustment as is the movement of the end *A*.

III. Making the Ergograph Self-Recording and Integrating

The use of the ergograph is less common than it was in the early days of its development, not so much because it has yielded all the information which can be secured by its employment as because, in order to obtain further information, a cumbersome set-up and a wearisome process of observation and counting during the operation are required. To obtain a record of fatigue of the finger with a kymograph necessitates the lifting of excessive weights so that the fatigue may occur rapidly enough to be within the limits of the short kymographic record. The kymograph is separate from the ergograph so that each set-up is temporary and not easily portable; alignment is easily destroyed. There is also the necessity for counting both the number of strokes

shown by the curve as drawn and the number of times the meter tape has turne its length during the process. With light weights or strong fingers these tw items mount to values that may easily become burdensome.

To obviate all of these difficulties we have applied the attachments show in Figs. 6 and 7, using as a base the standard ergograph. The object was t make a self-contained portable unit which would record the strokes both as t total number and as to individual length and variation, and which would in tegrate the distance through which the weight has been lifted, without im posing any appreciable additional work upon the subject.

The devices used for accomplishing these factors involve three separat mechanisms; the stroke-counter, the graphic stroke-recorder, and the di tance-integrator. The ergograph with these attachments is mounted on board 4 ft. long and 11.5 in. wide, which also carries the usual arm-rest. Ex cessive swinging of the pendent weight when the periodicity of the strok

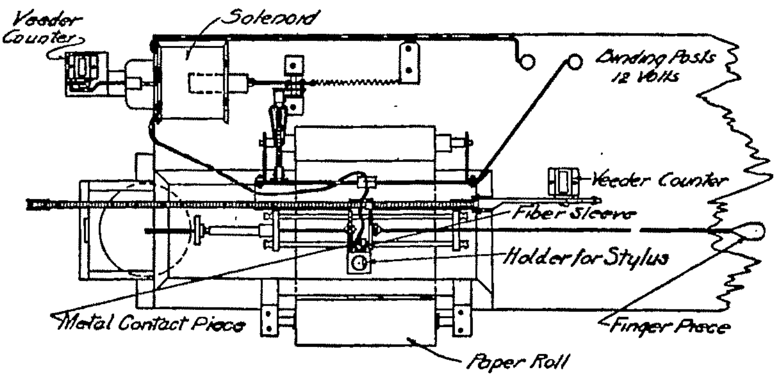

FIG. 6. SELF-RECORDING AND INTEGRATING ATTACHMENTS APPLIED T THE ERGOGRAPH (Plan View)

happens to synchronize with the normal pendulum cycle of the suspende weight is prevented by a cage or sort of 'elevator shaft' built at the end of th board as shown in the sketches. Affixed to the carriage of the ergograph is holder for a stylus fountain pen. The paper on which it writes is 6 in. wid and rests smoothly on the iron surface of the instrument as it passes from th supply roll on one side of the machine to the take-up roll on the other. A each stroke of the carriage and pen the paper advances about 1/10 in. so tha a continuous ink-drawn record is made showing the length of each pull and th variations in the series.

The take-up which advances the paper is a ratchet wheel 3 in. in diam. wit 100 teeth, which is operated through a linked pawl by the action of a solenoi with a movable core. This coil is of about 300 turns of No. 18 copper wir and operates satisfactorily on 8–12 v. Current is supplied to the solenoid eac time the carriage is pulled forward and only then. The movement of the cor is transmitted through a bell crank to the pawl on the ratchet, a check engagin the teeth to prevent backward motion. At the rear of the solenoid is a sprin

bumper to take up the jar. The motion imparted to this bumper is utilized to actuate a Veeder counter for recording the number of strokes made by the subject.

To provide one and only one impulse to the solenoid for each forward motion of the carriage a fiber sleeve is mounted on the round slide-bars which carry the carriage. Being inserted between the jaws of the carriage this sleeve must move forward and back with it but being light and loose and well lubri-

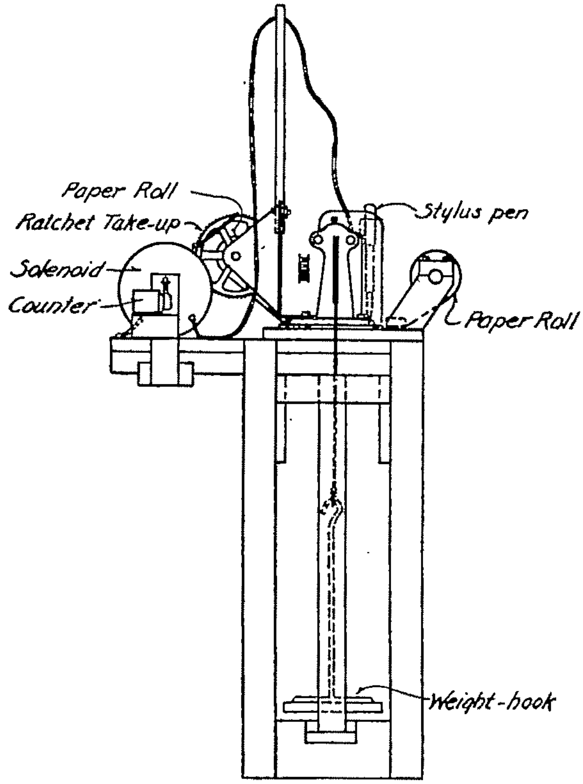

FIG. 7. STANDARD ERGOGRAPH WITH SELF-RECORDING ATTACHMENTS (End View)

cated it does not add appreciably to the work involved in operating the machine. There is play, or lost motion, of about 1/16 in. between the sleeve and the carriage. At one end of the sleeve is a metallic contact to which is soldered a flexible wire which runs to the solenoid. The other pole of the solenoid is wired to one of the two binding posts. From the other post a wire runs to the frame of the machine. Since the fiber is an insulator no current flows until the carriage is moved forward a sixteenth-inch, when contact is made between the carriage itself and the metal projection mounted on the sleeve. When this occurs a circuit is completed through the frame of the ergograph and the sole-

noid is actuated, moving the paper forward one notch and scoring one stroke on the counter. The circuit is broken as soon as the carriage starts back in answer to the pull of the main weights. Since this is always the last stroke which the machine makes, the solenoid is automatically out of circuit when the machine is not in use. When the circuit is broken a spring attached to the core of the solenoid pulls it back out of the coil, releasing the counter and resetting the ratchet ready for the next stroke. The carriage cannot be operated as rapidly as the solenoid works. The subject may therefore move his finger as rapidly as he desires without interfering with the take-up motion or the count of the strokes; nor is it necessary, so far as the mechanism is concerned, to employ any regularly repeated interval. The take-up works once for each forward stroke regardless of its length provided it is at least 1/16 in. The work of pulling the paper from the supply roll, under the two guides and the pen, and winding it up on the other side, and of actuating the Veeder counter is not done at the expense of energy on the part of the subject but entirely by current from the battery.

To record the number of times the meter tape is moved its own length a slight bump or projection is made at the zero mark. This offers no obstruction to the pawl which moves the tape on the standard machine but it is sufficient to displace a roller which is pressed up against the tape as it passes over one of the small pulleys on which it normally runs. In the machine we are using, this projection is made by rolling up the end of the tape into a cylinder about 1/8 in. in. diam. and sewing it down firmly to the tape itself. When the tape has gone around once, this bump depresses the roller which is mounted on the short end of a rocker-arm, the other end of which is connected to another Veeder counter so as to move it forward one number each time the tape has advanced one meter. To prevent undue jar on the counter-arm the link connecting the rocker-arm to the counter is made a spiral spring of sufficient strength to operate the counter with an easy stroke, regardless of the speed of motion of the rocker arm. The work of operating this arm is, of course, not imposed upon the subject since the tape moves only on the back stroke under the impulse of the weights.

The whole device may be picked up as a unit and placed on any convenient table or desk, connected to the battery circuit and so be ready for use immediately without adjustment or alteration. In a series of experiments recently made in our laboratory this machine was used to record over 30,000 strokes without appreciable wear or depreciation.

APPARATUS NOTES

A Technique for Screened Observation

In the course of an experiment on the judgment of projected motion, we desired to screen *E* and yet permit him to observe *O*'s face and also a spot of light used as an indicator of *O*'s movements. A peep-hole was unsatisfactory due to its small field of vision.

A window 6 × 6 in. square, and covered with a perforated tin plate, however, proved very satisfactory, giving *E* an adequate visual field and yet preventing *O* from obtaining any visual clues from the movements of *E*. In fact, several *O*s stated that they were rather mystified as to how the observation had been accomplished.

The material used was a thin tin plate, perforated with holes 1 mm. in diameter and 1 mm. apart, the surface toward *O* being bright, and reflecting the light from a 100-w. bulb. Such plating is commonly used for shielding radio tubes. This perforated grid presented a bright surface toward *O* while the space behind the screen was slightly darkened by covering an adjoining window. This arrangement, however, still allowed *E* ample light. The only defect discoverable was a slight reflection if *E* wore glasses and placed his face very close to the window. This defect is easily remedied by placing the light source to one side and close to the screen.

The technique could easily be applied to windows of larger area, when a greater field of observation is required.

Ohio State University T. W. Forbes

Records of Mutilated Speech and Music

While researching upon telephonic reproduction, the Bell Telephone Laboratories have made a number of phonographic disk-records to demonstrate changes in vocal and instrumental sounds due to the elimination of one or more partial tones. Band filters are used. Vowels and notes are sounded with and without the fundamental and other low partials, and also with and without the higher overtones. Other records exhibit the damage to connected speech and music through the shortening of the acoustical spectrum above and below. One of the most instructive of the records produces in direct sequence a given sound with all components above 1000 d.v. removed and the same sound with all components below 1000 d.v. removed.

The records make useful demonstrations of the dependence of complex sounds upon the constituent parts. The Cornell Laboratory has also used them in its Undergraduate Laboratory for instruction upon composition and clang color and for memorizing without intent to learn. The latter experiment rests upon the fact that certain sentences are repeated over and over by the disks without calling attention to their verbal significance.

M. B.

A Standardized Hypnotic Technique Dictated to a Victrola Record

The apparatus consists of a double-faced (12-in.) Victrola record. The technique demonstrated is the usual 'sleeping' method employed by Bernheim and the Nancy School.

On one side of the record the operator simply talks of sleep in the usual way. He dwells upon the subject's inability to move the right arm and to open the eyes. This side of the record requires 4 1/4 min. It is given first. Just before it ends there is a pause of 5 sec., after which the record wakes the subject by counting ten.

The second side of the record begins also by talking sleep and inserts a test of inability to bend the right arm. It also transfers the rapport from the record to another operator who is present, provided the subject has given his written consent but under no other conditions. If the rapport has not been changed to the other operator, there is again a pause of five seconds and the record again wakes the subject.

The interesting features in this record are: (1) it enables hypnotic experiments to be carried through on a standardized technique; (2) the record auto-

matically wakes the subject in the event that no operator is present; (3) should this other operator be using the record for hypnotic purposes, he simply turns over the record at the pause mentioned in part one and continues with the other side, taking over the control as described for the second part and stopping the record before it wakes the subject (it might be advisable to insert a pause of two or three minutes between the two halves of the record); and (4) therapeutic suggestions are interposed with the usual suggestions of sleep in such a way that the use of the record should not lead to bad results. The entire record is simply an effort to produce a standardized laboratory technique for experimental purposes.

Colgate University G. H. Estabrooks

Lacquers and Celluloids for Colored Surfaces

Mr. Edwin Newman reports from the University of Kansas the successful use of colored lacquers upon the metallic buttons which certain perimeters employ as colored objects. "A large variety of these lacquers have appeared on the market in colors that are both bright and saturated. Sufficient material for a hundred buttons was obtained at a trifling cost and it required but an evening to apply the lacquer. They have been used with complete success. The advantages of the lacquer are at once apparent. The colors do not fade; they may be cleaned in an instant with a damp cloth; if allowed to dry and to harden, they are practically indestructible; they are cheap; and they are easily applied. They could probably be applied also to colored papers."

The Marietta Apparatus Company has prepared celluloid disks for color mixers and other apparatus by applying a color pigment to the back surface. The colors are evenly put on and they seem permanent. As the operator's hand does not touch them, they do not soil. Possibly Newman's colored lacquers would usefully replace the ordinary pigments upon the celluloid.

M. B.

An Aid for Steady Visual Fixation

As an aid to stabilizing visual fixation the Cornell Laboratory has hit upon the use of a glass mirror. The mirror is placed in the line of regard, and the pupil of the reflected image of the eye is taken as the fixation-point. Since the mirror doubles the distance of the fixated object, it should be placed at half the focal length desired; *e.g.* if the focal distance is 1 m. the mirror should be placed 50 cm. away from the eye.

In monocular fixation, small mirrors (3-5 mm. in diam.) should be used. The exact size depends upon the focal distance. The mirror should be large enough in every case to include in its reflection the iris; for the iris serves as the background of the pupil, and thus of the fixation-point. In binocular fixation the mirror may be large enough to reflect the image of both eyes, the pupil of one being then selected as the common fixation-point; or a small mirror may be used and so placed in the line of regard as to reflect the image of but one eye, the pupil of which is taken as the fixation-point of both eyes.

By using the pupil as a fixation-point, *O* is able to 'hold' his eye, and so to report with a high degree of assurance any slip in fixation. This aid to fixation is limited, however, to light adaptation, because there must be sufficient light for the reflection of the eye in the mirror.

K. M. D.

NOTES AND DISCUSSIONS

Color Problems: The Divergent Outlook of Physicist and Psychologist

Jaensch's incorporation of the after-image in his system, the *Erscheinungsweise der Farben* of Katz, and the appropriation of visual contrast by exponents of *Gestalt* have brought color problems again to the fore in our laboratories. Recourse to the retina in the interests of certain psycho-physiological enigmas, *e.g.* the neural correlates of qualitative and intensive differences (Adrian, Troland, Hecht), has motivated other studies; while innumerable problems soluble only in psychological terms have been turned up of late by the illuminating engineer. Frequent discrepancies in the results both of these and of earlier researches are, in the opinion of the writer, due not merely to divergent outlook and method on the part of the experimenter but also to idiosyncrasies in the visual systems of the various observers.[1] In the interests of laboratory technique no less than of theory there is need of a better understanding of aberrant cases.[2] If, as the work of Hess and others renders probable, numerous gradations or variants exist both between the 'normal' and the 'color-weak,' and between the latter and the partially or totally 'color-blind,'[3] psychologically valid methods or the scaling and rating of color vision in both normal and deficient cases should be formulated, to be applied to observers in any given visual problem.

Yet when a case of unusual color vision (holding, perhaps, the key to numerous deviations from the norm) falls into the hands of a psychologist unskilled in the specialized technique of visual research or unversed in the vast literature of this special field, the would-be investigator faces a dilemma. Time is precious, and while one is orienting oneself in the problem and assembling suitable apparatus, the subject is apt to slip beyond recall. In the matter of color problems, moreover, the physicist was first in the field. The psychologist, as a late entrant, not only reaps the profits of his predecessor's labors, he suffers also the handicap of traditional misconceptions and is balked by the inevitable distortions arising when a function so complex as vision is attacked from the angle of the most abstract of sciences.

I cite an instance. Late in the spring of 1928 a case of congenital R-B-G blindness, with no detectable pathological symptoms, was brought to the notice of the writer. The retention of the primary *yellow*, with the loss of *blue*, (an

[1]For evidence and for partial recognition of this factor by physicists see: E. C. Crittenden and F. K. Richtmyer, An 'average eye' for heterochromatic photometry, *Bur. Stand. Sci. Papers*, No. 299, 1917, 96; I. G. Priest, The spectral energy required to evoke the gray sensation, *ibid.*, No. 417, 1921, 237, 252 f., 256 f,; K. S. Gibson and E. P. T. Tyndall, Visibility of radiant energy, *ibid.*, No. 475, 1923, 178 f.; and M. K. Frehafer, Preliminary note on after-images from stimuli of low saturation and short duration, this Journal, 41, 1929, 282 f. In the latter variations are reported in the hue of the Purkinje image correlative with pigmental differences in the observers.

[2]The growing importance of color discrimination in connection with traffic signaling, aviation, bacteriological tests, etc., furnishes an added incentive for research of this nature.

[3]C. von Hess, Farbenlehre, *Ergeb. d. Physiol.*, 20, 1922, 12 f., 19 f.

anomaly not easily reconcilable with the tenets of a 3-, a 4- or a 7-component theory) singled the instance out for more extended study. A rapid survey of the literature was undertaken for accounts of similar cases, for light on the possible rôle of yellow pigmentation in reducing the effectiveness of short-wave stimuli (blue), and for aid in settling upon the more important measurements and suitable apparatus and technique. Complete files of ophthalmological journals were not immediately accessible, and no succinct and reliable bibliography of the limited number of studies available in psychological periodicals was at hand.[4] Recourse was therefore had to such works of reference as J. H. Parsons' *Introduction to the Study of Colour Vision* (1915 ed.); J. von Kries' article on vision in Nagel's *Handbuch der Physiologie des Menschen*, III, 1905; and H. von Helmholtz's *Physiological Optics* (Amer. ed., 1924). G. E. de Schweinitz's *Diseases of the Eye* (1924 ed.), F. W. Edridge-Green's *Physiology of Vision* (1920), and M. Collins' recent book on *Colour-Blindness* (1925) were also consulted on special topics.

Misconceptions and false leads rising out of certain of these sources obstructed plans and retarded a grasp of the essentials of the problem until too late to apply certain significant measurements (dark adaptation and the exact comparison of center and periphery). Hence the following cautionary note to others who may find themselves in similar straits. Difficulties due to differences in point of view and terminology are perhaps inevitable; but erroneous citations and reports of experimental data in standard works of reference are more misleading because long undetected. The substitution of *a priori* deduction or stock interpretations for direct observation, and the wide currency of inferences based upon scantily verified data and unsafeguarded methods likewise call for protest. A comparison and critique of the divergent outlook and methodology of physicist, physiologist, and psychologist may clarify discussion.

Description of cases. First as to terminology and theoretical bias. Search of the texts and records for a similar case of gray-yellow vision revealed a literature obsessed by obsolete distinctions,[5] derived from the physicist's 3-component color-mixing theory. Cases are listed as trichromatic, dichromatic and monochromatic, terms admittedly non-descriptive, having reference to color-mixing equations merely. It is sheer accident that the second, *dichromatic*, squares with the current view of the commoner form of color deficiency as red-green blindness or *blue-yellow* vision (in which, as Hering demonstrated, all the spectral qualities can be duplicated through the use either of a 'yellow' stimulus

[4] S. P. Hayes' résumés of the literature on color defects in the *Psychol. Bull.* have since proved invaluable (8, 1911, 86-89; 12, 1915, 108-11; 13, 1916, 131-34; 16, 1919, 138-42; 23, 1926, 547-74). M. Collins' bibliography in *Colour-Blindness*, 1925, 227-34, while fairly full on the early history, is inadequate with respect to later experimental studies.

[5] Since the publication of the Report of the Colorimetry Committee for 1920-21 (*J. Opt. Soc. Amer.*, 6, 1922, 527-96), a slight improvement in terminology is noticeable, *i.e.* there is a tendency to replace *monochromatism* with *achromatopsia*, to separate brightness or brilliance from saturation, and to distinguish both from luminosity or the physical energy of the source. Separate curves for brightness and chroma values will perhaps follow, and the joint contribution of chroma and brightness to lamprosity or visibility (*e.g.* in flicker or equality photometry) be recognized; though the old notion of brightness as the sum or quotient of chromatic values obstructs progress.

(about 589 mμ) or a 'blue' (around 480 mμ) either singly or in combination. *Monochromatic* is, however, merely a physicist's tag, signifying that in this type of deficiency any wave-length of light can be matched against any other. Actually the vision of the subject is *achromatic*, without any color quality whatsoever. No genuine *monochromasy* in the descriptive sense, is, for theoretical reasons (the compounding of white from chromas), admissible. If discovered it is quickly cast aside as 'pathological.'[6] The preliminary search for a parallel case of 'yellow' color vision therefore proved baffling; though a wider canvass of the literature (Hess, Stilling, Köllner), plus experiment on control cases, has convinced the writer that many such cases (probably congenital) do exist, however camouflaged by the antiquated nomenclature and test methods still current.

Furthermore, although the old Helmholtzian terms 'red-blind,' green-blind,' and 'violet-blind'[7] are virtually obsolete (authorities in all camps admitting that both the first two groups are *red-green* blind) these rubrics still persist among users of the Holmgren wool test and in the backwaters of college and university instruction. The correlative terms of the physiologist, moreover, (*protanopia*, lack or non-functioning of the first or R process; *deuteranopia*, of the second or G process; and *tritanopia*, of the third or V process) are equally vicious and misleading. Lesser workers in the field of physics or physiology, accepting these terms at their face value, regard the retina as a simple sum in

[6]This is true even for Edridge-Green's 6-7 component theory, which assumes, *Gestalt*- or figure-and-ground-wise, that there must be a second hue, preferably at the remote end of the spectrum, in order to differentiate out the first. F. Allen, to be sure, representing the 3-component theory in its older, cruder form, attempts to find indirect evidence (by way of the selective after-effects of fatigue on flicker in different regions of the spectrum) for all seven possible combinations and eliminations of the three components (*i.e.* not only for R, G, V and total color-blindness, but also for R-V, G-V, and R-G); succeeding to his own satisfaction in the case of all but the last-named, V vision. His conclusions are vitiated, however, by various irregularities in his procedure and the make-up of his curves.

[7]'Red,' 'green,' and 'violet' in these compounds are sometimes taken as *stimulus-terms*, implying absolute blindness to the three corresponding sets of wave-lengths, or regions of the spectrum (in contradistinction to Hering's designation 'red-green blind,' which implies a gap in color-experience merely, *i.e.* the lack of the corresponding *color-qualities*). Again, they appear to refer simply to the absence or lack of function of the 'red,' 'green,' or 'violet' nerve-fibers of Young. Undoubtedly, in common usage, they connote an uncritical confusion of stimulus with sense quality, such as apparently inspired Brewster's curious notion of the three kinds of light, each representing all degrees of refrangibility, each coextensive with the spectrum, and superposed on the other two (cf. D. Brewster, The triple spectrum, *Athenaeum*, 1855, 1156).

In the 14th edition of the Encyclopedia Britannica (1929) in the article on Vision, J. H. Parsons admits: "It was, perhaps, natural that the three groups of partially colour-blind should be denominated red-blind, green-blind, etc., by upholders of the Young-Helmholtz theory. It has, however, had the most disastrous results, and has led to endless ambiguity. For in the obvious meaning of the terms these people are not red, green, or violet blind. They simply have different responses from the normal when their eyes are stimulated with these particular regions of the spectrum. The attempt of Von Kries to eliminate ambiguities by calling the conditions protanopic, deuteranopic and tritanopic failed in its object and seems only to have infuriated opponents of the Young-Helmholtz theory."

addition, and record their cases in terms of the hypothetically remaining pair of processes—blue-violet, or red-violet, as the case may be. Extraordinary plates, purporting to represent the color worlds of the two types—protanopic and deuteranopic—have been constructed along these hypothetical lines and even found their way into print.

Where arbitrary deduction is allowed to supplant in this fashion exact observation, yellow of course drops automatically out of the records. A derivative color, according to Young-Helmholtz premises, it disappears of necessity when either of its components, red or green, is lacking. A virtual conspiracy, therefore, exists against the admission of this quality into case descriptions; although, in the face of accumulating evidence, Parsons and others now concede, parenthetically, that it is perhaps the dominating hue in the spectrum of the commoner forms of color-blindness. The origin and history of this minor skirmish between exact observation and theoretical bias, as found in some of the older texts (*e.g.* Jeffries),[8] are illuminating. Dalton, Pole and others, though pronounced now red-blind, now green-blind (*i.e.* possessed only of G-V or R-V vision) by ardent advocates of Young or Helmholtz, stood out stoutly for the dominance of yellow in the warm end of their spectra; the dispute finally terminating in decisive evidence furnished by the discovery of cases of monocular and acquired color-blindness. Helmholtz himself in the second edition of his *Optik* (1892) abandoned the earlier *Ausfall* or *reduction* theory, accepting from Fick and others the suggestion that no element is missing or out of function in partial color-blindness. Rather the stimulus curves for the R and G elements have become identical, so that the two are always correlatively stimulated, evoking the derivative quality yellow instead of red or green.[9]

The complication thus introduced, however, is tantamount to positing a true 'yellow' process (a totally new mechanism, that is, susceptible to a wider range of wave-lengths). It detracts, therefore, from the vaunted simplicity and economy of the theory, and is consistently ignored by the majority of physicists and by numerous physiologists as well. The latter, if they worry at all over the refractory facts of color-blindness, are inclined to refer them to some mythical derangement of cortical centers—forgetting, apparently, that the fovea has bilateral representation, and that any malformation or lesion, if it is to explain binocular color deficiency, must occur symmetrically in the two hemispheres. Ladd-Franklin's retinal solution of the difficulty, referring the retention (or emergence) of the fourth component (yellow) in the periphery and in color-blindness to lack of development or reversion of the color molecule, is a shade less intricate. Her reds and greens, however, are not the vanishing pairs of the periphery or of color-blindness. When shifted to represent the latter, they yield not yellow but gray on mixture. Nor does she attempt to grapple, except pictorially and verbally, with the shortened spectrum and shift in luminosity of protanopic or 'protoxanthic' vision.

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[8]B. J. Jeffries, *Colour Blindness: Its Dangers and Its Detection*, 1879. See also M. Collins, *Colour-Blindness*, 1925, 3 f., for a brief historical survey of this dispute.

[9]See J. von Kries in Nagel's *Handbuch*, III, 1905, 203-4. Von Kries himself, however, refuses to subscribe to the change, aware that it entails more serious complications than those that it is designed to obviate.

A third stumbling-block in the way of the psychological reader, obscuring evidence and precluding comparison of data, is the physicist's sheer inability, on theoretical or other grounds, to distinguish saturation from brightness differences.[10] This not only renders dubious results obtained by the *Eichung* of the spectrum (the matching of any portion by mixture of wave-lengths from two others), or by the Rayleigh equation (the mixing of 'red' and 'green' to match yellow), but it also falsifies the stock description of 'protanopic' and 'deuteranopic' vision. Accounts, which are apparently authentic, are on record of cases of red-green blindness in which the long-wave end of the spectrum is not merely colorless but darkened, *i.e.* the range of effective light stimuli curtailed. Unable to distinguish between *dulled* and *darkened*, the formula-ridden physicist (and others) is moved to set these cases over against the commoner ones, construing deuteranopia in parallel with protanopia, and arguing that, since red is darkened for the latter, green must be darkened for the former. The distinguishing mark of 'deuteranopia' therefore becomes the matching of greens with darker tones than those demanded by the normal eye.[11]

A milder but equally misleading form of this delusion is widely current, the belief that the partially color-blind distinguish reds and greens primarily by brightness. Only a wide acquaintance with the literature and a careful study of cases will free the investigator from this obsession, which tends continually to snarl up procedure and confuse interpretation. Actually the common color-blind *O* appears to distinguish red from green primarily by saturation. 'Reds' are relatively highly saturated yellows (somewhat darkish and approaching golden-brown); 'greens' are low saturations (tannish). A shift in saturation without change in brightness suffices to confuse the *O*, and lead him to alter his judgment from 'red' to 'green,' or *vice versa*; whereas a considerable range of brightness would not shake his confidence.

Pigmentation and types of color deficiency. Search for the facts relative to macular pigmentation and its possible bearing on unusual types of color vision proved equally baffling. The passages referring to visual purple and macular yellow in Parsons' widely quoted *Introduction to the Study of Colour Vision*

[10] J. H. Parsons (*Introduction to the Study of Colour Vision*, 1924 ed., 31 f.) is plainly puzzled by the use of the term saturation (or *Sättigung*) in the literature. His only definition is in terms of purity or *degree of mixture with white light*. H. Köllner, usually fairly alert to psychological distinctions, in *Die Störungen des Farbensinnes*, 1912, 2, also defines saturation by reference to transitions of color tones toward white; brightness (*Helligkeit*) by reference to transitions toward black!

[11] *E.g.* De Schweinitz, *op. cit.*, 513, states (on the authority of W. Thomson) that the red-blind confuse light red with dark green and black; the green-blind, dark red with light green (a stereotyped opposition fundamental in Holmgren wool-test procedure). In Monroe's *Cyclopedia of Education*, 1911, R. P. Angier, who worked with Nagel, makes a similarly misleading statement to the effect that "all reddish colors to the red type look darker than greenish colors, whereas for the green type the greenish colors are darker and the reddish lighter," leaving the reader doubtful whether these brightness alterations represent departures from normal vision or from the contrasted type. One suspects, however, that the opposition is largely inferential, the cropping out of the old logical fallacy of contradictory opposites, with little factual basis. Color-mixing equations afford little clue as to which of the two components is *brightened* or *darkened*.

(1915 ed.) were especially perplexing. Not until a year later when Hering's original articles in *Lotos* for 1884 and 1885, Von Kries' Über Farbensysteme in the *Zeitschrift* for 1897, and Trendelenberg's researches on visual purple were available, was the unreliability of Parsons' account thoroughly apparent.[12] For example, the curves for scotopic luminosity and for the bleaching value of different wave-lengths for visual purple are interchanged, and Trendelenberg's results for the rabbit wrongly referred to the frog (p. 14)—errors corrected in part only in the 1924 edition. As for Hering's experimental study *Über individuelle Verschiedenheiten des Farbensinnes*,[13] ascribing the limiting types of R-G blindness (*i.e.* the 'red-blind' and the 'green-blind' of the older theories) to differences in macular or lenticular pigmentation, this is twice wrongly transcribed by Parsons (p. 259 f.). For example, Parsons' statement, "Herr B, regarded as yellow-sighted, with little pigmentation, Herr S, relatively blue-sighted, with greater," exactly reverses the relationship posited by Hering—to the confusion of the reader, already puzzled by the paradox that the wave-lengths absorbed by 'visual purple' are enhanced in effectiveness, those absorbed by 'yellow pigment' rendered less effective as visual stimuli. Hering is further erroneously quoted by Parsons as declaring that "his own extra-foveal matches stood to foveal as deuteranopic to protanopic"—an exact reversal of the facts.

Parsons also states that Hering found that his 'anomalous trichromates' (examined in parallel with an array of divergent dichromates) fell into two groups, one making the luminosity match of spectral red as 1.15:1, the other 7:1 For the correction of this peculiar twist to Hering's data, the reader is referred to the original (p. 162 f.). Hering found his small group of 5 apparently continuous; he was, moreover, concerned with the yellow valence, not the luminosity of red (two things not even approximately identifiable, except for a physicist convinced that luminosity is merely the summation of color valences, and unable to distinguish chroma from brightness). Finally, he was not matching but *mixing* red with blue to get a pure (*i.e.* non-yellowish) primary. The figures represent the necessary proportions of red to blue for the two limiting members of his group, Herr B (yellow-sighted) and Herr S (blue-sighted).

Additional misstatements by Parsons refer to Hering's choice of primaries (declared by Parsons to be 'bluish red' and 'bluish green'); to the wave-lengths of the corresponding stimuli, and their varying position for protanopes and deuteranopes—all in total disregard of Hering's choice of 'pure' (*rein*) red and green (crimson and verdigris—neither bluish or yellowish), and his refusal to fix stimulus relationships exactly, owing to the difficulty of ruling out adaptation and individual variation in pigmentation.

Though some of these errors are doubtless undetected slips of the pen, it is difficult to avoid the conclusion that Parsons either never consulted Hering's original articles, trusting to inferior transcripts, or that he was too indifferent to the 'opponents' theory of color vision to clarify his notions of Hering's postulates and procedure *versus* the deuter-protanopia distinction. The effect of these errors and inaccuracies is magnified by their uncritical acceptance and widespread citation.[14]

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[12]Some of these errors are perhaps derived from similar errors in Von Kries' article on vision in Nagel's *Handbuch*.

[13]*Lotos*, n. F. 6, 1884-5, 142-98.

[14]Cf. *e.g.* Collins, *op. cit.*, 44-45.

Further highhandedness and bias in dismissing Hering's use of variable pigmentation (or blue- and yellow-sightedness) as an explanatory principle in partial color-blindness appears in a later passage,[15] where data from Von Kries' *Über Farbensysteme*[16] are quoted. In the article cited, Von Kries reports a long and varied series of observations with spectral mixtures, attacking Hering's contention that 'protanopic and deuteranopic dichromates' represent (in analogy with 'protanomalous and deuteranomalous trichromates'), not two disparate reduction forms of normal vision, but a single group, characterized in common by a lack or weakness of the R-G mechanism, and distinguishable (if at all) by anomalies of pigmentation which may or may not occur in continuous series. The *'cold curves,'* however, reproduced by Parsons as rebuttal evidence, and apparently demonstrating a greater divergence in sensitivity to short wave-length stimuli (*i.e.* those affected by yellow pigmentation) between two protanopes than between protanope and deuteranope, are those derived from Von Kries' preliminary observations, the erratic and unreliable character of which is obvious upon mere inspection.[17] Later and more exactly standardised determinations showed, in fact, far less significant deviations. Furthermore, extraordinary irregularities of procedure render the *'warm curves'* reproduced for the two sets of *O*s non-comparable (except for confirmed Helmholtzians of the old order, who identify all the long-wave third of the spectrum with the 'red' process). For example, the data for these 'warm' curves, designed to show the shift of the peaks of stimulus sensitivity in deuteranopia toward the long-wave end of the spectrum, in protanopia toward the short-wave end (also the higher maximum of the latter),[18] were secured by using as a standard for comparison a stimulus wave-length of 651 mμ (orange-red) for the first group, of 589.1 mμ (yellow) for the latter!

Other observations cited by Parsons are designed to demonstrate the wide separation of 'protanopes' from 'deuteranopes'; the former constituting a homogeneous group, characterized by relative insensitivity to 'red' stimuli, the wave-lengths for which, as Von Kries points out, are precisely those unaffected by the variable yellow pigmentation used by Hering to explain deviations within his unified 'red-green-blind' group. If, however, we supplement the variations in 'visual yellow' posited by Hering with variations in 'visual purple,' this objection vanishes. It is not improbable, *i.e.*, that 'protanopic' (or, better, *scoterythrous*) vision, either in partial color-blindness or in color-weakness, is characterized not merely by diminished yellow pigmentation, but also by an *increase in the amount or effectiveness of 'visual purple.'* This pigment (really *rose*) with a maximal absorption somewhere between 511 and 554 mμ, by its selective reflection of long wave-lengths (along with a few of the shortest) may

[15] *Op. cit.*, 168 f. in the 1915 edition; 176-81 in the 1924. For an even more offhand and unfair statement of Hering's position, see the *Ency. Brit.*, 14th ed., article on Vision, p. 210.

[16] J. von Kries, *op. cit.*, *Zsch. f. Psychol.*, 13, 1897, 241-324.

[17] A source admittedly weak in high frequency radiations was used. Moreover, each of these widely circulated curves is based apparently upon a set of single unchecked observations for one *O* only; in all, data for two protanopes and two deuteranopes are used! (*Op. cit.*, 252, 273.)

[18] Correlative, according to the Helmholtzian, with the dropping out of the green- and red-sensitive elements respectively.

account for the relative ineffectiveness of the lower frequencies, either as brightness or color stimuli. Hence the loss of the brightness and yellow valences of the long-wave end of the spectrum, superadded to loss of the red primary in the case of R-G blindness. Strangely enough, this explanation, suggested by the marked resemblance between the luminosity curves for the 'protanope' and for normal twilight vision (along with the loss of 'red' sensitivity in the latter), appears not to have occurred to Von Kries, whose name is associated especially with rod and visual-purple theories. Von Kries, however, has assigned to visual purple primarily a peripheral function, whereas the current drift of scientific opinion is against the rigid exclusion of rod and purple-pigment phenomena from the fovea; a shift in harmony with the above conjecture.[19]

Not only, therefore, does the physicist's rule of laboratory economy, the 3-component mixing-law, when metamorphosed into a physiological theory and grafted on the retina, work havoc with exact observation and description, lending itself too readily to *a priori* inference. It also appears linked with innumerable irregularities of method, which vitiate not merely the conclusions but also the raw data of its adherents. Naturally apparatus-minded, the physicist (and in his wake the physiologist), having carefully calibrated his sources, is disposed to regard the observing *O* as a cipher, the retina as little more than a cover-glass interposed in the path of a light-ray. Not only, therefore, does he feel free to use *widely different wave-lengths for normal and comparison curves,* substituting at will an orange stimulus for a red or yellow, an olive for a green (Allen, Von Kries, Laurens and Hamilton),[20] but he is also *negligent in recording the exact retinal area stimulated* (foveal or parafoveal), thereby making it difficult to allow for the elimination of short-wave stimuli by macular stimulation, or of

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[19]Cf. F. W. Edridge-Green's *Physiology of Vision,* 1920, 43 f. Differences in pigmentation may of course be of pathological or accidental origin; but the suggestion that the tendency to excess in either pigment (yellow or purple) may represent a unit trait, separably heritable, with a survival or protective value relative to certain primitive environments (southern desert or foggy northland), yellow pigment absorbing excess ultra-violet radiations, visual purple sensitizing the eye to dim reflections, is not without factual support. The occurrence of yellow oil-drops in the retinae of hawks and falcons is suggestive. Also the hereditary transmission of non-pathological hemeralopia through several generations in families in southern France (Nettleship). Exact experimental demonstration is, however, as yet wanting.

[20]See H. Laurens and W. F. Hamilton, Sensibility of the fatigued eye to differences in wave-length in relation to color-blindness, *Amer. J. Physiol.,* 65, 1923, 569-584. In these and other recent studies, the derivative or secondary character of yellow is settled to the satisfaction of the experimenter by using long wave-length 'greens' and short wave-length 'reds'—both stimuli of a relatively high yellow valence (Allen, Laurens). Frequently a stimulus actually containing many 'yellow' rays is used (S. Hecht, On the binocular fusion of colors and its relation to theories of color vision, *Proc. Nat. Acad. Sci.,* 14, 1928, 237). The statistics collected, designed to deal a knock-out blow to the pretensions of yellow as an *Urfarbe* (the psychological brief for which is erroneously supposed to rest on its phenomenologically unitary character) frequently furnish data which with greater cogency and logic might be applied to the support of a 4-component theory.

long-waves ones by visual purple.[21] Thirdly, the precise conditions of *adaptation* or of *general illumination* during experimentation are often uncontrolled or unrecorded, also the exact duration of stimulation and of the interval between observations, or their order (from left to right of the spectrum). In the case of dark adaptation, there is frequent failure to take account of the selective absorption of visual purple. Slips of this sort vitiate many of the conclusions of recent work on color fatigue (Allen, Laurens and Hamilton). The factor of *simultaneous brightness induction* from the surrounding field, *e.g.* from the darkened walls of a tube or from an artificial pupil, although it has a differential effect on different hues, is also frequently overlooked. Finally, sweeping generalizations are based upon a *single unchecked or unrepeated set of observations* by a single *O* (this holds for much of the work of Von Kries and also of Allen).[22]

Experimental methods. Lastly, as to the search for specific methods of charting the capacities or limitations of the partially color-blind or color-weak eye.[23] The value of the much-advocated Rayleigh equation, *i.e.* the mixture of homogeneous red and green stimuli to match a homogeneous yellow (preferably with the aid of the Nagel anomaloscope), is virtually nil, except to adherents of a rigid 3-component theory. Actually, instead of working with two invariables, as the latter assumes, it deals with three unknowns. An overbalance of green in a given *O's* equation may mean not diminished sensitivity to that stimulus, but heightened sensitivity to red;[24] or merely a shift due to altered absorption in accessory pigments. Furthermore, the customary procedure of increasing intensity by widening either collimator slit (and thereby decreasing the homogeneity of the mixture or of the standard stimulus) introduces erratic changes

[21]Many irregularities in recent data obtained from 'normal' *Os* might be explained if the effectiveness of visual purple within the macula, especially under the 'mesopic' conditions usually prevailing in visual experimentation (cf. W. T. M. Forbes, An interference theory of color vision, this JOURNAL, 40, 1928, 17, footnote 31) were recognized, and its variability in different *Os* determined. *E.g.* the shift toward blue or violet with dimming of the stimulus area may possibly be a visual purple phenomenon (T. Karwoski, Variations toward purple in the after-image, this JOURNAL, 41, 1929, 625-36).

[22]Even the psychologist is not exempt from this shortcoming. The value of many of the determinations published by G. Rand and C. E. Ferree (*e.g.* Chromatic thresholds of sensation from center to periphery of the retina, *Psychol. Rev.*, 26, 1919, 16-41, 150 ff.) is lessened by the fact that they are reported for a single *O* only. This holds also for the results of Karwoski mentioned in footnote 21.

[23]The standard commercial tests (Holmgren, Nagel, Jennings, etc.) are all quite definitely constructed upon 3-component premises; the Edridge-Green lantern on a 6-7 component basis. None are designed or fitted for exact diagnosis, utilizing as they do too limited a range of hues and chromas. Their specific aim, indifferently performed at best, is the selection of the 'dangerous' color-blind. The Stilling confusion figures (based on Hering premises) provides, it is true, a fairly valuable preliminary. Unfortunately the usefulness of one of the best of recent modifications has just been jeopardized by the thoughtless publication by a psychologist (Miles) of a complete and easily memorized key.

[24]Hess, *op. cit.*, 16 f. For the recent use of this equation to indicate the range and distribution of color anomalies in 'normal' vision, see H. Köllner, *Die Störungen des Farbensinnes*, 1912, 52 ff.; M. Winfield and C. Strong, The Hering color-blind apparatus and the normal Rayleigh equation, this JOURNAL, 32, 1921, 425-8 (100 *Os*); and M. Collins, The Rayleigh color equation with rotating disks, *Brit. J. Psychol.*, 19, 1929, 387-93 (200 *Os*).

in chroma and procedure. Quite different chromas and brightnesses of the standard yellow may appear in the final matches; and, as a matter of fact, widely different equations are acceptable to the same *O*.[25] The equations, moreover, probably hinge not upon the production of a new quality, yellow (Helmholtz), but upon the summation of the yellow valences of the two stimuli (Hering); the yellow, masked by either saturated complementary (in normal vision), emerging into prominence with the balancing out of the latter through mixture.

Similar criticisms hold, *mutatis mutandis*, for matching by mixture over the spectrum as a whole. Differential sensitivity to wave-length also, as ordinarily determined,[26] with no attempt to distinguish hue, chroma, and brightness differences, has no significance to the psychologist. Two other measures commonly recommended, the determination of the shortening of the spectrum and of central scotoma, are discovered to require a highly specialized technique and to be of dubious import.

On the whole, therefore, the psychological experimenter, while utilising the recent work of the physicist in calibrating sources,[27] will find the methods of an investigator such as Hess far more suggestive and far less misleading. Hess, for instance, will never be found guilty of using as a control data secured on a different day, with unequated stimulus intensities, and wave-lengths selected from other (if nearby) regions of the spectrum (Allen). Rather he always pits a normal *O* against the *O* to be examined, testing the two simultaneously, or under identical conditions—a refinement in method inherited from Hering. A few careful quantitative studies carried out along the lines sketched in the *Farbenlehre* should go far toward clearing up the many obscurities of the subject. The limens for the four primaries,[28] the limits of the peripheral fields for brightness and color, and the brightness values of the different hues under dark and light adaptation need to be accurately determined, under carefully standardized conditions. Artificial daylight, a neutrally tuned eye, calibrated stimuli of given areas, at set distances, on a predetermined background, and with controlled exposure times, should be the invariable rule.

The experimenter in psychological territory, moreover, should use to the full his knowledge of the complexities of the psychological organism; setting precedents in methodology by his control of expectation, his repetition and reversal of series, his allowance for tuning and after-images. The shift of hues with alteration of objective intensities, with simultaneous induction, or mal-accommodation should always enter into his calculations. Certainly he will never commit the mistake of the mathematically minded physicist, anticipating a fixed relation between the wave-lengths of complementaries (Cook, Karwoski),

[25]See, *e.g.*, Collins, *op. cit.*, 110 f. The misleading statement recurrent in Parsons (in support of the 'reduction' hypothesis of color-blindness) to the effect that both normal and color-blind accept the same equations apparently refers merely to the *identity* of the components, and not at all to their arithmetical proportions in the equation as such.

[26]Parsons, *op. cit.*, 1924, 33 f.

[27]For sources, see *Sci. Pap. Bur. Stand.*, and recent files of *J. Opt. Soc. of Amer;* for apparatus, M. Luckiesh, *Color and Its Applications*, 1915; Köllner, *op. cit.*, and Hess, *op. cit.*

[28]For differential disks adapted to an abbreviated procedure, see Hess, *op. cit.*, 18 f.; and A. P. Weiss, A limen color mixer, this JOURNAL, 28, 1917, 408 f.

in disregard of the complications of the visual function above noted. Finally, he will be continually on the alert for the emergence of perceptual factors, or a patterning of the stimulus field suggesting perspective or 'figure and ground,' with its resultant shift of values. His records, couched in terms of hue, brightness, and chroma, should afford a new and much needed insight into the color worlds of aberrant cases, and provide a valid basis for a rating schema for use in qualifying observers for visual problems.

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METHOD IN MUSICAL PSYCHOLOGY

The pure psychology of music is far less advanced than many branches of psychology, say that of education, in theoretical validity and practical applicability. A great deal of incoördinated work has been done, but in the main it has either been approached from a philosophical, aesthetic and critical standpoint, or else in a too artificially scientific spirit which often seems to forget the real music altogether, not realizing its nature and its ramifications into the whole mental and physical life of the organism. As so often happens, the various approaches lead to the building up of mutually exclusive and incompatible systems and explanations. Indeed a psychology of music as conceived, let us say, by Dent[1] would have nothing in common with one conceived by Gatewood[2]; they would not even be based on the same types of music. I will therefore consider shortly the drawbacks to the various methods of approach or angles from which the subject has been attacked.

Musical theoreticians have consistently failed to adopt the psychological attitude and to assume the subjective viewpoint. They have discussed form and content, value, absolute *versus* program music, and the like, as objective problems, instead of recognizing that music consists essentially of emotions, images, and percepts in the minds of the composer, performer and listener. That music is 'good' which happens to appeal especially to the subjective tastes of the musicians of the period, these tastes being to a large extent determined irrationally by temperamental and various environmental conditions, by suggestion, contrasuggestion, conservatism and iconoclasticism. Any generalizations made about musical works or principles are more than likely to be contradicted by other writers, especially by those of another generation than our own, or by foreigners whose training has been unlike our own. Both the creation and the appreciation of music are functions of the personalities, or reactions to the affective and cognitive traits, of the people concerned, and if we knew all about these psychological factors and their interaction we should have a complete explanation of musical phenomena. A thorough grasp of this elementary psychological consideration would render futile most of the problems over which aestheticians and critics have wrangled for centuries.

The opposite, purely physical approach of Helmholz[3] and his English exponent, Pole,[4] is entirely off the point. All the knowledge of partials that go

[1]E. J. Dent, preface to W. Pole, *The Philosophy of Music*, republished 1925.

[2]E. L. Gatewood, An experimental study of musical enjoyment, *The Effects of Music*, ed. by M. Schoen, 1927, 78-120.

[3]H. von Helmholz, *Sensations of Tone*, 1st. ed., tr. by A. J. Ellis, 1875; 4th ed., 1912.

[4]W. Pole, *The Philosophy of Music*, republished 1925.

to make up different timbres or chords (which may often help the practising composer or performer) tells us nothing about the music that the psychological individual experiences. The notes on paper and the physical air vibrations are equally unimportant intermediaries, on an entirely different plane from the musical meanings that he imagines or perceives. The latter are not sounds, nor have they any of the attributes of musical tones, pitch, timbre, formal relations, or emotional significance apart from our consciousness of them.

The elementary psychophysical or sensationalist approach of Watt is almost as barren.[5] He would appear to regard music as a simple sensory function based on the subjective attributes or qualities of sound and fundamentally meaningless. Not only do all the objections of the Gestalt school apply to his species of 'constancy hypothesis,' but also music is one of the most obvious cases of emergence, the whole being more than the sum of the parts. Watt entirely fails to explain why music arises from and arouses emotion or aesthetic experience.

Next we may consider the researches of experimental aesthetics on musical elements, such as the investigation of the subjective effects of simple tones, intervals (bichords), or rhythms. Even these give few results that seem to be relevant to the psychology of music. Of indirect value are such results as the maintenance of characteristic attitudes towards tones in the attitude towards chords and to music itself; the effects of familiarity and repetition on the responses to unusual discords, the fallacious ascription of sadness to chords in minor keys, etc. But those who have established such data would scarcely maintain that the effects of music are a combination by association of the effects of these simple, elemental stimuli. Moreover, such effects are conditioned by the attitude of observers to musical works and to music as a whole, so that their significance is necessarily dependent on a general or synthetic view of musical psychology.

A similar objection applies to the otherwise valuable pedagogical and vocational tests that have sprung up of recent years in so striking a fashion. The psychologist often tends to delude himself in this sphere. It is wholly irrelevant to the pure psychology of music to demonstrate objectively that people who obtain a low score on a particular test are unlikely to be successful in the musical profession. The psychologist's problem is, rather, to show that the possession of certain specific traits is a distinguishing mark of the musical mind, *i.e.* to 'job analyze the musical faculty.' From this point of view tests of musical traits have been markedly unsuccessful; for the more precisely observable and scientifically measurable the trait, the less relation does it seem to bear to the musical faculty. Naturally it is desirable to proceed as far as this strictly scientific approach will take us, but correlations between simple capacities such as sense of pitch or rhythm and musicianship, though statistically significant, are so small as to have little theoretical interest. If the psychologist adopts more complex material, say, with a view to studying the mental processes involved in the perception, memorizing or reproduction of music, the objective result of his experiment or test will depend almost wholly on the musical meaningfulness of the material; in other words, on conditions determined by re-

[5] H. J. Watt, *The Psychology of Sound*, 1917; *The Foundations of Music*, 1920.

sponses to music as a whole. To one observer a musical phrase may be grammatical, logical, and easily intelligible, but another observer, whose musical experience differs quantitatively and qualitatively from that of the first, will be sure to find the same phrase of a very different degree of comprehensibility. For example, in one of my experiments, I devised a number of eight-bar tunes which seemed to me to be of roughly equal difficulty and musical logicality; nevertheless one observer took 117 seconds to learn one and 43 seconds to learn another under identical conditions, because their comprehensibility was quite different to him. The scientific experimenter will at once suggest the use of musical nonsense material in order to overcome this difficulty; *i.e.* phrases that are wholly atonal and meaningless. But even if this were possible (and contemporary music shows that its possibility is doubtful), results would immediately become as irrelevant to music as were Ebbinghaus's memory experiments to the theory of the remembering and forgetting that takes place in real life.

I can see no way out of this impasse; it will recur later in this paper. We simply have to compromise by bearing in mind both the musical and the scientific attitude. The success of modern applied psychology is due to a similar compromise; the scientific approach has been introduced, but it has been based on a fairly extensive and coherent idea of mental processes and human nature as a whole. This general idea is continually supplemented by data from psychopathology and other non-experimental spheres, so that it is wholly false to consider experimental psychology as objective from first to last. Thus intelligence tests and the like are able to aim at objective precision because they are developed out of introspective psychology. But in music the too philosophical and too scientific investigators are working comparatively blindly. Hence the view of the true province of the psychology of music which I wish to advocate is intermediate between the usual lines of approach, and it has hitherto received astonishingly little recognition.

It seems to me that a psychology of music should consist in: (1) A full description of all the factors concerned in any musical activity, particularly in its appreciation, performance, and composition, obtained both by objective observation and by introspection. (2) Experimental proofs of the constancy, relevancy, and significance of such factors. (3) A classification and generalization of these results in terms of recognized psychological principles. Naturally all three categories should develop together, but owing to the inadequacy of (1) and (2) this has scarcely been attempted. Only on such a basis can the more usual approaches be theoretically justified, namely: (4) the philosophical methods, (5) the scientific methods.

Admittedly the performer and the composer constitute a very difficult problem; they are far less available than the listener to the psychologist, since amateurs in these fields are so largely concerned with rather uninteresting technical details. I have, however, suggested a fruitful approach to the investigation of musical composition; namely, a study of great composers by means of their biographies.[6] The data, are of course, unreliable; composers usually dislike introspection in that it tends to spoil inspiration. I obtained, nevertheless, some useful results of a quasi-objective nature from a comparison

[6]P. E. Vernon, The personality of the composer, to be published shortly in *Music and Letters*, 10-11, 1929-1930.

of their personalities, the influence of sociological factors, their methods of work, etc., and the consequent characteristics of their music.

But clearly it is the listener who should claim our first attention. The nature of varieties of musical appreciation should be the initial and central problem. Very few investigators have given any account as yet of what listeners, other than themselves, actually think, feel, and do; or have tried to take into consideration all the factors, auditory and non-auditory, that enter into musical situations.

This direct approach is fraught with difficulties and dangers, so that reasons for its previous neglect are not hard to find.[1] They may be enumerated as follows.

(a) Many of the experiences in listening to music are auditory or kinaesthetic and organic, and cannot easily be put into verbal terms.

(b) Time is such an essential constituent of the experience that ordinary retrospection with regard to mental processes of short duration is inapplicable. Many people tend to become absorbed in music to which they are listening, so that, more than in most branches of psychology, introspection may destroy its own data.

(c) Few musical people have psychological training. In my own research, however, I found that they were better at introspection than I had expected, since they seldom listen in a completely passive and subjective fashion, but remain critical of what they hear and of their reactions to it.

(d) Every hearing of a musical item is essentially unique, depending largely on the degree of familiarity with the work, the mood of the moment and other extramusical factors. Introspections, however good, have little valid application to other listeners, to other pieces of music, or even to the same music at another hearing.

(e) The chief difficulty is produced by what one may call the musical psychologist's fallacy, so frequently does it intrude, namely, that of assigning his own modes of thought and attitudes to his observers. If he is musical himself he tends to regard the musically untrained in a lump as inferior copies of himself, and is quite unable to comprehend the individuality and differences of their points of view. If, as seems to be more often the case, he is not really musical himself, he shows even less comprehension of the musician's point of view, often neglects him altogether, or considers his somewhat intellectual type of aesthetic appreciation as anomalous to musical psychology. Again, few workers have employed music that is accepted by the musician as really good or capable of producing genuine aesthetic experience.

It may be objected to my notion that both the experimenter and some of his observers should be drawn from the ranks of well trained and highly discriminating musicians that such people constitute less than 1% of the music-loving public. Yet the following facts must be acknowledged. (1) They are the only people who approximate to understanding what they hear; they therefore have the best right to speak about it. (2) In spite of the distortions exerted by fashion, suggestion and other affective factors, it is the trained (*e.g.*

[1]It should be pointed out that Vernon Lee is the one investigator who has realized this attitude (cf. Varieties of musical experience, *North Amer. Rev.*, 207, 1918, 748-757).

composers, conductors, and critics) who chiefly determine the music that shall last; hence they are the most worthy of consideration. (3) I obtained good evidence that the musically untrained tend to incline to the attitude and opinions of the trained towards music, the more their experience.[8]

Naturally the ordinary music lover should not be neglected: he throws as much light on the highly developed product as child psychology does, say, on language. But that is no reason for the apparent divorce of music from the musician by most experimenters in this field. Similarly, the pure musician who entirely lacks the psychological outlook would be of no use in investigating these problems. On both sides we need to find the happy man.

Examples of this psychological fallacy may be cited. Several classifications of types of musical appreciation have been advanced, but from lack of analysis and comprehension of other listeners they are most unsatisfactory. Some investigators group their observers into those who listen for rhythm, melody, or harmony. In my own work I have not discovered a scrap of evidence for this: the investigators are merely putting their own theoretical predispositions into the mouths of the observers. Again, the majority of writers describe the emotional or subjective and the intellectual or objective types, but entirely fail to recognize the very large and important class of those who synthesize the two attitudes in their highest appreciation. It seems to me that the truest aesthetic appreciation is essentially a fusion of emotion and intellect; it is an appeal to all sides of human nature, not to one isolated set of impulses (though I am quite prepared to admit that this is my personal prejudice or predisposition, and that it does not really apply to others).[9] From my own investigations I think it probable that the purely subjective listeners are usually, though not always, the wholly unmusical who can only interpret what they hear in emotional terms, while the purely objective are either desiccated academic critics or else amateur musicians who are trying to learn to comprehend and appreciate by reacting against excess emotion; the majority of trained musicians grow out of these primitive stages and harmonize the two attitudes.

The fact is that in dealing with an intimate, subjective experience like musical appreciation it is intrinsically impossible for observers and experimenters really to understand one another. Let us free ourselves from the fallacy that words are universal, that they mean the same to everybody. Test instructions, aspects or traits to be rated, etc., mean quite different things to different people. For instance at an experimental concert that I conducted the audience were asked to rate, among other categories, the amount of aesthetic appeal or value. As far as I could make out, some took this to mean:—"the feeling of beauty or aesthetic pleasure produced in me;" others to mean: "the value of the music, quite apart from whether I enjoyed it or not;" and still others to mean: "the amount of thought and criticism I gave to its value;" and so on.

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[8]It is well known that even quite unmusical people tend to prefer good music in the long run, and an experimental demonstration of this has been given by A. R. Gilliland and H. T. Moore, The immediate and long-time effects of classical and popular phonograph selections; essay in *The Effects of Music*, ed. by M. Schoen, 1927, 211-222.

[9]Experimental evidence for this is given in my article, Non-musical factors in the appreciation of music, *Musical Times*, Feb.-April, 1929, 123-124, 227-228, 320-321.

It seems to me that all investigations which try to express purely qualitative, subjective states in objective, quantitative terms are doomed to failure. The aesthetic experience is, almost by definition, a new integration of experiences; it is the least stereotyped, the least objectifiable of mental processes. When we are dealing with extramusical factors such as over-emotional or over-intellectual responses, visualization, gregarious influences, synaesthesis, and the like, we have some more or less definite, stereotyped processes that can be observed and described fairly well in scientific terms. But when we get to these essentially personal experiences we find, first, that they cannot be translated adequately into words, and secondly, that communication is impossible, since the experimenter cannot comprehend the mental organization of observers who differ widely from him. If he tries to describe others he is really only expressing varieties of his own mental organization, his own ideas as to these attitudes and experiences of others. He may, of course, quote their statements freely, but the chances are that he has selected the results that he presents, or that he has biased the observers by setting the situation in which they introspect. Hence, in respect to this central aesthetic aspect of musical appreciation, these considerations indicate further difficulties to be guarded against in experimental attacks.

Two extremes of experimental method have been exploited by different investigators, the free or uncontrolled and the scientific or controlled. In the former anything and everything the observer has to say is recorded either by the observer himself or by the experimenter. This inevitably means that the observer rationalizes, selects, and abstracts so that only partial bits are displayed to the view of the experimenter. Important points are often neglected, sometimes because, like synaesthetic responses, they seem so obvious to the individual concerned. There is also a tendency, I find, for observers not well trained in introspection to record mere artificial criticisms of the music rather than what they actually thought and felt. On the investigator's side, also, the more phenomenological the introspections the less is it possible for him to be impartial in interpreting and working up the results, quite apart from the already mentioned difficulties of communication. Again, I have found it most difficult to isolate or obtain definite evidence on any point, using this uncontrolled method. For instance, at an experimental concert where it was applied one or two of the items received a rather inadequate performance, and I had hoped to trace the effects of this in my observers' introspections. Actually very few mentioned it, but for all I know this may have been because, (1) they failed to notice it at all, (2) they were dissatisfied but failed to analyze their displeasure down to the performers, (3) they noticed the technical faults but forgot about them before they had time to write them down, (4) they disliked recording their criticisms for various reasons such as politeness; fear of making blunders themselves, etc. In fact, so many factors influence the introspections that without some form of control it is almost hopeless to expect to extract any reliable data, to introduce any generalization or systematization.

The fully controlled method can, however, be even more unsatisfactory. Usually, to obtain a degree of objectivity, specific questions have to be answered by the observers (sometimes only 'Yes' and 'No' answers being permitted), or else specific emotions or aspects of the music have to be rated for

each musical item. The inevitable result is that the observer's thoughts are forced into the experimenter's grooves, for these abstractions only apply to the mental processes of those who devise them; only if the experimenter could comprehend and allow for the individual differences of all his observers are such methods really justifiable. In general, the better controlled and systematized the conditions, the more is the situation likely to become abnormal and artificial. Many experimenters have taken observers singly, not realizing that for the great majority of listeners social influences and gregarious factors play a very large part in appreciation. On the other hand if several observers approximating to a normal audience, listen simultaneously, they will have to write their own notes, and a further unnatural factor is likely to be thereby introduced, that of long, possibly idle intervals between the items. For similar reasons the elimination of the personality of the performers by the use of the phonograph is unsound; almost every investigator except Gilman has employed phonographically reproduced music so as to standardize the stimuli,[10] and that at a period when it even less resembled the original than nowadays. We have got so used to the vibrations and distortions of tone produced even by good phonographs that we scarcely notice them; yet in so far as good classical music and the like depend largely on exact and balanced performances, they cannot but make appreciation more difficult. Very little general observation is needed to realise that, for most people, music must be performed "in the flesh" for normal appreciation.

Other experimenters have made attempts to eliminate or control some of the extramusical factors that influence normal musical appreciation; for instance, they have not told to the observers the names of the musical items or composers. If they think that thereby they can make observers appreciate the music solely for itself, unbiased by technical interests and the like, they must be singularly ignorant of the average listener's mentality. The individual who listens to music as pure sounds is a myth based on our psychological theories; his perception of what he hears inevitably depends on his previous experiences with music, his already developed perceptual systems, his tendencies towards intellectual analysis or emotional interpretations. In two experimental concerts of my own I combined a rating method with uncontrolled introspection: in one programs were provided, the performers were in full view, etc., but in the other the music was not announced and the performers were behind a curtain.[11] In this second, abnormal concert not only was there far more technical interest displayed in the listeners' introspections (88% kept on trying to guess the identity of the composers and the music in spite of being asked not to do so), but also the technical interest was more distorted and inappropriate than when, as in the first concert, it was guided normally by a program. The guesses were astonishingly incorrect, the ratings and remarks were far more vague and difficult to interpret. The more musical people are incapable of listening normally until they have pigeonholed what they hear technically and historically; those, on the other hand, who habitually

[10]B. I. Gilman, An experimental test of musical expressiveness, this JOURNAL, 4, 1892, 558-576; 5, 1893, 42-73.
[11]Cf. P. E. Vernon, op. cit.

visualize or interpret what they hear emotionally continue to do so even without titles to guide them.

These observations show how difficult is any approach to musical psychology. There is, in fact, a fundamental opposition between aesthetic experience of any kind and scientific method, which no investigation can fully overcome. The study must be, in its very nature, prescientific, for the present, and we have to compromise. Naturally a scientific approach should always be introduced where feasible, but the artistic attitude must not be forgotten, else the investigator will find that he has destroyed the phenomena he was trying to investigate, and that his results are quite irrelevant to music or art. We must not scorn to make use of 'anecdotal' and other heterodox methods, general observations of the behavior and characteristics of audiences, conversations and expressions of opinion on musical problems by people of all degrees of musical experience, questionnaires, books on music and musical criticisms, etc. In this way the investigator may progressively accumulate a better comprehension of other listeners' points of view, and so be in a better position to apply enlightened scientific methods.

The enormous advances made by experimental psychology are due not merely to its absorption of scientific aims and principles, but also to the thousands of years of 'arm-chair' consideration which binds together and introduces some coherence into these more detailed and objective approaches. The psychology of music can never attain to a similar status until it develops a like general theoretical background that accords with the musician's as well as the psychologists's outlook.

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A CRITICISM OF THE CONFIGURATIONIST'S INTERPRETATION OF 'STRUCTURALISM'

For some time a point of view has been developing that goes by the name of Gestalt Psychology, a construction taking its proximate rise in researches devoted to the phenomena of perception. This school has been active in systematizing the materials derived from the province of psychology as a whole. Not only have the configurationists produced a large number of researches, many of them possessing a high order of merit, but they have also reinterpreted the experimental findings of other schools in terms of Gestalt-Theorie.

Without question, this new point of view has stimulated psychological investigation at large. If the positive contributions of the configurationists have not always seemed to fulfil the expectations awakened by their sanguine promises, at least the criticisms advanced by them against the older and more current interpretations have been most suggestive, even though they have not always been entirely fair.

Discontent with the older structuralism had been growing long before the Gestalt psychology arrived upon the scene. Within the ranks of the structuralists themselves Külpe, at the beginning of the century, broke away in part from the teachings of his master Wundt and founded a new mode of attack upon the higher thought processes. In the Würzburg monographs

there is more than ample indication of the fact that Külpe and his associates considered the Leipzig dogma in need of readjustment and supplementation. With an historical background of steadily increasing doubt as to the adequacy of classical structuralism, Gestalt psychology emerged and announced itself as the 'New Psychology.'

Configuration utilizes the method of functional analysis and is a system built upon the doctrine of wholes. It flatly rejects the structuralist's plurality of elements and regards askance the whole program of the structuralist's analytical approach. The configurationist sees everywhere wholes, uniques, that are not to be resolved very far, if at all, into constituent parts. In brief, Gestalt psychology deals with gross meanings.

The present discussion is not at all concerned with detailed consideration of the positive contributions of the Gestalt school. Rather, the question is here raised whether the violent attacks launched by this school against the psychologies called 'structural' might not possibly be modified, toned down, without any loss of dignity to Gestalt psychology and with a consequent increase in fairness toward structuralism.

An unfortunate tendency is apparent on the part of the configurationists to portray structuralism in such fashion as to set it up to unreserved ridicule. The advocates of Gestalt let slip no opportunity to caricature the teachings of structuralism. Such caricaturing would seem to have become almost an obsession with certain exponents of Gestalt-Theorie. A survey of their polemical writings might well lead the uninitiated to conclude that the configurationists were the first to grant any consideration to wholes, uniques, emergents, although, to be sure, Mach and Von Ehrenfels are given some acknowledgment by them. Also, according to the account given by the Gestalt writers, this school was the first to demonstrate the lack of a point to point relationship between stimulus and sense-perception.

Let us enumerate a few typical criticisms formulated by the configurationists against structuralism. The term attention, for instance, should be relegated to the scrap-heap and be replaced by the term attitude. The term integration has no proper place in psychology, for it is necessarily implicative of "Und-Verbindung." The phrase "due to experience" should be tabooed: instead, say "due to the configuration." Again, it is not a matter of utilizing both the concept of figure-ground and the concept of clearness-levels, but the latter is to be excluded rigidly from all respectable psychological discussion. As for the problem of learning, the term "Insight" solves most of the difficulties. It might be suggested here that the term insight functions as a most convenient "maid-of-all-work" for the configurationists. When search is made for a definition of insight, it at once appears that insight may assume so many forms, is so distinctly protean, that one is sometimes forced to wonder at the magical powers attached to this word.

The configurationist impresses one by his so frequently strained attempts to appear thoroughly *new*. A quotation from Koffka will do excellent service in this connection; he says, "At the end of her twenty-first month, the names of forms which the child had learned from a toy consisting of small and variously shaped pieces of cardboard, were for the first time applied to things in her

environment. Thus, for instance, the folded edge of a man's collar was called a triangle. This should not be understood to mean that the collar edge simply reproduced the name *triangle* on account of its similarity with the triangular cardboard, but rather that the configuration which was acquired in the use of the toy now entered into the perception of a man's collar."[1]

The statement that "the configuration which was acquired in the use of the toy now entered into the perception of a man's collar" is intended to be illuminating and immediately to clear the whole complicated problem. All is explained by the invocation of "configurational law"! Incidentally this sort of explanation is supposed to demonstrate the failure of the older psychology.

If one happens, for example, to be concerned with the problem of the visual perception of depth, he should not, according to the Gestalt psychologists, be so old-fashioned as to recognize that anything of explanatory value may be derived from the facts concerning the disparity of retinal images, but he should sweepingly assert that the phenomenon is "due to a conflict of configurations." When pressed for a more specific answer, he should further maintain that the events under consideration are *primarily* central. That is to say, one should promptly place the problem in a region about which practically nothing is known as to particulars. This type of explanation gives one a sense of security as it cannot be disproved. Unfortunately, however, this sort of procedure resembles nothing so much as the course adopted by a speculative philosophy. Still one may, if he is pushed further or is in doubt, take refuge behind the word "maturation"; where he is safe.

The Gestalt psychologists have all too frequently been guilty of setting up theoretical straw men and then knocking them down. One thoroughly unfortunate result of this wholesale and destructive attack upon structuralism takes the form of a dulling of the critical sense of students. Take for example Koffka's "*Growth of the Mind.*"[2] In this work a devastating onslaught is made upon structuralism. The young student, after a reading of the book, is apt to make up his mind that no good whatever is to be found in structuralism, and that Gestalt psychology either has solved or is well advanced on the high road toward the solution of all problems.

In order to show that the structuralists have not been entirely blind to the fact of unique wholes, of Gestalten, quotations will now be made from the writings of Wundt, Külpe, and Titchener.

Wundt has said: "the expressions outer experience and inner experience do not indicate different objects, but different points of view from which we take up the consideration and scientific treatment of a *unitary* experience."[3] Again: "The immediate contents of experience which constitute the subject-matter of psychology, are in all cases processes of a composite character. Sense perceptions of external objects, memories of such sense perceptions, feelings, emotions, and volitional acts, are not only continually united in the most various ways, but each of these processes is itself a more or less composite

[1] K. Koffka, *The Growth of the Mind*, (R. H. Ogden, tr.) 1924, 289-290.
[2] *Op. cit.*
[3] W. Wundt, *Outlines of Psychology* (C. H. Judd tr.), 1907, 2 (italics in this and the following quotations are mine).

whole."[4] He furthermore states that "all the contents of psychical experience are of a composite character. It follows, therefore, that psychical elements, or the absolutely simple and irreducible components of psychical phenomena, are the products of *analysis and abstraction*."[5]

Külpe has stated: "But there is one other point to which attention must be called if we are to avoid misunderstanding of the character of the conscious elements. . . . By help of the attention we may, it is true, subject even the less intensive elements to special investigation or observation; but *real isolation*,—the actual experience of one single sensation, for instance,—*can never take place*."[6]

Titchener says: "Whether we begin our psychology with 'sensation' or with a case of 'association of ideas,' we are always beginning with an abstraction."[7] He goes on to say: "This sensation, which is a structural unit of the adult mind, is not the genetic unit of mind at large. Mind has not grown by aggregation of sensations, by the simple addition of our 'blue' and 'yellow' to a given 'black' and 'white' that are also like ours, and by the further addition, still later, of a ready-made 'red' and 'green.' Even granted that we could analyse the primitive mind into sensations, still its 'black' and 'white' would be so different from our own as to be hardly recognisable. At the time when the heart begins to pulsate, there are no muscle fibrillae in the myocardium; we have the sight of purely protoplasmic, undifferentiated cells making strong rhythmical contractions. If, now, the structural elements of the primitive mind are 'sensations,' they are sensations only in the sense that these primitive heart-cells are 'muscle' cells. . . . . Knowing . . . does not come, any more than does mind, from the addition of sensation to sensation."[8] Titchener maintains: "This fact, then, is important: that we may attend to the *perception as a whole*, . . . But the other fact, that systematic observation always reveals the complexity of the perception, is of no less importance."[9] Further on he states: "Let us assume for the moment that any perception may be analysed, without remainder, into a number of sensations. It would still be true that the mere enumeration of these sensations is not an adequate account of the perception. For the sensations which we find, in the particular case, form a group; they have been selected, singled out, marked off, from the other contents of consciousness."[10] "Perceptions are selected groups of sensations, in which images are incorporated as an integral part of the whole process. But that is not all: the essential thing about them has still to be named: and it is this,—that perceptions have meaning. All perceptions mean; they go on, also, in various attributive ways; but they go on meaningly."[11] "We have no reasons to believe that mind began with meaningless sensations, and progressed to meaningful perceptions. On the contrary, we must suppose that mind was meaningful from the very outset."[12]

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[4]*Ibid.*, 28.
[5]*Ibid.*, 31.
[6]O. Külpe, *Outlines of Psychology*, (E. B. Titchener tr.), 1908, 21.
[7]E. B. Titchener, *Experimental Psychology*, Vol. I, Pt. ii, 1901, 3.
[8]*Ibid.*, 4.
[9]*Text-Book of Psychology*, 1910, 349.
[10]*Ibid.*, 364. [11]*Ibid.*, 367. [12]*Ibid.*, 369.

Titchener, discussing James on the problem of analysis, makes the following significant statement: "A third inference, which is certainly as dangerous as the two mentioned" [by James] "is this: that the psychological elements, just because they are elementary, are chronologically the first things in mind, so that perceptions grow, are formed, by the interconnection of originally separate sensations. The elements are, as we have seen, the results of analysis; the perceptions are the original things, and the sensations are found in them by observation; perceptions are given us, and we discover that they are analysable. Misunderstanding here is fatal to the student of psychology, for it means misapprehension of the central psychological problem."[13]

This last quotation in particular is a most important one for the correct understanding of the structuralist position. Gestalt psychologists have been ranting about the chaos theory of mind presumably propounded by the structuralists in the field of genetic psychology. Have they ever paused in their criticisms to read the passages quoted above?

Analysis of the 'structural' point of view reveals three principal facts. First, that clear and unequivocal recognition is accorded the phenomenal actuality of wholes, *Gestalten*. Second, that careful distinction is drawn between the systematic 'sensation' and the observational attribute. Third, that sensation is treated as an *analytic, not as a genetic,* concept. Titchener, in the last quotation above given, states it to be a necessary supposition that mind was meaningful from the very first.

It would appear that configurationists have not noted these facts. Spearman, writing of Gestalt psychology, says, "if this imposing movement is to be traced back to any single source, the best claim is undoubtedly on behalf of Wundt with his well-known principle of 'creative synthesis.' "[14] Boring recently asked, "Is Gestalt a protest against Wundt and his influence? What about Wundt's *Aktualitätstheorie*, his creative synthesis and the law of psychic resultants? Was Wundt nearly so atomistic as his critics would have him be, or did he too have a vision similar to that of James, and Ehrenfels, and Wertheimer'?"[15]

The sensory attribute is, for the structuralists, the ultimate psychological datum. The systematic 'sensation,' however, is for them *always a logical abstraction*. In the utilization of abstractions psychology differs in no wise from other disciplines, such as physics, with its concepts of an ether and quanta.

No doubt the term structural has been an unfortunate one, leading as it so frequently has to an interpretation of the word as synonymous with the term atomistic, and 'sensation' as synonymous with 'psychic atom.' Titchener was aware of the unfortunate misuse so often made of the word structuralism, and on account of such misuse finally expressed his view that the terms structural and functional were obsolete as applied to psychological systems.[16] The word

[13] *Ibid.*, 350-351.

[14] C. Spearman, The new psychology of 'shape,' *Brit. J. Psychol.*, 15, 1925, 212.

[15] E. G. Boring, The problem of originality in science, this JOURNAL, 39, 1928, 88.

[16] E. B. Titchener, Functional psychology and the psychology of act, this JOURNAL, 32, 1921, 533.

structure has been historically weighted in the direction of materialistic meanings of one order or another, and is applicable to dynamic patterns such as constitute the subject matter of psychology in an analogical sense only; and such analogical application of this term is the only one that the structuralists would admit.

As a matter of fact, the structuralists acknowledge the phenomenal priority of wholes, but are alive to the fact that due to attentional limitations, an exhaustive account of the contents of a given whole can be obtained only under the condition that the various aspects are selected and described one by one, and moreover that a description of stimulus conditions does not constitute a description of the psychological order.

The Gestalt psychologists have had much to say concerning the glaring defects of the 'structural' mode of analysis and the effectiveness of the so-called functional, indirect, analysis. But, as concerns the supposedly artificial nature of the structural analyses, it seems that such analyses can not be so completely absurd as the configurationists have represented them to be in view of the fact that certain synthetic checks on the attributive analyses have actually been made. There is, for instance, the synthetic experiment in tactual perception, where such perceptions as those of liquidity,[17] oiliness,[18] and stickiness,[19] have been artificially produced upon the basis of attributive analyses without the utilization of liquid, oily, or sticky stimuli.

The concept of wholes is of the greatest importance for science in the large, but it can be done to death, it can be utilized as a deus ex machina. More than half a century ago Wundt wrote confidently that the faculty psychology had received its deathblow. We know, however, that although the faculty psychology in its grosser form has vanished, it nevertheless persists under the guise of various subtle forms. Occasionally, one might be willing to venture the appellation Neo-Faculty Psychology in respect to configurationism. Its manipulation of entities called *Gestalten* has frequently all the appearances of faculty psychology on masquerade.

The problem of formal properties is the one treated by the Gestalt psychology. But there is another and equally important problem. This is the problem of filling, of content; and the problem of content has been most thoroughly dealt with by the psychologies called structural.

As concerns Wundt, he has held that everything comes to the immediately experiencing subject in wholes. These require, however, according to the Wundtian school and its modifications, constitutive analyses into attributive terms. Wundt and his disciples have never taught that mind is the result of the bundling together of psychic atoms called sensations, even though the configurationists glibly refer in terms of seeming contempt to the "bundle-hypothesis" of structuralism. To the structuralist, mind is meaningful from the very outset. Wundt is not an atomist, whatever else he may be dubbed, and what-

[17] I. M. Bentley, The synthetic experiment, this JOURNAL, 11, 1900, 405.

[18] L. W. Cobby, and A. H. Sullivan, An experimental study of the perception of oiliness, *ibid.*, 33, 1922, 121.

[19] M. J. Zigler, An experimental study of the perception of stickiness, *ibid.*, 34, 1923, 73.

ever his failings may have been. If the contents of psychological pattern have ever received highly technical and exhaustive description from the point of view of constitution, then Wundt and his followers deserve the credit.

Bear in mind, along with the attractive aspects of Gestalt psychology, the existence of many internal bickerings within its own domains. Instead of talking about *the* Gestalt psychology it would be more appropriate to talk about the Gestalt psycholog*ies*. *Gestalt-Theorie* is just that: a theory, an hypothesis, and a valuable one. But it is not a revelation of infallible order.

It is the conviction on the part of many that the well-known lines written by James forty years ago at the conclusion of his *Principles* are quite applicable to the condition of the psychologies and the near-psychologies of to-day. Recognition must be more fully accorded by some thinkers to the fact that mental organization may be described and interpreted from a number of points of view and in terms of different observational attitudes. No single point of view can be deemed exhaustive of the possibilities of description. In view of the immense complexity of mental structure-function, and in consideration of the marked temperamental differences existing between workers in the field of psychology, it would be well to maintain an open-minded, appreciative, but carefully critical attitude toward a number of systems. Omniscience can not be attributed to a single system. Each system has had something of value to contribute to our total fund of knowledge. The psychologist above all investigators should strive to develop and maintain a balanced historical sense. Only then can a system of psychology, be it 'structuralism' or the still adolescent *Gestalt-Theorie*, obtain a stable growth, effective application, and just evaluation.

Clinton, N. Y. PAUL CHATHAM SQUIRES

SEX-DIFFERENCES: SOME SOURCES OF CONFUSION AND ERROR

The traditional point of view toward the mental ability of women is being modified rapidly. The average man appears to have changed his stock of generalizations and rationalizations to some degree. Popular magazines now carry articles which attempt to place the new knowledge vouchsafed by 'science' before the multitude. The shrewd publisher finds a ready market for 'scientific' books which deal with this alluring topic. The novel and short story also take their turn in providing half-truths for the 'lay reader.' The total effect of such writing has brought about a decided change in attitude, but whether or not the information so disseminated has scientific validity is a question of importance and doubt. The writers of this article feel that sweeping dogmatic attitudes toward all problems of sex are almost always based upon decidedly scant information. Magazines, novels, *et al.*, have disseminated much information that is clearly unsound or unproven.

False conclusions with reference to sex-differences have been due in many instances to one or more of the following influences: (*1*) differential selection of test items; (*2*) differential selection of the individuals studied; (*3*) the limited number of individuals studied; (*4*) inaccurate, or crude, statistical procedure and treatment of results; (*5*) hasty generalization due to the assumption that findings are applicable to all age-levels when certain age-levels only have been

studied; (6) hasty generalization due to careless use of terms; and (7) the assumption that group differences are innate or inherited predispositions.

The above influences will be commented upon briefly in the discussion following.

(1) *Differential selection of test items.* Girls have been found to excel boys in linguistic ability by practically all investigators. Conversely, a consistent difference in favor of the boys has been found on practically all tests involving the use of number and in most tests which demand ability to manipulate mechanical contrivances. Inclusion of many items of the above types of questions in intelligence tests provides therefore a discriminating factor in test results.

The superiority of men over women on the results of the Army Alpha Test is well known. Although a few investigators at first thought that the results were indicative of male superiority, practically everyone now admits that this particular test was weighted heavily in favor of men.

In contrast to the Army Alpha Test is the 1928 edition of the Scholastic Aptitude Tests used by the College Entrance Examination Board.[1] In this test the numerical sub-tests, *e.g.* the test in arithmetical problems and the one in number-series completion, were deleted (because of statistical studies of preceding years) and sub-tests employing synonyms, easy paragraph reading, hard paragraph reading, classification, antonyms, analogies, and double definition were used. On the modified test the girls exceeded the boys by 17.28 points. The mean for the boys was 490.86; for the girls, 508.14.[2]

The above citations explain why intelligence test data are often not convincing when used for making sex-comparisons. Nevertheless, most of the studies in which intelligence tests have been employed reveal small differences only. A common practice in dealing with intelligence tests is to consider differences in total or composite performance upon a number of tasks calling for widely varying mental reactions. The expression of the total performance is usually in terms of M.A. or I.Q., blanket terms including many varied and totally unlike cognitive responses. Terman finds that girls do slightly better in the Stanford-Binet Test than do boys of the same C.A. until the age of thirteen is reached. Miss Goodenough believes that the slight superiority of girls is due to the fact that "more than half of the total number of items in the section of the Binet under consideration are chiefly upon types of performance in which females have been found to exceed males at all ages for which information is available, while the types of performance in which males have usually been found to exceed females are practically unrepresented."[3] Because of the conspicuous differences in the types of test items employed in the intelligence test, "the question of mental precocity as related to sex must be answered in terms of specific functions or traits, rather than in terms of unanalyzed general tendencies." It appears therefore that little data can be adduced to demon-

[1]The tests employed by the College Entrance Examination Board are called Scholastic Aptitude Tests not because they differ fundamentally from the so-called intelligence tests, but rather because it is now recognized by many that the earlier descriptive term was inappropriate.

[2]C. C. Brigham, *Third Annual Report of the Chairman of the Commission on Scholastic Aptitude Tests*, 1928.

[3]F. L. Goodenough, The consistency of sex differences in mental traits at various ages, *Psychol. Rev.*, 34, 1927, 458 f.

strate male superiority in general intelligence; reported differences have often been due largely to the weighting of the test-instrument in favor of one sex or the other.

(2) *Differential selection of the individuals studied.* In 1911 Cohn and Dieffenbacher tested students in one of the few co-educational schools of secondary school rank in Germany.[4] They found the girls superior to the boys. Since these German scholars did not believe the female to be superior to the male, they attributed their results to the fact that the girls comprised a more highly selected group than the boys.

In America several investigators have found high school senior boys to be superior to high school senior girls in intelligence test-scores.[5] Such findings have been explained usually as due to the more rigorous selection of boys throughout the elementary and secondary school years. It is of course well known that boys are eliminated from school more rapidly than girls and that dull pupils tend to drop out of school at earlier ages than bright ones. Since sex-difference in rate of elimination from school occurs at very early ages, it is only logical to conclude that difference in intelligence test scores at the high school level may be due to differential selection of the individuals studied.

In the above-mentioned studies the fact of differential selection was taken into account in the interpretation of findings. Nevertheless, differential selection is often overlooked or neglected in other studies which report sex-differences in traits other than intelligence.

(3) *The limited number of individuals studied.* The necessity of having an adequate sampling of subjects is obvious, but it is often ignored. A very large number of studies are based upon responses of less than a dozen subjects of either sex. Winsor reports that differences between five men and five women have been cited as evidence of greater variability of the male.[6] One writer studied only two subjects, himself and his wife; he then wrote an article on sex-differences.[7] When conclusions based upon studies which have used only a few subjects are given a second- or third-hand recital, (and this is frequently done) the number of subjects studied is often not mentioned and the reader is then given an erroneous impression regarding the reliability of the original studies.

(4) *Inaccurate or crude statistical procedure.* Winsor has shown that reported sex-differences in variability are due often to peculiarities of formula for determining variability.[8] Measures of gross variability occasionally have displayed one result and measures of relative variability have revealed a quite

[4] J. Cohn and J. Dieffenbacher, *Untersuchungen über Geschlechts- und Begabungsunterschiede bei Schülern*, 1911. Quoted by H. T. Woolley, The psychology of sex, *Psychol. Bull.*, 11, 1914, 375.

[5] W. F. Book, *The Intelligence of High School Seniors*, 1922; S. S. Colvin and A. H. MacPhail, Intelligence of seniors in the high schools of Massachusetts, *Bull. U. S. Bureau of Educ.*, No. 9, 1924; D. G. Peterson and T. A. Langlie, The influence of sex on scholarship ratings, *Educ. Adm. and Sup.*, 12, 1926, 458-469.

[6] A. L. Winsor, The relative variability of boys and girls, *J. Educ. Psychol.*, 18, 1927, 327.

[7] H. D. Marsh, Individual and sex-differences brought out by fasting, *Psychol. Rev.*, 23, 1916, 437-445.

[8] *Op. cit.*

different tendency. The careful student should demand therefore the formula which has been employed for measuring variability when sex-differences are reported.

When individuals have been grouped on the basis of sex, the resultant group differences have usually been regarded as 'sex-differences.' L. S. Hollingworth points out that a chance difference would be found if the individuals in certain groups were re-segregated on the basis of any incidental factor—say, eye-color or presence or absence of physical anomalies—and the same comparisons made.[9] Apparent sex differences must be examined always in terms of probable error.

For many years, workers assumed that the female was inferior to the male in general intelligence. A convincing part of the evidence was the difference in brain-weight between male and female. Today it is realized that brain-weight is an unreliable measure of intelligence. Woolley states (and practically everyone now admits) that the relatively small weight of the female brain as compared with that of the male is of little significance in affecting mental ability.[10]

Prior to the past dozen years it was commonly assumed that the sex-impulse was much less intense in women than in men. This drive has been studied extensively in animals. In certain species it has not been found that female animals show a weaker sexual urge than males. Females surmount obstacles and traverse electric grids in order to reach their mates as often as do the males. Moreover, among the animals studied in the laboratory, the amount of activity directed toward a member of the opposite sex does not appear to be greater in the male than in the female. Modern students are beginning therefore to doubt the existence of inherent sex-differences with reference to the strength of the sex-propensity in man. They are inclined to attribute apparent difference to such factors as social custom, community mores, etc. This attitude has developed almost entirely within the past decade and it has resulted largely from improved methods of animal experimentation.

(5) *The assumption that findings are applicable to other age-levels than those actually studied.* A fifth source of error is hasty generalization due to the inclusion of all or nearly all age-levels in the formulation of conclusions when certain age-levels only have been studied. Thurstone has found that scores vary markedly from grade to grade and from age to age.[11] Dunlap also has pointed to the fact that variability depends (among other things) on age-level.[12] Because of the irregular development of numerous human characteristics, one may expect to find that at a certain age-level girls will be more variable than boys in some regards, and at an earlier or later age the reverse may be true. With respect to any characteristic, the greater variability will occur during the period in which the most rapid changes in that characteristic are taking place. The averages for total behavior manifestation (covering several years) sometimes obscure the real sex-differences, and in other instances

[9] L. S. Hollingworth, Comparison of the sexes in mental traits, *Psychol. Bull.*, 15, 1918, 427-432.

[10] H. T. Woolley, A review of the recent literature on the psychology of sex, *Psychol. Bull.*, 7, 1910, 335.

[11] L. L. Thurstone, A method of scaling psychological and educational tests, *J. Educ. Psychol.*, 16, 1925, 433-451.

[12] K. Dunlap, *Social Psychology*, 1925, 32.

the averages exaggerate the general trend. The writers are here interested in emphasizing the fact that the findings for one age-level do not hold invariably for all other age-levels. They wish also to call attention to the danger of obscuring significant differences by treating *en masse* data secured from heterogeneous age- or grade-groups. These points have frequently been neglected or minimized when findings with reference to sex-differences have been reported.

(6) *Careless use of terms.* Psychological literature contains much material on sex-differences referring to such traits as "fidelity of report," "ability to describe an object," "range of information," etc. Early writers seem to have considered 'fidelity of report' a unitary trait; they certainly assumed that simple devices measured complex traits and that a single study of a few subjects justified broad generalizations.

In discussing sex-differences with reference to "fidelity of report" Whipple writes: "In all of Stern's work, both in narratives and depositions, with pictures, or events, or estimations of times and distances, whether under oath or not, the reports of men have been more accurate (by from 20 to 30%), ... Stern's conclusions, however, have not been confirmed by Wreschner, Breukink, or Miss Borst."[13]

When sex-differences are reported in vague and unanalyzed terms it is of course inevitable that the findings will be contradictory and the conclusions conflicting. McGeoch has demonstrated in three recent articles that the ability of children from ages 9 to 14 to make a correct report is specific in relation to the material reported.[14] It is therefore ambiguous to discuss sex-differences in 'fidelity of report' unless the specific material to be reported and the specific test-conditions are set forth.

A second example of the use of terms that are too inclusive is afforded by the literature dealing with 'sensitivity to pain.' Many investigators have reported that women are *more* sensitive to pain than are men. Ottolenghi and Lombroso state that women are markedly *less* sensitive than men to electrical stimuli.[15] It is evident of course that pain may be aroused in innumerable ways, and that greater sensitivity to pain aroused by one method does not posit greater sensitivity to every kind of pain.

A more modern example of the careless use of terms is the statement that females show a consistent superiority over males in "verbal memory." The term 'memory' connotes *a very wide variety* of performance. Although girls frequently do better than boys on tests which demand certain types of memory it is by no means certain that they are able to excel in every type of memory. Bassett reports that girls are slightly *inferior* to boys in retaining historical information; that boys show greater retentiveness for information relative to war, fighting, and geographical location.[16] The girls on the other hand were

[13]G. M. Whipple, *Manual of Mental and Physical Tests*, Pt. ii, 1915, 398.

[14]J. A. McGeoch, The influence of sex and age upon the ability to report, this JOURNAL, 40, 1928, 458-466; The relation between different measures of ability to report, *ibid.*, 40, 1928, 592-595; Intelligence and the ability to report, *ibid.*, 40, 1928, 596-599.

[15]Quoted by Whipple, *op. cit.*, Pt. i, 239.

[16]S. J. Bassett, Sex-differences in history retention, *School and Soc.*, 29, 929, 297-298.

superior in retaining historical facts which treat of domestic conditions and home life. It seems therefore that the type of material has a marked influence upon test results and that the current notion that girls are superior to boys in 'verbal memory' may have been due to the use of terms that are too inclusive.

This tendency to employ loose or inexact terminology probably accounts for many of the other conflicting results that are discussed by Whipple.[17] Whipple states that conflicting results have been found with reference to such abilities as "uncontrolled association," "discrete association," "analogies test," "computation," "logical memory," and "size-weight illusion." The preceding remarks are probably applicable also to such vague and unanalyzed traits as "immediate memory," "delayed memory," "mechanical ability," etc.

(7) *The assumption that group-differences are inherited predispositions.* It was mentioned above that certain differences in the strength of the sexual impulse are probably of sociological rather than of biological origin. This difference and certain other differences have been assumed to be biological in origin. Examples of such assumptions are numerous. Thus, Jastrow has mentioned general paralysis as a typically masculine insanity. He seems here to imply that the greater frequency of general paralysis in men is a clue to the existence of some innate sex-difference in neural functioning.[18] Hollingworth has pointed to the fact that general paralysis occurs more frequently among men because syphilis occurs more frequently among them.[19]

In a forthcoming article the writers present data with reference to sex-differences in play behavior.[20] A list of activities in which girls were found to participate much more frequently than boys was assembled. A second list of activities in which boys were found to participate much more frequently than girls was also prepared. The writers attempted to identify in their two lists those activities which were most likely to demand an appreciable amount of mechanical skill and motor ability. It is of course true that the preceding terms are vague and that almost any activity involves some degree of motor ability. It is apparent however that some activities require much more mechanical dexterity than others. When generous allowance is made for erroneous judgment in the above regard it was evident that the boys participated much more frequently than the girls in activities requiring motor skill or mechanical ability.

Of course various interpretations might be placed upon the writers' findings. It is obvious that differences in background of the sexes are in themselves very marked and perhaps sufficient to account for the differences in types of play behavior. It is conceivable that a list of motor activities might be assembled which would elicit more frequent participation among girls than among boys.

Although it has been found by several investigators that boys and men are superior to women and girls in tests which posit mechanical ability and certain types of motor skill it is very doubtful that the so-called mechanical ability and aptitude tests are valid measures of *innate* abilities.

[17] *Op. cit.*

[18] J. Jastrow, *Character and Temperament*, 1915.

[19] L. S. Hollingworth, Sex-differences in mental traits, *Psychol. Bull.*, 13, 1916, 382.

[20] H. C. Lehman and P. A. Witty, Sex differences: interest in tasks requiring mechanical ability and motor skill, *J. Educ. Psychol.*, (forthcoming).

To the writers it seems obvious that mechanical ability and motor skill are not unitary traits, but generic terms which include many specific functions or traits. This hypothesis is supported by the findings of Perrin,[21] Muscio,[22] and Barton.[23] These workers found that relatively simple motor abilities correlate only loosely with each other. The question whether the so-called mechanical aptitude tests measure anything more than acquired abilities is therefore a valid one to raise. Do these tests measure *innate ability* or do they test merely specific acquired modes of response? If the tests of mechanical ability are merely measures of previous experience and habit acquisition, one is not justified in designating them motor or mechanical ability tests unless one grants that the ability is an acquisition and not an innate predisposition. It is apparent that girls are more skillful than boys in sewing, knitting, crocheting, etc. This does not mean however that men *cannot* become as competent as women in these tasks with *equal practice and motivation*. Measurement of sex-differences in the above 'abilities' would not be measurement of innate ability but merely a statement of present attainment.

The nature of certain motor activities participated in by boys and by girls suggests that the varied experiences and social stimuli are potent forces in effecting such differences as are brought out by the so-called mechanical aptitude tests. The girls were found to participate very much more commonly than the boys in such activities as "cutting paper things with scissors," "stringing beads," "sewing, knitting, crocheting, etc." The boys on the other hand participated much more frequently than the girls in activities such as "using a hammer, saw, nails," "making or using a wireless or other electrical apparatus," "throwing rocks or stones," "climbing," etc. These differences in recreational interests may be traceable to social customs, attitudes, and values. So conspicuous are these factors that they suggest that alleged innate sex-differences in mechanical ability may be due to differences in attitude and experience, products almost solely of environmental factors.[24] It may be that sex-differences in performance in the mechanical aptitude tests are therefore not tests of *ability* at all but merely tests of the present level of performance. Perhaps certain fundamental assumptions of those who presume to test these 'abilities' will need to be revised or modified.

Results of some major investigations. There was a period not far removed when "male superiority" was proclaimed and generally accepted. More recently it was believed by many that important sex-differences were to be explained by the fact that the male is by nature more variable than the female. For example, as recently as 1926 Havelock Ellis defended his earlier position

[21]F. A. C. Perrin, An experimental study of motor ability, *J. Exper. Psychol.*, 4, 1921, 24-56.

[22]B. Muscio, Motor capacity with special reference to vocational guidance, *Brit. J. Psychol.*, 13, 1922, 157-184.

[23]E. Barton, *Correlations among Motor Abilities*, A. B. Thesis, Univ. of Wisconsin. (Quoted by C. L. Hull, *Aptitude Testing*, 1928, 21.)

[24]It is of course obvious that, in so far as *general* mechanical ability is concerned, sex-differences may be (and probably are) due merely to the weighting of the test in favor of one sex or the other. The earlier reports with regard to sex-differences in general intelligence are suggestive in this connection, and the fallacy of these earlier reports should not be ignored.

concerning the relative variability of men and women.[25] Experimental evidence reveals that whenever a large population of a given age has been tested, girls are as variable mentally as are boys. For example, in revising the Binet–Simon Test, Goddard studied 2000 children. His data failed to show any sex-difference in variability.[26] In administering his series of arithmetic tests to large numbers of New York City children, S. A. Courtis likewise found no sex-difference in variability.[27] In revising the Binet-Simon Scale, Terman studied 1000 unselected children; the supposed greater male variability was not found.[28] In standardizing his Language Scales, Trabue studied 1590 boys and 883 girls. His figures show that (according to quartiles) there is no sex-difference in variability.[29] Frasier studied the grade location of all of the thirteen-year-old children in 20 American cities. The greater male variability was not demonstrated in his analysis of 62,219 boys and girls. The number of boys and girls at the extremes of the distribution were approximately the same. Moreover, the range was the same for the sexes.[30]

General intelligence and educational tests have thus far not been standardized separately for the sexes. From available data, it appears that sex-differences do not warrant such a practice.

In so far as school needs are concerned, E. A. Lincoln points out that increased provision for individual differences meets adequately the problems of sex-differences.[31] To the psychologist the problem of sex-differences nevertheless continues to be a pertinent one. In view of this topic's unfortunate history and in view of the numerous possibilities of spurious findings and erroneous conclusions, reported sex-differences should indeed be scrutinized critically.

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First International Congress on Mental Hygiene

The Congress is announced for Washington, D. C., May 5-10, 1930, under the presidency of Dr. William A. White. Annual meetings of the American Psychiatric Association and the American Association for the Study of the Feebleminded will be held concurrently. The subjects for discussion in the Congress bear upon mental hygiene at all ages of man, upon the family and the community, upon instruction, and upon care, prevention, and treatment. Various related subjects, anthropology, education, eugenics, psychology, nursing, sociology, etc., are also to be represented upon the extensive program of the Congress.

M. B.

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[25] H. Ellis, *Man and Woman: A Study of Human Secondary Sexual Characteristics*, 6th ed., 1926. See new preface.

[26] H. H. Goddard, Two thousand normal children measured by the Binet Measuring Scale of Intelligence, this Journal, 18, 1911, 232-259.

[27] S. A. Courtis, Report on the Courtis Tests in Arithmetic, *N. Y. Committee on School Inquiry*, 1, 1911, 391-546.

[28] L. M. Terman, *The Measurement of Intelligence*, 1916.

[29] M. R. Trabue, *Completion Test Language Scales*, 1916.

[30] G. W. Frasier, A comparative study of the variability of boys and girls, *J. Appl. Psychol.*, 3, 1919, 151-155.

[31] E. A. Lincoln, *Sex Differences in the Growth of American School Children*, 1927.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Systematic Psychology: Prolegomena. By EDWARD BRADFORD TITCHENER. New York, Macmillan, 1929. Pp. xi, 278.

Psychologists who are familiar with Titchener's long and productive term of academic service will remember that his chief university courses were two; one an Introduction which stands represented by his text books and laboratory manuals, the other a Systematic Psychology, extending first through a year's lectures and later through two years. Early in his teaching career the Systematic lectures were written substantially under the outline of Külpe's *Grundriss* (1893). The general constitution and program of these lectures were not much changed through thirty years and more; although considerable new material, factual and theoretical, was added during this long period. Titchener himself relinquished the course about 1900; but for a great many years he cherished the hope that he would at some time bring to publication in a large work what he called his "system." Various phases and by-products of this system appeared in occasional lectures upon Feeling and Attention and upon the Thought Processes, in many articles and in the textbooks, the latter always cast in strict systematic terms. The great work, however, for which the model always stood before him in Wundt's *Grundzüge*, never appeared. Outlines, segments and parerga in plenty; and now we receive with high interest the *prolegomena*.

The volume opens with a comparison of Wundt and Brentano, of empirical and experimental psychology. A chapter devoted to the general nature of science and two upon the definition of psychology (point of view and subject-matter) make up the body of the book. The lesser half of the work (pp. 3-22, 157-259) is reprinted from articles published in this JOURNAL in 1921-22 (Vol. 32, 108-120, 519-542; Vol. 33, 43-83). It includes the comparison of Brentano and Wundt and most of the chapter on subject-matter. The new sections chiefly treat of point of view and of science taken in the large. A few pages added to the last chapter include in general summary the author's views respecting the proper relations of psychology to physics and biology.

The republished sections of the book call for no review and for comment only where they are integral to the entire work. They are in the main critical expositions and estimations of other systems, most of them from the 19th century. Those writings which fall in part in the earlier years of the present century but derive largely from earlier times include the names of Münsterberg, Meinong, Stumpf, Husserl, Lipps, Külpe, Witasek, and Höfler. Angell was roughly contemporaneous with Titchener's 'structural' period and Messer follows in the traditions of Husserl and the later Külpe. All of this systematization which Titchener expounds and values was either done or else in the baking before 1910. It is perhaps natural, then, that the psychological ideas among which Titchener moves in his discussions belong—nearly all of them—to the third-of-a-century following 1874 (Wundt's *Grundzüge* and Brentano's

Psychologie vom empirischen Standpunkte). A biological hint or two comes from a later time; but there is scarcely a suggestion of the late revolution in physics or of recent matters in psychology.

The author skilfully expounds Brentano and Wundt, himself voting heartily for the experimental Wundt and the Wundtians, who *describe*, and against Brentano and his empirical kind, who *argue*. Had he considered fruits more than formal conceptions he might have recalled respectable descriptions of an experimental sort which came from Brentano's great disciple Stumpf, who (together with his laboratory students) filled for many years one of the meatiest sections of Titchener's own lectures upon Systematic Psychology.

In the chapters which follow the Introduction Titchener proposes to define psychology, and since he desires to exhibit his own subject as a science among others he proceeds to derive a conception of science-at-large which shall bring all the fundamental sciences (including psychology) to a common ground. This common ground he finds, as he thinks, both in point of view and in subject-matter, terms in which he had ordered his scientific thinking for at least thirty-five years.

Titchener's occupation with 'system' was almost an obsession. All systems —his own and others—he constantly tested in terms of point of view, subject-matter, and method. 'Method' (of which he wrote much and with great conviction in his more productive years) he did not do for the present context. For him it always logically followed subject-matter. The real reason why he did not again, in the last ten years, 'tackle method systematically,' as he might have said, is not easy to assign. The task certainly was less difficult in those years when he was devoted to 'introspection' and wholly confident of the general competence of that method.

In reading the chapter devoted to science, those who have known the author will find presented much that they already knew of Titchener's general views. The man of science is as important as the material, the point of view and the method. "We choose as our starting point," he writes, "the scientific temper or attitude or frame of mind" (28). This is (on the negative side) *dissent*: "the man of science, then, is out of accord with the majority of his fellow men" (30); and (on the positive side) *observance*, a "disinterested and impersonal observance of the world of human experience." The man of science is "the servant of nature: interpretation he leaves to those of different bent" (34). Interpretation includes such things as valuation, meaning, and human uses. Bare existence only is left. "An instinctive determination to identify himself with his subject-matter, to lose himself in it, to become one with it." Does this echo an early revolt in student days and the espousal of the righteous and protestant cause of science when science and religion made warfare? The English references cited in the context seem to suggest it. At all events the implication is important; for the contention reveals our author's formal attitude not only toward psychologists and systems but also toward many other men and matters. It is a kind of priestly dedication. Its *credo* seems to imply (quite against the logic of the man of science) a doctrine of human, intellectual and professional values. It is possible that men of Titchener's social orientation are easily inclined toward such a position.

When we turn from the man to the materials of science we learn that the fundamental matter is "an intensive living-through of some item of the existential universe. . . . . a sort of participation" (39). At the same time "the attitude of science is . . . . . complicated by the non-scientific, extra-scientific attitude of logic" (44). The large part necessarily played by logic is obviously disturbing to Titchener, who insists that the special competency of the man of science is "his competency as observer" (49). The disturbance may have been accentuated by his reflection that rigorous observation at first-hand and in an experimental setting had occupied but a very small place in Titchener's own scientific labors since the Leipzig days and what he calls 'logic' a very large and preponderant place. "Observation is," as he remarks, "the single and proprietary method of science and . . . . . experiment, regarded as scientific method, is nothing else than observation safeguarded and assisted" (43). Whether this single and proprietary method can safely be delegated to others by the man of science is not discussed in the book; although the author does suggest that "we might . . . . . ask the logician to plan our experimental procedures and the man of science to make the relevant observations" (44). Had Titchener not so wholly committed himself to 'existence' and had he not so thoroughly "interwoven . . . . . observance, existence and observation" (45), his expository way would have been easier and more 'logical' and his account of science more internally consistent. He will not desert the ancient dichotomy of 'fact' and 'logic' and nevertheless logic will not stay out of, nor can it come in to, his science.

Because of its important consequences we must pause upon the corollary that all description starts with analysis. "It is universally agreed, then, that the first problem of science is analysis" (58). Whence the "then?" The only justification which the reviewer finds in the text is that direct acquaintance-with "can be gained only piecemeal, little by little." But when has this contention been "universally agreed" upon? Many men proceed otherwise. Indeed the prescription seems to rest rather upon some such logical assumption behind observation as a doctrine of physical or of mental 'elements' or as a genetic theory of the derivation of the complex from the simple than upon necessity, either logical or scientific. It falls in nicely with the older methods of teaching chemistry and, in psychology, with a first descriptive chapter devoted to sensations; but it stands squarely against the author's own conception of biology, where the unit is the organic individual, not to be known characteristically by an analytical split-up but by the observation of its total dependence upon 'environment' (see below). In Titchener's own complacent and individualistic psychology analysis was a cardinal principle of procedure, a principle to be enforced by declaration and by precept; but when it is arbitrarily prescribed for all science that is 'really scientific' it wears the appearance of a dogma.

The fundamental sciences he assumes rather than shows (87) to be three, physics, biology and psychology. All are at bottom observational. While they must combine 'logic' with observation, logic is always secondary and the observational acquisition of facts primary. Their common ground is 'existential experience.' In psychology this experience is logically dependent upon the nervous system, in biology it is logically dependent upon a physical environment, and in physics it is logically interdependent but externally independent. The three subject-matters are diverse but related. On the 'material' side,

physics is energetic, biology behavioral, and psychology sensory; on the 'formal' side, physics is non-individuate (or universal), biology is individuate, and psychology is systemic, *i.e.* it correlates with a single organic system (264). When stated in terms commoner in the history of the sciences, the objects of physics comprise an entire 'order of nature' with interdependent masses and energies, the objects of biology are limited to individual living beings which sustain relations of dependence upon non-living objects and agencies, and the objects of psychology are sensory phenomena which appear only in conjunction with neurological (*i.e.* biological) objects and processes. In still grosser terms we may say that the materials of physics are all-of-a-piece and interactive, those of biology are, so to say, variegated clots of existence which presuppose physical objects and agencies, while those of psychology are sensory facts functionally related to the body. Titchener is extremely non-committal on the "material characterization of psychological subject-matter," only venturing "sensory" as a "leap in the half-dark" (265). It is extraordinary that when he comes to the momentous question 'What after all is the stuff of which psychology is made?' he virtually gives up with a thin page and a 'postponement' which must be "altogether indefinite" because "the very nature of these facts is in dispute" (265).

Titchener's conception of and his practice in psychology are thus substantiated by a generalization looking toward biology and physics. Here British science, from Spencer and Darwin down to Karl Pearson, is chiefly called to witness. If psychology demands an observational description of the existential sort, so do physics and biology. Thus runs the argument. But the argument shifts in the last two chapters, where definition is the main theme. Here physics tends to take the lead and biology becomes a major problem. Psychology then is given dignity and rank by its ability to fit into a place coördinate with these other subjects.

One of the main objects of the book seems to be to bring biology into the general logic of the sciences; and the author thinks that he has solved this problem in a new and satisfactory way. To relate physics and psychology, proposals in plenty (such as they are) have been made; physical and spiritual, extended and non-extended, conscious and non-conscious, mediate and immediate, and many more. He reviews them all and finds none satisfactory and none that leaves a coördinate third place to biology. So he proceeds "to write a differential formula for psychology in terms of its relation to physics and to biology" (88). Physics itself gives superficial and naïve solutions. Philosophy and psychology have taken the task more seriously. There are proposals by Wundt, Avenarius, Mach, Ward, Külpe, Ebbinghaus, and James. That of Avenarius is most engaging. His proposal to throw over a special subject-matter (consciousness) and to define by point of view is, as Titchener writes, "satisfactory; and yet we cannot say that we are satisfied" (137). Avenarius's 'experience' included value. Here Wundt is "a far safer guide" as he leads toward an existential experience. If we then regard this sort of experience as dependent upon a determinate biological system (nervous system) we shall attain a 'satisfying' point of view for psychology. Furthermore, biology's point of view is of the same sort; existential experience regarded as dependent upon a determinate *physical* system, *i.e.* environment. This last dependence Titchener looks upon

as a discovery "not hitherto . . . . . proposed" (139). Yet he cites Charles Darwin and L. J. Henderson and he talks of 'evolutionary process,' a functional 'complement' to the organism, 'maximal freedom,' and 'suitability.' All operational, functional, adaptive terms! The biological concept of 'environment' would seem to be the last that Titchener would have consented to join with an 'existential' point of view. He must have been misled by Henderson's physico-chemical terms and he must have overlooked Henderson's 'suitability.' Again, it may be doubted whether this is really "definition of biology by point of view" (141). After all it is the *organism* which is environed and without the organism there is (as Titchener agrees) no environment. The organic subject-matter would seem then to determine biology and the point of view to have been secondarily derived from the operational notion of a sustaining and 'promoting' environment. This is by no means (nor has it been since the days of Lamarck and St. Hilaire) the *logically* dependent member of an existential relation.

But if we could grant that this is really a logical dependence and is a true determination by point of view, we might still ask whether the science of which Titchener writes is 'biology.' In some passages, biology is obviously the 'general biology' of evolutionary fame, in others Titchener seems to be providing in general for the sciences of life, and again he has before him the living individual in its inorganic setting (biological existence is "individuate and behavioral" (263-264). Titchener cannot abide 'behavior,' which is valuational, and 'life,' which is institutional and practical. His own terms are softer and the reader may not at once discover their real import. Had the author dealt frankly with his sustaining environment he would have taken a hint from his behavioristic enemies and provided for the opposing term of the interaction, thus bringing in *response-to* as candidly as he had *stimulation-from* the environment. His demand for the existential everywhere blinds him to the double intercourse of living things with 'physical nature.' When we recall his own stand for 'sensationalism,' with its British affiliations, we can readily apprehend his liking for the 'impressional' side of matters, both in psychology and in biology. In fact his conception of existence seems to imply a description of the passive and the receptive. To be sure, he makes physics energetic; but then he does not work out his physics and especially show how a subject-matter energetic in form might be existentially described. And had he seen that the environmental relation is only a special case of *text* and *context*, of dealing with the materials of science as variously related, he might have provided for physiology, anatomy and embryology as well as for ecological aspects alone of living beings. Internal contexts, related organs and systems, and developmental stages are quite as important (whether considered in 'biological' or in 'physico-chemical' terms) as the outside dependence, which Titchener vainly tried to make purely 'logical.' In one place (262) environment is made to include "the organism itself, in so far as organic phenomena are not individuate but energetic." This troubles Titchener's logic, for a thing can scarcely be 'dependent' upon itself. He provides here neither for those biologists who *biologize* internally nor for those others who regard their whole business as plain physics and chemistry.

His obsession with British biology of the Darwinian type led him to stress a purely local and practical line of demarcation at the rind and limit of the body to the neglect of a general distinction of text and context which would have

carried straight through his three sciences. The context for psychological facts would have been bodily and for biological facts both bodily and inorganic, while physics would either have supplied its own context, or been supplied from biological and psychological sources, or have remained (when taken as an entire system) only text. And without his heavy concern for 'biology' the arbitrarily assumed trinity of the sciences might have been dropped and such subjects as physiology, geology and galactic astronomy accorded a judicial hearing.

Those readers who know the author's intellectual history will probably agree that the main interest served in the chapters carefully brought together by Professor Weld is the contention that science is the existential description of experience. The contention involves an observer who is in dissent from, and in revolt against, many cherished views and interests of man. The observer eschews the practical, the technological and the valuational, and he identifies himself with the immediately observed. The observed is always experience and experience regarded as 'functionally or logically' dependent or interdependent. We know what all this meant in terms of Titchener's psychology. It never covered the entire field save by the author's arbitrary delimitation. Were he to apply it to his science of the organism, it would seem to mean a kind of passive ecology. What it might produce in the physical sciences is very difficult to say. In fact, it is not at all certain that the common basal term *experience* carries the same meaning in the author's three sciences. He nowhere shows that it does. Although most men who are devoted to the sciences would like to think of their work as a serious, straightforward and objective business, I do not believe that we should find in most of the works of science very much which conforms in strictness to Titchener's formal conceptions. And it is a striking fact that our author was himself a collector, a classifier and a critical arranger of facts, men, objects and systems—all matters (as he says) of logic. Even in his own experimental problems he delegated most of the observing. This very proficiency in sorting, distinguishing, bringing under rubrics and setting into order seems to the present reviewer to have determined the main virtues and uses of the book. The book is full of information well arranged. The psychological reader who fails to understand it, to delight in its diction, to be vastly informed by its historical lore, and to be quickened by its logic in his own scientific and professional thinking cannot reasonably regard himself as a master in his subject. Whether the *Prolegomena* would have led on to a rounded and useful 'system' can scarcely be predicted. Since they fairly present the author's general background for his earlier decades, it may be well to let the present essays stand as a reasonable and enlightening *apologia* to the *Outline*, the *Textbook*, the *Experimental Psychology*, and to much beside in the author's large contributions to his subject.

Cornell University MADISON BENTLEY

The Psychological Register. Edited by CARL MURCHISON with the cooperation of an international board. Worcester, Mass., Clark University Press, 1929. Pp. ix, 580.

The *Psychological Register* is a Who's Who in Psychology. In it are listed, with a few notable exceptions, the names of all the psychologists in the world

of sufficient standing to merit recognition by their confrères. Besides biographical items—full name, address (usually of 1927), place and date of birth, academic and professional history—the *Register* gives a more or less complete bibliography of every individual.

The names are arranged alphabetically by 'country' and alphabetically within every one of those divisions. The classification by 'country' does not, however, follow any fixed principle. It is based upon the political state in which the individuals reside, as for example, Austria, Belgium, China, Czechoslovakia, Denmark, Finland, etc.; but this principle frequently gives way to linguistic and geographical considerations. The free State of Danzig is included with Germany, Portugal with Spain; Canada is grouped, not with the other Dominions under 'British Empire,' but with the United States under 'America;' Argentina and Brazil are grouped together under 'South America,' though they are the only two states represented from that continent. The psychologists of the British Empire, with the exception of the Canadians as noted above, are grouped together in a single alphabetical classification. It is, consequently, difficult to obtain from the *Register*, without much labor, a list of the psychologists residing in Australia, England, India, New Zealand, Scotland, South Africa, and Wales. The *Register* would have been more serviceable had a single principle of classification been employed—preferably on inclusive alphabetical arrangement followed by a geographical index.

The volume closes with an index of names; consequently the reader may find the relevant facts for any given individual though he may not know beforehand the country in which the individual lives or is listed. The expedient thing to do when looking for an individual is to turn immediately to the index. Among the names that the reviewer misses are I. P. Pavlov of Russia, Sigmund Freud of Austria, C. G. Jung of Switzerland, E. Rubin of Denmark, F. M. Urban (formerly of the University of Pennsylvania) of Czechoslovakia, R. H. Wilcocks of South Africa, G. Humphrey of Canada, J. E. Evans, G. M. Fernald, D. A. Laird, D. B. Leary, and A. H. Sullivan of the United States of America.

The *Register* is printed in small type (8 pt. solid), and in double columns. The authors' names are printed in boldface, and stand out distinctly from the body of the page. The surnames are, as a rule, printed in capitals; the given names, in capitals and lower case. Many exceptions were, however, inadvertently made. The prefixes, De (pp. 56 f.), Le (p. 385), Mac (p. 144), Mc (pp. 149 f., 328), etc., are printed in upper and lower case. This is inconsistent with good usage; small capitals instead of lower case letters should have been used. Again, surnames with accented letters are printed in large and small capitals, the accented letters being printed small (cf., for example, pp. 370, 486, 539). The effect is unpleasant and good usage is further violated. The editors should have insisted upon the printers using type of one size.

The biographical facts given after the individual's name are sufficient for ordinary needs, the salient items reported showing the individual's training and the extent of his influence. Information concerning marriage and children might profitably have been given. The biographical information is printed in English, French, German, Italian, or Spanish; other languages are usually replaced, though not always, by English. In the reviewer's opinion it would

have been better had English been used throughout; the book is published in English and it will be chiefly used by English-speaking people.

The bibliographies (translated into English in the case of Russian, Japanese, and Chinese, and given in the original in the case of other languages including the Scandinavian) are uneven and incomplete. Most of them were furnished by the authors, but many, as the Preface relates, had to be prepared in the editorial office. The latter are naturally less complete than those prepared by the authors, but they suffer from a further limitation in that they do not extend back of 1894, the date of the first *Psychological Index*. The bibliographies prepared by the authors differ also in completeness. Some of the authors, in accordance with the editors' request to list only important studies, prepared a selected bibliography; others, ignoring it, included everything they had written. The request for a selection was unwise, not only because it made for inequalities, but also because it eliminated from the bibliographies of those who complied with the request the very items that usually escape indexing, and that are, as a consequence, difficult to discover.

The limiting dates of the bibliographies vary from 1927 to 1929. Here, as elsewhere in life, the men who were on time suffered; bibliographies which were returned when called for were cut short at 1927. The dilatory, who returned their bibliographies late or who had their bibliographies prepared in the editorial office, profited; their bibliographies were brought down to 1928 or 1929. Good editorial practice, to say nothing of justice, demands that the limiting date should be the same for all.

The references given in the bibliographies are frequently incomplete. Sometimes the date, sometimes the volume number, and many times the page references are lacking. Where page references are given, various styles of citation are followed: inclusive pages, beginning page and dash, or beginning page alone.

The American section, which runs to 296 pages and forms over half of the book, is the best edited of all the sections. It is not, however, free from error. All the errors mentioned above find ready illustration in it. The most frequent error, however, is the omission of pages from book references. The poorest edited sections of the book are the Austrian, in which the various styles of citation follow one another in haphazard order (cf. p. 303), and the Belgian and French, in which volume number and page are omitted from most of the citations. The differences in editorial style should be smoothed out before the appearance of a second edition. Date, volume, and inclusive pages should be given in every reference.

The proofing was on the whole carefully done. The errors noted above are all vagaries in editorial style; they cannot be charged to the proofreaders. The following errors must, however, be charged to them. Wrong cases are used in printing the authors's names on pp. 51, 299, and 302. Accents are omitted in Hoernlé (p. 324) and Juhász (p. 489). Accent and 'von' are omitted in Stephen von Máday (p. 490). The umlaut is omitted in Charlotte Bühler (p. 297). J. P. Nafe's first article (p. 168) is on the 'affective,' not the 'effective' qualities. P. C. Squires (p. 222) is incorrectly placed as at the "Univesity of Kansas in Los Angeles, Calif." This shift of the University from Lawrence, Kansas, may be a bit puzzling to non-American readers. E. W. Scripture

(p. 210) and G. H. Wang (p. 262) are incorrectly included in the American section. As their bibliographical data clearly indicate, they belong respectively to Austria and China. A few names, moreover, are incorrectly alphabetized (pp. 161, 196, 361, 377, and 402).

The *Register* nevertheless is a valuable book of reference. It cannot of course be used to the exclusion of other indexes; but it supplements them. It gives information that can be found in no other place. The psychologists of the world owe the editors a debt of gratitude for undertaking its publication.

Cornell University KARL M. DALLENBACH

An Historical Introduction to Modern Psychology. By GARDNER MURPHY, with an appendix by HEINRICH KLÜVER. New York, Harcourt Brace & Co., 1929. Pp. xvii, 470.

There has never been a thorough-going history of modern psychology, of psychology as it exists to-day—mostly in laboratories—in its historical setting. Brett wrote of it in his third volume, but not from within it. Klemm constantly touched it. Pillsbury has very recently included much of it. At first glance it would seem as if Murphy had supplied the need. However, he calls the book *An Historical Introduction to Modern Psychology*, and not the History of Modern Psychology. Is the title correct?

Essentially it is, but the book is also, in a way, a history that debouches from the past toward the present, so that when it is done all of the very most recent psychology has been arrayed along the entire modern front. Most histories begin to become dim within the point of near-vision; historical perspective, however, is not for Murphy an end in itself. Murphy wants to write about the present, and to put it, which has no perspective to itself, into a kind of perspective by showing it as the latest stage of a genetic development.

The first one-sixth of the book is all history and atmosphere. Murphy gives a bit of the intellectual background of the seventeenth century, devotes a chapter to the seventeenth and eighteenth centuries, writes another chapter of the nineteenth century on Herbart, phrenology, and the French and Scottish schools, and concludes with a discussion of the general intellectual antecedents of experimental psychology in the other sciences, in philosophy and in wider intellectual movements. If this part and the next of the book have been flavored by Merz's *History of European Thought in the Nineteenth Century*, that fact is no disgrace to the author.

The second section of the book, about a quarter of the whole, is called "From Weber's Experiments to the Age of Wundt." It deals with Weber and Johannes Müller, with Hamilton, the Mills, Bain and Spencer, with evolution from Erasmus Darwin through Charles Darwin and Galton, with psychiatry from Pinel and Mesmer to Charcot, and then with the experimental psychology of Helmholtz, Lotze, Wundt, Cattell, and a few others. Cattell is misplaced, and G. E. Müller and Stumpf are neglected, but in general all this part is history too.

The third and last part occupies a little over half of the book. While Murphy does not seem to me always to have been fortunate in his chronological divisions, this part can be said to consist of sixteen chapters each of which presents some current phase of psychology in its historical development from about 1880 or

some later date. The topics are: memory, James, structural and functional psychology, thought, learning, behaviorism, child psychology, social psychology, the psychology of religion, psychoanalysis, instinct, intelligence, personality, physiological psychology, and contemporary German psychology. The two chapters on the last topic form an appendix written by Klüver. It is apparent at once from this list that the book does not progress as a whole. One has in the second half of the book a collection of historical accounts for different topics. Perhaps this effect is inevitable, for history has the two dimensions of breadth and time, and language has only the one dimension of progression. In writing history an author cannot go both ways at once, and Murphy has not neglected to pick up connecting threads of woof as he advances along one line, although he can hardly be said to have woven the strands into a single fabric.

I am being critical, for such is the function of the reviewer. I think Murphy's book is the best history of psychology that has ever been published for the use of modern psychologists, and the reader must remember that he does not claim that it is actually a history at all. But, if perfection be the standard, he seems to me just to have failed of integration. This failure is due in part to the chronological 'zig-zag' which characterizes this latter half of the book.

The 'zig-zag' is also the method of progress within the chapter. One could take almost any chapter, but let us examine the one on Child Psychology (pp. 279-288). After a brief introduction one is started in with Preyer (1881) and in two pages has advanced to Thorndike (1914), then one is brought back to Stanley Hall (1904) and to Groos (1896) and forward to Piaget (the present), back to Witmer (1896) and forward to Watson (1917). Would not the student of the history of child psychology have to get out paper and pencil and rearrange all these men in chronological order to understand what had really been happening? Murphy writes principally about the last fifty years, and he writes of them, except for his explicit tracing of genetic relations, as if this half century were all one big conscious present.

We should next consider the scope of the book and hence the brevity of the treatment of individual topics. Murphy's scope is as broad as it well could be, if the more philosophical aspects of psychology are left out of account. One senses his interests. He moves away, where he can, from physiological and experimental psychology toward the social psychologies, from systematic psychology toward the applied psychologies, from the nervous system, sensation, perception, feeling and emotion toward psychoanalysis, instinct, intelligence, and motivation. He has the right to choose; no author could be omniscient. However, one feels that he prefers the broader reaches of modern psychology to the closer atmosphere of the laboratory.

On the other hand, he has by no means neglected the laboratory, and the resultant range of the book makes his treatment of most topics very brief. When I began the task of this review, I tried, with the malign instinct common to reviewers, to find the names of important men who were entirely omitted, and succeeded very badly. Nearly all the great are named. Then for a while I was admiring Murphy's condensation, but after a time this brevity began to pall. Hardly ever could I get any real information about any topic. William James has a chapter and is well done, and there are some other like places; but in general one moves on to the end, picking up crumbs with never a bellyful.

Well, what was Murphy to do? He could not write fully of so broad a field in four hundred pages. He made his choice, and I am merely recording my impression of the result without saying how the fault should be remedied.

I think Murphy is accurate, or else I must disclaim expertness in some historical fields where I thought I was well versed. In the entire book I have noted nothing that I should call a certain error of fact, except his placing (pp. 162 f.) of Wundt's tri-dimensional theory of feeling in 1893 when the date should be 1896 or 1902. This date is unimportant as Murphy mentions it; it could be important in fixing the effect of Wundt's theory upon contemporary research.

Murphy's literary style is unusually good as compared with most psychological writings in the journals, and in books too, of the present day. Of course, he has not the felicity of William James; none of us has. But he never calls attention to himself on account of his phrasing, and he has succeeded in never once offending the ear of a critical reviewer. He may have worked hard for perfection in an age when few psychologists do, and he ought to be congratulated.

Not so much can be said for Klüver's appendix. Klüver's treatment of contemporary German psychology has the ring of an expert, as those who have read his articles well know. However, the sentences are too long and the paragraphs much too long. There are at least two paragraphs (pp. 429 ff., 448 ff.) that are almost three pages in length. Of course Klüver is not writing in his mother-tongue, but that fact does not prevent his reader from feeling that the reading is more of a duty than a pleasure. Murphy leaves one comfortable if hungry: Klüver fills his reader but makes him wade for his dinner.

In another way, too, Murphy obliterates himself from the book like a perfect host, but it is impossible to read Klüver and to forget the author. For instance, Klüver gives long lists of names separated by commas—fifteen names in one place (p. 431) and eleven in another (p. 438). These names have no use. If the reader knows what the men in the given context stand for, he knows too much to read the chapter. If he does not know, he is certainly not being told, especially as there are no foot-notes with references, or even initials, to start him on an investigation for himself. The result is that the reader feels that the author failed to suppress a show of knowledge where its exhibition had no meaning, and he concludes that the author is not really his friend.

The book is not the best sort of reference work. There must be about nine hundred foot-notes in it and nearly all of these are references. These citations seem to be accurate as far as they go (though Klüver cites a misleading edition of Mach, p. 430), but they lack initials for the author cited and all page references. One gets surname, title and date only. Everyone knows how provoking it may be to be referred generally to a book and to be left to find his own page. Moreover, although there may be nine hundred references, there probably should be two thousand. One is left again and again without the desired documentation, either for a man's name or for a particular topic. Murphy has done well in this matter, but he could have done better.

One other matter that makes me uncomfortable in reading this book is Murphy's habit of translating the titles of books. The main discussion of Lotze does not even mention his famous book, but in two other places it is cited as his *Medical Psychology*. The *Medicinische Psychologie* was never even

translated, and, if Murphy is going to translate the words, why should he not translate the meaning too? Lotze did not write in 1852 a medical psychology as those words connote to-day; he wrote, in the phrase of his own subtitle, a *Physiology of Mind*. There are lots of other instances where the English title sounds odd (*e.g.* Descartes, p. 8; Leibnitz, p. 11, Herbart, p. 46, Preyer, p. 280).

All this is general. I can now turn to certain criticisms that forced themselves upon me as I read. The reader must, however, not misunderstand me. I can nowhere say flatly that Murphy is wrong. The very nature of my criticisms is a compliment to Murphy.

Fechner (p. 52), I think, got the notion of the limen from Herbart and the notion is not so different from Herbart's. Titchener thought so. J. Bernstein was influenced by Fechner, possibly through A. W. Volkmann, and his diagrams of the limen of consciousness closely resemble Herbart's.

It is quite proper to give Charles Bell his due for the law of the spinal nerve roots (p. 78), but it does not seem to me quite fair to reduce Magendie to fourteen words in a foot-note when the Bell-Magendie law is under discussion. Hardly anyone thinks that Magendie cribbed, even unconsciously, from Bell, and anyhow the introduction of the law into France is important, for France was then the important nation for scientific physiology.

Certainly Weber originated Weber's law (pp. 79-85), but a reading of *Der Tastsinn und das Gemeingefühl*, or of Lotze's and Bain's early comments upon the research, does not convince me that Weber attached the importance to the law that Murphy seems to believe. It really took Fechner to give the law its full significance.

Johannes Müller discovered the reflex in its modern form (p. 86), but he himself admitted Marshall Hall's priority and Murphy might mention Hall.

Carmichael would probably say that Murphy gives too much credit to Johannes Müller for the law of specific nerve energies and too little to Charles Bell (pp. 96-98), but I am not sure that I disagree with Murphy's evaluation. Bell in his obscure little brochure made less of this law than Weber did of Weber's law, and Murphy does mention Bell (pp. 96, 78).

John Elliotson has always seemed to me to be much abused, and I should like to see Murphy give more credit to him, and also to Braid, in his discussion of hypnosis (pp. 141-143). Furthermore, he connects Braid too much with phrenology, and he is hardly fair in saying that it was some time before Braid came to a clear opinion in the matter of 'neurypnology' (p. 142). Braid had reversed his opinion, learned how to hypnotize, and had formed the basis for the view that is attached to his name, within a few weeks after he first denounced the mesmerist, Lafontaine.

Murphy's only harsh phrase in the whole book is his statement that Hering made it his duty to disagree with some authority on everything (p. 153). German polemics do not appeal to the modern eclectic, but Hering does not seem to me to have got his due. It is as if the foot-note about the Ladd-Franklin theory had affected the whole discussion.

Wundt is a difficult topic for anyone to tackle (pp. 160-173), and Murphy seems to me to have done fairly well. There is the error about the date of the new theory of feeling (mentioned above), the repetition of Stanley Hall's

misstatement that Wundt was assistant *to* Helmholtz (p. 160), whereas he was assistant in physiology to give instruction to medical students independently of Helmholtz when Helmholtz was professor of physiology, and a wrong subtraction of dates which places Wundt in Leipzig five years (p. 162) before the founding of the *Institut*.

The introduction of a full discussion of Cattell's work (pp. 173-178) into the 'age of Wundt,' when his contemporaries are left for the modern period, seems to me to be in false perspective. The designation of Cattell as "the most original and productive member of the Wundtian group" (p. 173) is a courting of controversy. Really now, was Cattell more productive and original than Külpe, Meumann, Titchener, Kraepelin, or Münsterberg? I have always thought of Cattell and Münsterberg as persons who were *in* the Wundtian group but not *of* it. Perhaps this divorcement is what Murphy means by 'originality;' but then why does Cattell go into the chapter on Wundt when the others are left out and in some cases distributed elsewhere? Part of the trouble here is that Murphy's chronological 'zig-zagging' leaves the dates of the 'age of Wundt' only vaguely defined.

Murphy relates Ebbinghaus to Herbart but not, in any intimate way, to Fechner (p. 190). He should look up Ebbinghaus' dedication of the *Grundzüge* to Fechner: "Erkenn' ich wohl, ich hab' es nur von Euch." Ebbinghaus, without personal contact with any psychologists, discovered Fechner's *Elemente* in a Paris book-stall, was greatly impressed, probably took the book and the ideas to England where he came into closer contact with associationism, then, in England or France, began his experiments in an attempt to measure memory as Fechner had measured sensation. Of course, the expository structure of *Über das Gedächtnis* makes it look as if Ebbinghaus had gone to physics, and not to psychophysics, for method, but the exposition of that famous book can hardly be the history of Ebbinghaus' thinking.

Brentano is skimped (pp. 233 f.); he deserves more space. He not only set the stage for Austrian psychology, but he determined Stumpf. But then Stumpf is skimped too (p. 181), as is also G. E. Müller (four scattered paragraphs, pp. 168, 181, 197 f., 256 f.).

An entire chapter devoted to instinct (pp. 336-346) is a little shocking, when there is no chapter on visual sensation, or on perception, or on feeling. Is not attention as important historically as instinct?

Psychophysiology is in, but it is scattered. The picture of the nervous system that centers around the neurone theory comes in the chapter on Memory (pp. 199-204), where no one would ever look for it. Related material enters in other places, and hence much of the proper content for the chapter on Physiological Psychology (pp. 395-405) is not where it should be in this series of historical essays.

Murphy ends by summing up the entire history of modern psychology. It shows, he thinks, a tendency away from structures toward functions, away from elements toward *Gestalten*, away from the qualitative toward the quantitative, and in addition the development of the genetic and statistical methods. I cannot quarrel with him in this summary. I should, however, quarrel with his picking out, in this resumé, the normal law of distribution as one of the greatest laws of psychology (p. 413). Murphy still has the majority of psy-

chologists with him, but my guess is that psychology is being retarded by their error. For a similar reason he makes more of the method of correlation (p. 412) than I should dare to do. His final pages, emphasizing determinism and precision as the 'good' in scientific psychology, will meet with general approval, but he has missed here his chance to point out how all science moves toward the abstract as it develops.

Some of my criticisms depend on personal opinions. More reflect what I think the evaluative consensus to be. Only a few could be called corrections. There is a great mass of information in this book. The index gives 669 names. Murphy, it seems to me, has written an exceptionally good book, as these undertakings in psychology go.

Harvard University EDWIN G. BORING

Experimentell-deskriptive Psychologie der Bewegungen, Konfiguration und Farben unter Verwendung des Flimmerphänomens. By KURT HAACK. Berlin, S. Karger, 1927. Pp. viii, 263, 1 pl.

This monograph presents the detailed results of a minute phenomenological examination of flicker. The discussion is presented under the three main headings indicated in the title, the first of which has appeared previously.

Haack devotes 28 pages at the beginning of his study to the exposition of his problem and to a thorough review of the literature dealing with flicker. Then, in the first section, he discusses the various forms of movement experience that he has found to appear in a flickering field. He distinguishes first between movement (Bewegtheit) and the object moved (Bewegten) and further, in the case of the former, between direction of movement (Bewegungsrichtung) and rate of movement (Bewegungsgeschwindigkeit). The basis for the first distinction is the observation that movement may be seen without a moving object being noted (p. 29). He remarks, however, that Wertheimer's description of phi must be modified since even in the absence of a perceived moving object the "Etwas" or the "Bewegung" "die Qualität Hell oder Dunkel oder Farbig aufwies" (p. 31). On the other hand, the carrier of movement was frequently visible for brief instants now in one position and now in another. Various forms of the experience were reported with rotary movement when the center was perceived as the center of a plane circle (p. 35) or of a sphere (p. 45), and when the center of rotation was imaginary (p. 61).

The perceived direction of movement might be in any one of the three planes of space—frontal, sagittal, or horizontal—and sometimes occurred at an angle to one of these (p. 48). The latter, however, seemed to be a summated or 'principal' direction in the presence of more than one possibility of direction. Some combinations, of course, were more frequent than others. Numerous factors in the stimulus and in the observer's attitudes modified the form and direction of the movement. A non-rotary movement is reported also and characterized as "Strömen" or "Schneeflockenfall" (p. 82).

Although flicker can not be separated from a certain impression of movement it is independent of the rate of that movement (p. 84). We must not forget that the movement experience Haack is discussing is the movement in a flickering field. It is only as such that he is concerned with it since flicker is intrinsically labile (p. 88).

Haack proceeds in the second section to describe the various configurations (Scheinfiguren und Scheinkörper) which appear in the experience of flicker as the carriers of movement. The familiar figure-ground concept is the basis of the discussion and the configurations of various elements—points, Schneeflockenstrom; lines, ziczaclinie, Haken; surfaces, Landkarte, geometrische Figuren—are described in the same detail as the aspects of movement have been, together with the effects of attitude, size, position, etc. upon the configuration.

In the final section, Haack reports the color phenomena that appear in the field of flicker. The configurations of the flickering field tend to be imbedded in a "Medium" (p. 189 f.). This impression is most frequently given in terms of brightness and is characterised as "Rauchwölkchen," "Flöckchen," "feiner, zarter Dunst," "ein dünnes Blättchen." The brightness of the "Medium" is perceived as illumination (Licht) which may be either light (Hell) or dark (Dunkel) but the latter are not simply of the qualitative series black-gray-white. "Das Hell und das Dunkel ist etwa so, als wenn es ohne Weiss bzw. ohne Schwarz wäre" (p. 192). Variations of brightness are qualitative as well as quantitative and require a system of terms, viz., dunkel-hell-licht-weisslich-graulich-schwärzlich. Colors (die bunten Farben) always have a different localisation from gray. They either lie in front of the gray or are seen as though through a hole in it. Likewise their mode of appearance varies, —*e.g.* Tiefer und Breitenkohäsion und Adhäsion—and they come in such perceptual forms as shimmer, luster, glow, etc., and as one color seen through another.

We have in this monograph, then, a most exhaustive catalogue of what may be seen in flicker. Although the work is by title "experimental" a diagram of the apparatus is the only reference to the conditions under which the observations were made. While one cannot demand that a phenomenological study of this sort be given a statistical treatment, it is disappointing that no hint—unless it be the word 'flicker'—is present of how one may set up and observe the same phenomena. It is hard to escape the impression that the work though meticulous is casual.

Hobart College FOREST LEE DIMMICK

Changing Backgrounds in Religion and Ethics: A Metaphysical Meditation. By HERBERT WILDON CARR. New York, The Macmillan Co., 1927. Pp. 222.

The purpose of Professor Carr's "metaphysical meditation" is "to express definitely the consequences in ethics and religion of accepting the principle of evolution." He maintains "that all the phenomena of life belong to one evolutionary process" is a 'scientifically established' fact and that that is "the one fact in our scientific survey of the cosmos which offers us an analogy of what the universe in itself may be, the one fact which gives us the key to the construction of a metaphysics of existence."

Objective science discovers in nature two processes: "a process of cosmic evolution and a process of living evolution." Physics and chemistry deal with the first; biology with the second. The material of the first process is matter;

its principle, inertia; that of the second, life; its principle, activity. Cosmic evolution represents a descent. "To physics and chemistry the matter which constitutes the planetary mass which we are able to submit to scientific analysis is a stage in the degradation of energy." The direction of cosmic evolution is "from motion to quiescence, from activity to inertia, from instability to stability." The process of living evolution of life is an ascent. It is "continuous creation."

Now these scientific facts—matter and life—with their opposing principles, inertia and activity, show us only separate and one-sided pictures of the universe. Is there a possible way of reconciling them "as complementary aspects of one reality?" Physicists and biologists have "no clue to the relation between these two systems of objective phenomena, no interpretation of the opposing principles." Their primary concern is precise description of the facts in their respective domains. But "every scientific student is profoundly convinced that the universe is a rationally explicable unity." The question of the unity of the universe is not prompted by idle curiosity; "it is imposed on us by that appetition which is inherent in the nature of reason." Science rightly confines itself to the description of facts. Facts, however, must not only be described; they must also be interpreted and explained—but "to explain the universe requires metaphysics," therefore explanation "is the philosopher's job."

To explain the universe in view of the mentioned scientific facts means that metaphysics must "devise a rational scheme by which these two opposite principles, matter and life, shall be brought into unity in one concept," For dualism is not "satisfying to the scientific and philosophical conscience. In science and in philosophy mind apart from living body, living body apart from mind, are pure abstractions." To the biologist and psychologist the words 'life' and 'mind' denote forms of living activity and not independent entities.

The metaphysical principle that, in Professor Carr's opinion, can satisfactorily explain existence is life, viewed as creative evolution. It is the theory "that all actually existing living forms of activity are the present realization of a universal dynamic activity, manifesting itself as an urge or impulse or striving, not only one in its origin, but also one in the full extent of its multifarious process." This universal life-activity is actualized only in the individual; and it is only in individual experience that unity of existence can be found. Yet the individual seems to be of very small importance to the evolutionary creative principle revealing itself in him. His whole purpose seems to be "the carrying on of the life process beyond him"—in the creation of other individuals. After he has fulfilled this task he is cast aside. This indifference to the individual, on the part of the universal life activity, is found also in evolutionary religion and ethics.

It would be of considerable comfort to us if we could distinguish some purpose or end which the universal activity seemed to realize; but "there is no indication whatever, so far as the simple scientific facts are concerned, of a purpose or final end of living activity itself." Professor Carr's religious ideal of a perfected humanity, for instance, is not such an end. Our values, whether aesthetic, moral, or religious are not independent of us. They are not something outside or above the existing world; they are "the existing world in its

aspect to human experience." As to their future preservation the God of Creative Evolution cannot offer us any assurance. For this new God, supposed to be so superior to the old one, is the creative activity itself, for which "all human valuations are meaningless."

What, then, is the meaning of life when life is interpreted through Professor Carr's metaphysical principle. I believe the answer was happily formulated by the late George Simmel whose philosophy of life shows a similar orientation with regard to biology—"Die Tragik der geistigen Kultur."

University of Iowa BONNO TAPPER

Vom Problem des Rhythmus. By R. HÖNIGSWALD. Leipzig and Berlin, B. G. Teubner, 1926. Pp. viii, 89.

One of the most significant monographs on the concepts which underlie the rhythmic perception has appeared under the above title. Some graduate student well versed in technical German would do us all a service by translating it. The author makes no apologies for establishing his base line in philosophy in order to interpret more adequately the experimental results obtained in the psychological laboratories. Not only does he offer us, therefore, an analytical study of the concept but he relates it very ingeniously to a number of outstanding systematic points of view such as those of *Gestalt* and *Struktur*. At the same time the interrelation of the actual experience of rhythm as a mental reality and the theory of its interpretation is assumed.

The author clears the ground by telling us what he is not going to do. Then he proceeds to erect his own idea of rhythm as a psychological reality. Without doubt rhythm is fundamentally a *temporal* experience which is cognitively differentiated and unified. It is not a single membership of elements but an organization of members. But while it presumes the temporal aspect it is not ultimately defined in terms of this principle because time as such can be experienced as an entity only in relation to other experiences orientated in time. The experience of articulated time combines with the articulated experience of time and becomes the ultimate background of rhythm. This fundamental fact next suggests the observation that rhythm can be scientifically envisaged only through a mentally produced objective relationship. Here again we find the inevitable paradox that from the dynamic point of view rhythm must take its place among other cognitions as an act of perception as well as a referable circumstance in the environment. Subjective and objective are melted together into one alloy. Rhythm is, therefore, founded on the relation between experience as such and that which is experienced.

This leads us at once to the next important consideration that rhythm is not a sum of elements but a whole. It is a product which though mentally experienced and organized appears to be objectively conditioned. From this point the author weaves his concept through various circumstances and interpretations. Sensation and stimulus, psychology and physics, mental activity and objective conditions are compared and differentiated. In passing, a number of concepts are interpreted such as, configuration, partial configuration, and special configuration. The problem of empty time is also faced. Not only are there sallies into present day points of view but even Leibnitz is made to

contribute to the concept of 'monadity' which, as far as the reviewer can gather, is an expression symbolizing an abbreviated cognition of a kaleidoscopic world as it is mentally unfolded from within. In this wise especially in rhythmical performances is the cognitive element also unfolded, ultimately bringing rhythm into a close approximation to the *Struktur* or personal self. The importance of characteristic personal rhythms is emphasized not only in the industrial world but also within the latent habits of association and thought. The last touch is given to rhythm in terms of memory and recall. When it is finally delineated it emerges as an entity which requires reproduction for its existence.

Thus we are brought to the end of a monograph which provides us with one of the keenest analyses on the theoretical level which we have had thus far. It symbolizes also a type of theorizing which is not very common in this country although there is some hope for it since physics has furnished us so good a precept as well as an example. For, according to the author, philosophy is in the broadest sense the underlying theory of objective conditions while psychology in a narrow sense is simply a phase of this theory. The monograph shows a lively style, deep insight, broad scholarly orientation, as well as vertical thinking!

University of Iowa CHRISTIAN A. RUCKMICK

Zur Psychologie der Tiere und Menschen. By W. BETZ. Leipzig, J. A. Barth, 1927. Pp. xi, 206.

In this delightfully written treatise—completed by the author's confession in ten weeks—we have a running account of mental development from the lowest forms of animal life to the highest, including man. The author was interested in developing his psychology because he soon became convinced of the relative unimportance of consciousness. He makes much of the fact that there is a considerable amount of 'intelligent' activity without consciousness, that a large portion of the brain is anaesthetic and that the reflex adjustments of the body to rotation and position are not found in experience. Even learning in the conditioned reflex occurs without conscious effort. This develops into the curious situation that one can tell, in many instances, what one thinks but not how. When the author says that a person can be awake but not conscious our opinion is confirmed that he is using the term conscious in the sense of reflective experience and does not admit the lowest forms of cognition.

The chapters are replete with suggestive material most of which betrays itself of an origin which is nevertheless stimulating. Perception, sensation and feeling, general orientation, recognition, concept, idea, social phenomena, conditioned reflex, instinct, learning, and the thought processes are included in the chapter headings and give us some idea of the comprehensiveness of treatment. These comprise only one half of the treatise. The other headings cover such subjects as speech, the variability of instincts, and intelligence and mind. They suggest applications that have been worked out through the whole animal series. Throughout the work there are many peculiar analogies. The Chinese pagoda, for example, is supposed to have obtained its characteristic shape originally from the trees which our progenitors, the anthropoid ape, knew so well.

It is a matter of regret to the author that man has attained so large a size. We could be so much better provided with food if we were the size of cats or even of mice. The only difficulty would be the unevenness of combat with large animals, but chemists among these diminutive men could devise odors and gases which would make the world safe for mice-men. There is much organic development which is profitless and impractical for even the brain could be much reduced in size.

Many of the analogies are ingenious. The difference between our inherent reaction to visual objects as such and our responses to a world of pictures is naturally brought out by the device of going about looking at our environment through a frame of ground glass as in a camera. In the last chapter intelligence is reduced to a reflex mechanism which reacts to situations as a whole and in the rough. The only new qualifications are adroitness of response, elasticity in adjustment and refinement of adaptation to environment. But he holds no great hope for the improvement of intelligence over the next few millenniums. One must be satisfied if it does not essentially deteriorate.

The book can not be taken as a scholarly treatise but merely as a survey. Many of its conclusions may be answered or doubted but most of them have a certain plausibility when framed within the compass of their specific application. The range of treatment is enormous. A typical instance of the sweep of the imagination based upon fact which is displayed in the book is illustrated by the observation that it is probably impossible for fish to see spatial objects yet man can turn space into time in the theory of relativity and he can transplant geometry into algebra!

University of Iowa CHRISTIAN A. RUCKMICK

Histoire de la philosophie. By ÉMILE BRÉHIER. Paris, Félix Alcan, 1926-28. Vol I, pp. 791.

This work, of which only the first three instalments have yet appeared, represents an attempt to approach the problem of the history of philosophy with a minimum of assumption concerning its origin, its autonomy, and its progressive development. The first instalment covers the period from Ionian natural philosophy to Aristotle and the Lyceum; the second continues the exposition to Neoplatonism and the intermingling of Hellenic and Oriental ideas at the dawn of the Christian era, while the third, which is devoted to mediaevalism and the Renaissance, brings the discussion down to the end of the sixteenth century and completes Volume I.

While recognizing with Hegel and Comte (in contrast with an eclecticism or an unfruitful scepticism which finds historically only the strife of competing sects and systems) that there is indeed a unity of the human spirit which links the past to the present, Bréhier purposes to explore the material presented chronologically in order to ascertain if possible what the law of connection may be, rather than to exploit the data for the illustration of any preconceived law. His investigation proceeds accordingly on the basis of minute special studies of the various authors and epochs and hence is furnished for reference with an adequate supply of scholarly footnotes together with a well-selected working bibliography for each chapter. This plan is carried out consistently

throughout the work with a thoroughness which can hardly be indicated in a merely preliminary review.

One is continuously impressed by the penetration of the author into the central meaning of a given doctrine which, with his sure grasp of details, enables him thoroughly to organize his material with a welcome freshness of treatment where a merely cut and dried recital of doctrine would not be unusual. A reader would, for example, hardly look for more than a stereotyped account of the fragmentary teaching of pre-Socratic philosophers, and yet Bréhier reinterprets them all in a highly interesting manner, illuminating even some of the obscurer utterances of the "gloomy Heraclitus." The volume is in general the product of conscientious, mature scholarship and, without descending to the plane of a first seller, is readable by anybody who wishes to read a book on this subject.

Vanderbilt University HERBERT SANBORN

Cours de morale a l'usage des écoles primaires supérieures et des cours complementaires (1re année). By FÉCILIEN CHALLAYE and MARGUERITE REYNIER. Paris, Félix Alcan, 1927. Pp. viii, 232.

As life comes from life so, we are informed in the preface, the moral life in every individual is born of the moral life of those who have preceded him. Man can no more make his own morality than his geometry. The great moral leaders of all ages—Buddha, Confucius, Socrates, the prophets of Israel, Jesus, Luther, Rousseau, Tolstoi, etc.—take and emphasize some large idea more or less directly from tradition in their own social milieu, and they make it valuable for mankind of all countries and of all time. One then, oriented toward the future, confronts his own personal experiences with ideals which he has received from society. Thus, we read, the moral life "ought to be the product of social traditions and of individual experiences and reflections." The two virtues which the authors think ought to be the most highly esteemed by future humanity are love of work and fraternal goodness or kindness (*bonté fraternelle*). The former of these "imposes the duty of exercising an indisputably useful profession, of honoring all workers, and of demanding for all of them a situation materially and morally satisfying. The maximum of well-being and culture." "Fraternal goodness, condemning violence in relations among men and among different classes, orients all conscious endeavor towards an ideal of harmonious humanity."

Thus from a good beginning in the preface the authors run more and more into general formulas as to duty, and later they go into sentimentality, as is probably natural to do with youth. They "expound this code with optimism and confidence in human nature." Convinced that morality is a matter of general sentiment rather than of cold logic, (it is characteristic not to see any middle ground) they aim less to demonstrate than to motivate. The course which they outline for the children and adolescents consists of an introduction on the superiority of man over animals and on the good will conscientiously striving for accomplishments in the way of just acts; twelve chapters on individual life—courage, love of work, love of truth, sincerity, modesty, etc.; four on the family—duties to parents and to various members; two on the school—duties

to the teachers, respect for rules, amity and comradeship; two on professional duties—choice of a profession and the professional consciousness; and eight on society, along the same line—generosity, respect, politeness, etc. The lessons consist of short, usually sentimental, expositions followed by résumés, 'thoughts,' and 'readings' from famous poets and other great men, and questions for personal work. It is the Sunday-school type of course, and is as blind as such courses usually are to the inadequacy of mere formal exhortation and of the arousal of sentiments about right doing, and as disregardful of the means of attaining the desired well-being of all. This is indoctrination rather than education. It would be interesting to obtain by some reliable means the actual effects upon moral conduct of such instruction, much of which is mere propaganda. Some very good selections are included in the *Cours*.

J. P.

L'orientation lointaine et la reconnaissance des lieux. By ÉTIENNE RABAUD. Paris, Félix Alcan, 1927. Pp. 112.

The author assimilates into a coherent whole the facts so far obtained by himself and by other investigators on the means various animals have of regaining their nests. The method of attacking the problem was threefold: observation in a natural setting where the investigator remains passive, experimentation in a natural setting where the investigator may partially control the conditions, or experimentation in the laboratory where the total situation is controlled.

Rabaud is emphatic in dispelling any interpretation that hints at a mystical sense of orientation through a means not known to man or not consistent with the structure of the animal nervous system.

The predominance of particular sensory cues employed by different animals depends on the type of organism or on the particular circumstances within a particular type. Bees use visual cues to bring them to the immediate environs of their hive while olfaction guides them within the hive. Ants that travel in columns are stimulated by the odor of the trails they have made. Solitary ants use vision. Molluscs resort almost exclusively to tactual cues. The discussion of the orientation among vertebrates is brief, being limited chiefly to pigeons. Here vision plays the largest rôle with the secondary support of kinaesthesis.

The behavior of invertebrates and vertebrates is fairly similar. They both employ various sensory cues that must be in sequence but not necessarily in a chainlike order as the suppression of a particular cue does not inhibit the return to the nest. The orientation in all cases seems to be configurational. All cues have some bearing on the total situation. The nest also is not an isolated landmark but belongs to a total setting.

Two points of interpretation by the author cannot be accepted, the one that images of various sensory cues are registered within the nervous system, and the other that these images serve in a memorial function.

One admirable feature of the book, however, is the tendency of the author to realize that the environment of a particular animal is peculiar to him and need not be at all the setting as we perceive it.

Sarah Lawrence College THEODORA MEAD ABEL

Reconstructing Behavior in Youth. By WILLIAM HEALY, AUGUSTA F. BRONNER, EDITH M. H. BAYLOR, and J. PRENTICE MURPHY. New York and London, Alfred A. Knopf, 1929. Pp. xi, 325, ix.

Any academic psychologist who becomes discouraged by the baffling nature of the problems which he faces should read this book for spiritual refreshment. He will soon discover that the difficulties which confront him in his efforts to devise apparatus, construct satisfactorily valid and reliable tests, or even to keep up with all of the new departures in statistical procedure are insignificant and trifling in comparison with the apparently insuperable obstacles which the field worker of the social agency confronts who seeks to utilize the world's supply of psychology in the reconstruction of delinquent youth. Here is the story of what was done with 501 cases, and what came of it all. The different kinds of delinquencies are segregated. What was done with them is told along with reports and summaries of successes and failures. There are chapters on the technique of child placing, on the selection of foster parents, their training and their supervision, and on many other such matters. The appendix presents two cases in complete detail and a full statistical summary. There is wisely no attempt at psychological generalization; and yet one cannot read these cases, with the tales of superbly tactful handling along with the occasional frank admission of blunders, without a feeling that much progress is being made. There is properly no psychological bias, but an evident desire to utilize every possible scrap of psychological information, whatever be the source, which may in any way help in the solution of the problems.

One is inclined to be very generous in commenting on such a report—too much so, perhaps. It certainly ought to be of service and a source of inspiration to many. At times, as he read, the reviewer wished that the book might have been written by one hand instead of by so many. It was to that multiple authorship that he attributed the repetitions and what seemed at times a failure to achieve an agreeable smoothness and ease of organization and presentation.

University of Oregon — EDMUND S. CONKLIN

The Study of Religion in State Universities. By HERBERT LEON SEARLES. University of Iowa, Iowa City, Iowa. University of Iowa Studies, First Series No. 141. *Studies in Character*, Vol. I, No. 3, 1927, pp. 91.

This study presents itself in two divisions: (1) the presentation of material to prove the highly dubious point that there is, in its incipiency, a science of religion; and (2) the collation of matter to prove that the former impossibility of teaching religion in public schools is surely resolving itself into a possibility and a desirability. Under the first division, and in conformity with Karl Pearson's definition of scientific method, it is alleged that there is a science of religion consisting of fact-gathering and fact-arranging, deductions of hypotheses from arranged facts, and *disinterested* inquiry. These three features of methodology would make use of the history of religions, the comparison of religions, the philosophy of religion, and the psychology of religion (if any such exists over and above the subject-matter contained in the other three approaches). The method would cull much from literature, archaeology, anthropology and ethnology, philosophy, and psychology.

The history of the status of the teaching of religion in public schools in this country is given at length. Constitutional and other legal sanctions and prohibitions in the various states are presented. The present status in colleges and universities, together with growing changes in it in the form of *trends*, is pointed out. Some thirty-seven state universities, or their adjuncts or subdivisions, it appears, are engaged in this type of work, in one form or another. The thesis of the study is: if all knowledge is the domain of the university or college, it cannot fail to encompass that portion of knowledge which relates to religion.

The paper contains, besides Foreword and Acknowledgments, eight chapters, eighty-one references, and four appendices. The appendices relate respectively to which state universities engage in the work; questionary data from teachers as to the advisability of such teaching, difficulties met with, etc.; constitutional and other legal restrictions upon such teaching in the various states; and the plans now being followed or being formulated for use in the University of Illinois, Kansas School of Religion, Ohio Union School of Religion, University of Oklahoma, Wesley College at the University of North Dakota, Bible College of Missouri, Michigan School of Religion, and the School of Religion in the University of Iowa.

The whole monograph creates in the mind of the reviewer the suspicion of widespread, but none the less dignified and pious, propaganda, put forth by zealots and politicians, within and outside of college faculties and administrations, to cater to forces which seek to preserve from a natural and well-deserved death the moribund and decaying ecclesiasticism and credistic denominationalism in which most of us grew up.

University of Oregon H. R. CROSLAND

Mouvement et pensée. By M. G. PEUCESCO. Introduction by Charles Richet. Paris, Librairie Félix Alcan, 1927. Pp. 170.

In this simple presentation we have an attempt to explain the highest mental processes of thought as an outgrowth of the reflex arc. When the reflex is inhibited and the refractory period is interposed an interlacing of reflex arcs occurs whose output is a resultant similiar to the total reflex action. These inhibitions together with the refractory period are due to the vibrations which counteract one another producing oscillatory movements in the cells of the brain. Thought is, therefore, reduced to a vibratory movement but of a somewhat mysterious sort. While the work aims to be grounded in physiological psychology and avoids altogether a psychological animism the sources and movements described have more of a biological than a physiological connotation.

There are several schemes illustrating the convergence of neural paths and their resultant in terms of a single form of energy. In the final chapter the author pleads for an integration of the mental life based upon this interplay of vibratory movements. The highest form of reasoning then is derived from a mutual interference of neural currents illustrated on the lower plane in terms of motor inhibitions and the refractory period.

While the author cites histological data and physiological researches from numerous investigators the general tone of the book is theoretical.

University of Iowa CHRISTIAN A. RUCKMICK

Die Gefühlswerte: Grundriss einer Psychologie der Tiefe. By F. Noltenius. Leipzig, J. H. Barth, 1927. Pp. 352.

In this book the author presents his conception of the nature of psychology and especially the significance of affective states in the development of the various systems of evaluation regarded as a basis for social control. There is an analysis of psychological problems on the basis of their theoretical importance, and in this the point of view is genetic and social. In addition to a critical analysis of the term 'mind' (*Seele*) there is a historical account of the way in which this term has been modified in its usages in medicine, art, ethics, logic, and religion. Through all these chapters the author makes application of his conception of the nature of the affective processes. From the more intensive affective processes of primitive man, the author passes into their relationships to modern conceptions of value and the cultural processes upon which they are based. There is a sharp discrimination between the good and the true as resultants of affective processes, and particularly as derived from the series of which unity and totality may be regarded as limits. The technique which the author uses corresponds in some respects with psychoanalysis, but the deviations must be considered before attempting a critical evaluation of the method.

The description of the abnormal phases of psychology, while not based upon the results of careful analysis or experiment, are of interest to those working in the field of the psychology of religion. For the American reader the book will be regarded as polemical rather than scientific.

Ohio State University — Albert P. Weiss

Angewandte Psychologie. By Emil Abderhalden. Berlin, Urban & Schwarzberg, 1927. From Abderhalden's *Handbuch d. biol. Arbeitsmethoden, VI. Methoden d. exper. Psychol.*, C, II, 1-744.

This number of the Handbook of Biological Methods is devoted to applied psychology. It represents one of the most extensive contributions to this field. It is restricted mainly to the German industries and is made up of and describes carefully the various tests, the apparatus, and the instructions and test blanks that have been used in the various investigations. No attempt can be made in this review to list the many different studies that are reported. All phases of applied psychology are treated, and it would seem that the workers in this field could not afford to overlook this volume of the handbook.

Ohio State University — Albert P. Weiss

Your Growing Child. By Bruce H. Addington. New York, Funk & Wagnalls Co., 1927. Pp. 405, ix.

This is one of the ever-increasing number of books written for parents. The book is written informally rather than in the more technical style of the scientist. The importance of early training in the home is emphasized in contrast to the influence of later training received in school. Throughout the book the influence of environmental factors is stressed and that of heredity minimized. The book is not intended for the student of psychology and consequently is of little value to him.

Peabody College — S. C. Garrison

Experiments in General Psychology. By NORMA V. SCHEIDEMANN. Chicago, Univ. of Chicago Press, 1929. Pp. 108.

The book is intended not as a laboratory manual for experimental psychology, but it purposes to "give the student an opportunity to experience, in a systematic manner, various phases of mental life." The 52 experiments, illustrative of parallel textbook material, cover sensory phenomena, imagination, illusion, emotion, attention, memory, learning, association, suggestion, dreams, personality, and reasoning. The apparatus required is extremely untechnical and can be readily improvised by the student. Directions for procedure and space for tabulating, recording or interpreting results are given for every experiment. Much of the material presented in this notebook manual would be superfluous in a course where there are classroom demonstrations; but it may be used as a guide to experiments which can be performed individually. It includes, however, much that is trivial and scarcely on a level with usual experimental procedure. It is to be regretted, also, that only one experiment is devoted to auditory phenomena. A collection of the common visual illusions with questions and explanations in terms of perceptive cues is a good feature of the book.

Florida State College for Women EMELINE MOUL

Manual of Psychology. By G. F. STOUT. Fourth ed., revised, in collaboration with C. A. Mace. London, University Tutorial Press, 1929. Pp. xix, 680.

This work is sufficiently well-known to require little comment by the reviewer, especially since no fundamental changes are introduced in the new edition. Professor Stout is aptly referred to, in the preface written by Mace, as "a representative of the central tradition in British Psychology." He is not an experimentalist, and there is naturally a tendency to settle problems by verbal formulations which often are tautological. For example, his explanation of the learning process by calling it a "cumulative disposition" means no more than "learning is learning."

The preface to the first edition refers to the book as an "exposition of Psychology from the genetic point of view." The approach is genetic in the sense that the leading topics are discussed in the order: sensation, perception, ideation.

There are extensive discussions of methodology and of the mind-body problem. The book is very comprehensive, and is perhaps too difficult for use as an introductory text, although the author refers to it as such.

Vanderbilt University LYLE H. LANIER

Freiheit, Wollen, Aktivität. H. REINER. Halle, Max Niemeyer, 1927. Pp. 172.

This work deals with the phenomenological investigations directed toward the problem of free will. The problem of freedom itself is critically discussed, especially in its relation to the problems of human adjustment and its manifold manifestations. The relationship between freedom and volition and resulting activity is given especial attention. This is a contribution toward a systematic psychology.

Ohio State University ALBERT P. WEISS

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THE RÔLE OF CONTEXT IN ASSOCIATIVE FORMATION[1]

By G. L. FREEMAN, Cornell University

The ingenious use of nonsense syllables in the study of associative formations is generally recognized as one of the outstanding advances in the history of experimental psychology. At the same time, many of the investigations employing this material have implied a doctrine of the mechanics of association which meets with little support at the present time. It is unfortunate that the method should fall into disrepute solely because of old presuppositions. It is now generally recognized that the 'meaningless' character of nonsense syllables is only relative and that the assimilation of them in series always ushers in some sort of contextual setting;[2] but this knowledge warrants neither the discarding of the method itself nor the neglect of the results obtained by its use. It is possible that the material may still be of great value in the study and control of the very factor originally assumed to have been ruled out, namely, context.

In this research we have been interested in adapting nonsense syllables to the study of the contextual factors operative in associative formation. Our material has been arranged to suggest a variety of such contexts. Other factors have been kept as constant as possible in an effort to compare the efficacy of the several modes of organization employed. Our observers' commentary

*Accepted for publication June 1, 1929.

[1]From the Psychological Laboratory of Cornell University. The research was directed by Professor Bentley.

[2]The assumption of most experimenters who utilize nonsense material is that the 'syllables' are the 'stimuli' fed into the organism which assume a context through assimilation. The actual stimuli are simply patterns of ocular excitation, not syllables. The apprehension of syllables precedes, or goes along with, their integration into new and larger units.

furnishes new and valuable information upon the psychological functions involved.

Associative formation has suggested a number of promising problems in functional performance. In this study we have mainly considered the four most important of these problems which have greatly influenced both our general plan of experimentation and our specific procedures. These problems are as follows.

(1) *The conditions of associative formation.* It is generally agreed that our knowledge of the way in which items suffer integration must come through a study of their conditions. Ebbinghaus seemed to have suggested the ideal method for this purpose. But in the experimental application of his method, the organic factors involved were seriously neglected. Such factors as repetition, contiguity, and length of series received unnatural emphasis as 'conditions,' while the *secondary operations* or devices used in the formation of associated complexes were ignored and subordinated. The view of association as organization places emphasis upon the neglected organic factors.[3] Contiguity in space and in time is a 'condition' only in so far as it determines organization. Repetition does not result in the mechanical building up of 'bonds' but aids rather in solidifying functional totalities and organized performances.

According to a view of traditional associationism, the organism was regarded as passively receptive to all incoming energies.[4] All that was necessary, therefore, for the association of objects, ideas, and events was their contiguity in time. But this view fails to account for the fact that only certain of the events occurring in the same physical proximity are *selected* for integration. This selection is made by an active organism and is determined by instruction. Contiguous events will serve to initiate the organic functions of integration only when their arrangement serves as an *occasional instruction.*

The alleged importance of repetition dates properly from the early researches of Ebbinghaus and Müller. In reality, frequent repetition was neces-

[3]For a treatment of association as organization see M. Bentley, *The Field of Psychology*, 1927. Cf. also W. Köhler, *Gestalt Psychology*, 1928.

[4]Excellent summaries of the writings of the English School will be found in H. C. Warren, *A History of Association Psychology*, 1920; and G. Murphy, *An Historical Introduction to Modern Psychology*, 1928, 13-30, 106-112.

sary in these studies simply because the choice and arrangement of material tended to limit the organism to new and untried modes of organization. Significant studies which show the ineffectiveness of mere frequency in establishing associative connections have been made by Kühn,[5] Poppelreuter,[6] Smith and McDougall,[7] Peterson,[8] and Kuo.[9] Comprehensive criticisms of the doctrine of mechanical frequency-linkage will be found in Ogden,[10] and Helson.[11]

The remainder of the long list of conditions of association could be interpreted in a manner similar to the above. If we hold strictly to our functional point of view, it follows that all these alleged conditions are modifying circumstances only as they actually effect performance. This effect may take either of two directions; the 'conditions' may favor the use of a particular type of operation or they may hinder the use of any type whatever. A better understanding of the way in which these operations are initiated is necessary for estimating much early work upon the conditions of association.

The instruction to 'learn' a series of nonsense syllables may be apprehended and carried out in various ways. That is to say it undergoes considerable revision and reformulation by the organism and so modifies functional performance. Besides the verbal direction or *formal instruction*, the arrangement of the materials may aid assimilation by serving as an *occasional instruction*. If neither the occasion nor the verbal direction suggests an appropriate performance, the resourceful *O* will manage to create one through *self-instruction*. Adequate control of the organic conditions of associative formation is reached only through specific formal or occasional instruction. Even then the danger of additional self-instruction makes the commentary of a trained observer a necessary check.

(*2*) *Organization and context.* The failure of associationistic principles to account for the conditions of effective assimilation was early evidenced by the need of bolstering hypotheses in the

[5]A. Kühn, Über Einprägung durch Lesen und durch Rezitieren, *Zsch. f. Psychol.*, 68, 1914, 396-482.

[6]W. Poppelreuter, Nachweis der Unzweckmässigkeit die gebräuchlichen Assoziationsexperimente, usw., *Zsch. f. Psychol.*, 61, 1912, 1-25.

[7]M. Smith and W. McDougall, Some experiments on learning and retention, *Brit. J. Psychol.*, 10, 1920, 204-212.

[8]J. Peterson, Frequency and recency factors in maze learning by white rats, *J. Animal Behav.*, 7, 1917, 338-364; Learning when frequency and recency factors are negative, *J. Exper. Psychol.*, 5, 1922, 270-300.

[9]Z. T. Kuo, The nature of unsuccessful acts and their order of elimination in animal learning, *J. Comp. Psychol.*, 2, 1922, 17 ff.

[10]R. M. Ogden, *Psychology and Education*, 1926, 184-193.

[11]H. Helson, The psychology of *Gestalt*, this JOURNAL, 37, 1926, 52-53.

form of memorial aids,[12] the stamping-in effect of pleasant consummation,[13] and the clarifying force of attention.[14] Investigators found that their alleged 'nonsensical' material acquired from apparent necessity a certain amount of context. Instead of trying to account for these contexts by way of external agents, a more empirical procedure would make them simply aspects of the performances employed in organization.

Context need not be assumed to reside in the materials organized. A great deal of organization may be reportable and we may simply experience its *result* as the context of the material. By the use of nonsense syllables we are able to discover how organization is dynamically brought about and maintained.

The importance of organization both to the formation and to the reproduction of associative trains has been demonstrated experimentally. Nagel found that syllables assimilated under one arrangement were apprehended as entirely new when this arrangement was changed.[15] Terms taken from the middle of series frequently failed to reproduce their associates, though the total performance could be completed without error when properly initiated. The more recent study of Pans showed that the removal or interchange of a context present during assimilation had a detrimental effect upon correct reproduction.[16] Both of these researches indicate that associated items are always events *in a context*. Changes of organization mean changes in context, and an item taken out of its context fails to initiate the organized performance of which it is a part.

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[12]No one is better qualified to tell us what factors underlie the organization of a series of nonsense syllables than is G. E. Müller. Nevertheless, throughout his long series of researches he is not entirely clear of the relation of his aids (*Hilfen*) to the things aided. Association is fundamentally a *synthetizer* and new complexes are 'glued' together by the *Hilfen*, which are simply older and preëxisting bonds. Müller's recent reply to his *Gestalt* critics (*Komplextheorie und Gestalttheorie*, 1923) has, however, brought his experimental work into a broader perspective. In this treatment the *Hilfen* frequently seem to operate upon the material and organize it in a strictly functional manner.

[13]Summaries of the recent work upon affectively toned associations will be found in E. S. Robinson, Memory, *Psychol. Bull.*, 21, 1924, 581-584, and J. A. McGeoch, Memory, *ibid.*, 25, 1928, 538-539. It is probable that the emotive predicament frequently serves an as occasional instruction and so initiates integrative functions. But such an aid would be antecedent to—hence, not to be confused with—the processes of organization.

[14]'Attention' with the intent to learn seems to be necessary to all assimilation. Only when it is assigned the rôle of an *integrator* shall we have to inquire what are the actual secondary operations employed under attention.

[15]F. Nagel, Experimentelle Untersuchungen über Grundfragen der Assoziationslehre, *Arch. f. d. ges. Psychol.*, 23, 1912, 156-253.

[16]S. Pans, The influence of context on learning and recall, *J. Exper. Psychol.*, 9, 1926, 37-64.

(3) *The effectiveness of various modes of organization.* The problem of effective associative formation is intimately connected with the type of secondary operation employed. The practical bearing of this problem is obvious. It has notably popularized the study of memory and association. Nevertheless the reader of psychological works will fail to find much usable knowledge upon the real psychological basis of 'economy' and 'efficacy.' The only feasible means of study is to compare the outcome of various modes of occasional and self instruction. The most serious defect in research of this type has been the unwarranted assumption that simply setting the stage for a certain procedure is sufficient to guarantee its use by the 'associating' organism. In fact, the tendency of early investigators to think solely in terms of such environmental setting resulted in making economy dependent upon the *material* used rather than upon the *operation* employed.

Ebbinghaus[17] differentiated between meaningful and meaningless sequences, while Pohlman[18] listed the efficacy of various materials in the following order words, consonants, numbers, and nonsense syllables. More recent experimentation has attacked the problem from the operational standpoint. Two typical researches are those of Klemm and Ollson[19] and Guilford.[20] But even this work affords little insight into the actual modes of organization. Confusion arises particularly between adherents of rival theories of learning.

In order to avoid the controversy which results from differentiating these modes on the basis of their superiority, we have used the following classification. Certain arrangements suggest their own organization; other organizations result, more remotely, through self-instruction and from various functional resources of the organism. The contexts which these organizations assume are therefore either *intrinsic* or *extrinsic* to the arrangement of the material. This classification orders the modes of organization according to their origin. While nothing is here said of relative efficacy, we might expect to find arrangements suggesting intrinsic aids to be generally superior.

(4) *The residues of associative formations.* One of the characteristic features of associative formations is their reformation

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[17]H. Ebbinghaus, *Memory* (Ruger and Bussenius, tr.), 1913.

[18]A. Pohlman, *Experimentelle Beiträge zur Lehre vom Gedächtnis*, 1906.

[19]O. Klemm and E. Ollson, Über den Einfluss mechanischer und sinnvoller Hilfen bei Gedächtnisleitung, *Zsch. f. päd. Psychol.*, 26, 1923, 78-95.

[20]J. P. Guilford, The rôle of form in learning, *J. Exper. Psychol.*, 10, 1927, 415-423.

by way of memory. The career and outcome of these functional residues have been the subject of much concern and experimentation. The modern associationists explain 'reproduction' upon the basis of the 'bond' hypothesis, according to which an absolute link between two or more discrete items is the determining factor.

Ach,[21] for example, believed that he could measure the strength of this 'bond' by the following method of *associative equivalents*. After *O* has associated two nonsense syllables a certain number of times, he is instructed to rhyme or invert when one appears. *O* may either carry out the given instruction or may reproduce the syllable formerly associated with the stimulating object. A reaction in which the association persists in the face of contrary instruction is called an *intended failure reaction*. The number of repetitions necessary to bring about such a reaction is the *equivalent* or measure of the strength of the 'bond.' Even when an intended failure reaction does not result (as by insufficient repetition), contrary instruction will set up a *heterogeneous activity* and so lengthen the reaction-time. (*Homogeneous activity* is aroused only when the instruction and associative disposition operate in the same direction.)

Ach's reasoning is typically associationistic in that it says nothing of the properties of the trace in question. In order to be operative this trace or bond would have to be called out by the excitant for reproduction. It is difficult to see why this need ever have occurred under the conditions of Ach's test, since the 'contrary' instruction serves to initiate functional traces quite foreign to those formerly connected with the presented syllables. The traces which the method is supposed to measure would manifest themselves only through reformation of this instruction. In Lewin's[22] noteworthy repetition of Ach's method it was shown that reproduction is not a matter of strong associative 'bonds' but rather is dependent upon favoring conditions in the way of formal instruction and occasional setting.

Recently it has been shown that besides the residue of absolute linkages, there exist traces of total organizations.[23] These traces, the absolute and the structural, have different careers. At first an associative organization will be specific. But with time the performances involved will become divorced from the original material. These performances, acquired in connection with definite integrations, are retained as part of the functional equip-

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[21] N. Ach, *Über den Willensakt und das Temperament*, 1919, 18-24.

[22] K. Lewin, Das Problem der Willensmessung und das Grundgesetz der Assoziation, *Psychol. Forsch.*, 2, 1922, 65-141, 191-302. A fairly complete summary of these two papers will be found in H. Helson, *loc. cit.*

[23] W. Köhler, Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn, *Abh. d. preuss. Akad. d. Wissenschaft, phys.-math. Klasse*, Nr. 2, 1918.

ment of the organism and are used in meeting other situations. The excitant for these traces need only be conditions *similar* to those initiating the original performance. Of course the 'response' may not be 'right' as measured by the absolute reaction. If, for example, the presented item is apprehended simply as a formerly rhymed syllable, we cannot expect this tract to reproduce of necessity the exact rhyme. A study of many of the experiments on reproduction and recall will reveal instances of 'right' contextual relation between the so-called 'wrong' response and its stimulus. We are beginning to see that the effort to trace the career of absolute 'bonds' has led to the neglect of the more important residues of functional organization. The adequate description of these latter factors is a problem for the future.

The Experiments

I. General Plan

Our general plan was to utilize but one type of material, the nonsense syllable, and in various experiments to vary the context in which this material appeared. Such contexts were never assigned by formal instruction but were set rather as advantageous or favoring conditions. The *O*'s commentary showed whether or not these conditions actually aided assimilation by suggesting appropriate secondary operations. Aside from the systematic variations, the conditions of impression were kept as constant as possible throughout the experiments. The material used was repeated until completely mastered. Retention of the associative complexes was tested by approximate means both immediately after assimilation and also after an interval of one month from the time of the last experiment. Conditions common to all experiments are given below; the systematic variations are discussed in connection with the procedures of the individual series.

(*A*) *Conditions of impression.* (*1*) *Materials used.* Three-letter syllables were used in all experiments. They were either *conventional* or *unconventional.* The conventional syllables were formed in accordance with the rules of Müller and Schumann[24] (diphthongs were not used). The great number of this type of syllable which already have symbolic significance (in English especially) suggested that such combinations would be inherently easier than a type with which the *O* was less familiar. To test this supposition, unconventional syllables (formed by placing the vowel at the beginning or end of a syllable, *e.g.* omk)

[24] G. E. Müller and F. Schumann, Experimentelle Beiträge zur Untersuchung des Gedächtnisses, *Zsch. f. Psychol.*, 6, 1894, 95-106.

were used with those of the conventional type. No syllable was used twice in the experiment.

(*2*) *Arrangement of material in series.* Meumann has maintained that economy in learning and in memorial efficacy is a function of the method employed.[25] This assertion seems especially apt when one considers that certain of the 'memory methods' suggest various modes of ligating the material presented. In the *Ersparnismethode* of Ebbinghaus, a series of syllables are connected end-to-end (longitudinal ligation), while the *Treffermethode*, as employed in the Müllerian researches, emphasizes paired ligation. Operations effective in connecting a number of items in series may be quite ineffective for joining two of these items into a complex. Our experiments emphasized both paired and serial ligation. Series of paired associates were used throughout the research, and *O* was required completely to master these special orders. In general, the material for a given experiment was divided equally between series of conventional pairs (wof-sul), series of unconventional pairs (url-knu), and series which included both conventional and unconventional pairs.

(*3*) *Number and length of series.* Six series of 15 paired-associates were used in all experiments.

(*4*) *Order of presentation.* Variations due to differences in position were controlled by having the *Os* repeat the series in various time orders. Six *Os* made it possible to arrange a given series in all positions.

(*5*) *Method and rate of presentation.* The syllable-pairs were shown successively and read aloud by the *O* as they appeared. They were made by pasting Willson's gummed letters (black, no. 5) on buff cardboards arranged serially about a large revolving drum. *O* sat in a booth in the experimental room. The syllable-pairs appeared (indirectly lighted) with the opening of the exposure-window immediately before him.

The apparatus employed was a large drum carrying the six series of syllables, and arranged on a skid or track so that any series could be placed in front of the exposure-window as desired. The shutter of the exposure-window formed an integral part of this apparatus, opening at each turn of the drum. The drum was rotated at a constant rate, with brief pauses between the presentation of successive series.

(*6*) *Number of repetitions.* The number of repetitions varied from experiment to experiment. With one exception (Experiment III), all series in a given experiment were repeated the same number of times. The following procedure was rigidly followed throughout. Each series was repeated ten times in succession on each working day. If a series were acquired early, it was repeated with the others until all could be reported verbatim. In the first three experiments all the series were repeated a given number of times, regardless of the fact that most *Os* had completely mastered them before this requirement was fulfilled.

(*7*) *Observers.* Six *Os* served constantly in the experiments.[26] They were all graduate students in psychology. Three other *Os* took part in Experiment

[25]E. Meumann, *The Psychology of Learning* (J. W. Baird, tr.), 1913, (appendix ii), 368-370.

[26]Misses Davidson (*Da*), Drury (*Dr*), Bulbrook (*B*), and Messrs. Glanville (*G*), Kemp (*K*), and James (*J*). The writer wishes to express his appreciation for the coöperation of these individuals in a task which was, to say the least, very exacting and time-consuming.

III_2. One of these was a graduate student in psychology; the others were 'naïve' undergraduates.

(8) *Stability of O's general attitude.* It is a well-known fact that intent to learn and degree of effort expended are important factors in the economy of learning and retention. We attempted to control these factors by stressing in our formal instruction maximal alertness and effort.

(9) *Diurnal variation.* There was necessarily some variation in the time at which the series were presented on successive days as well as in the interval elapsing between presentations. Since the *Os* worked at different hours, this factor may be considered as introducing a variable error.

(10) *Practice-effects.* The possible influence of practice upon economy was checked by comparing the *O*'s rate of assimilation in the original experiments with that obtaining for identical arrangements after an interval of approximately four months. This interval was filled with 'practice.'

(11) *Instructions.* The formal instructions were similar for the various experiments:

"After a ready signal, you will be shown a meaningless letter-combination, then another and another. Read each combination aloud as it appears. Assimilate the series as simply and as rapidly as you can. At the end of the hour you will be asked to write down such combinations as you can recall, as well as to comment upon the various stages of assimilation."

(12) *The method of commentary.* The method of commentary involved the closest coöperation between observer and experimenter.[27] *E* listened carefully to *O*'s reading of the syllables to detect cues to operations being used to organize them. If the use of such an aid was not reported, *E* would frequently seek confirmation for his opinion by the judicious use of questions. Besides this, *E* noted any variation in the general attitude or conduct of the *O* which was not reported and which might influence the economy of assimilation.

(*B*) *Tests of memorial efficacy.* The efficacy of the associations formed in the several experiments was tested at two different periods; immediately following complete mastery, and after an interval of one month from the date of the last experiment. Tests for the first period were concerned particularly with the retention of the paired-associates; those of the latter group included a test of the serial ligations in all experiments.

(1) *The test of contrary instruction.* Our use of this test was confined to the first two experiments. It is essentially the same as that used by Lewin. Syllables equal to the number of first terms of all the pairs in a single experiment (90) were shown to the *O* in haphazard order until he was familiar with them.[28] Later these neutral syllables were mixed with the 90 first terms in a given experiment and the whole number (180) presented with the following instruction:

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[27] Cf. M. Bentley, *op. cit.*, 197-199.

[28] The 90 cards bearing these 'neutral' syllables were shown to each *O* once on each of five consecutive days. Their order differed with each presentation. At the end of the fifth presentation, these syllables were mixed with an equal number of syllables which *O* had never seen. *O* was asked to respond to these by saying "plus" if the syllable was familiar and "minus" if it was not; this afforded a measure of the degree of familiarity before the first experiment. This test showed over 75% correct recognitions for all *Os*.

"You will be shown meaningless letter-combinations. When a combination appears say aloud its inverted form, *e.g.* git–tig or elb–ble. At the end of the hour report on the period preceding the responses."

An exposure-apparatus made to carry a single card at one time replaced the revolving drum used during the assimilation of the series. Each card contained a single syllable formed of gummed letters. In their general appearance to the *O*, these cards were identical with those seen during assimilation.

Since it was desired to test for the presence of any 'heterogeneous activity,' such as was alleged by Ach,[29] the following system was used to record the reaction-time to the presented syllables. *O* pressed a key at every 'ready' signal and the raising of the exposure-shutter caused a copper pin to drop into a mercury cup and so to close the circuit. The moment *O* began to speak the response word, he released the key, thereby opening the circuit again. The actual reaction-times were recorded by means of a Renshaw polygraph. The reaction-record was run coördinately with a time marker obtained by connecting one of the magnets of the polygraph with a Kronecker interrupter. This system permitted the estimation of reaction-time in units of 0.1 sec. It was sufficiently accurate for our purposes. *O* made 100 practice-reactions before performing the regular experiments.

(*2*) *The test of free association.* The 90 first terms of the paired associates were mixed with the same 90 neutral syllables as was used in the test described above. The entire number was then shown to the *O* with the following instruction:

"When a combination appears, say the first thing suggested. Do not pronounce the combination itself. At the end of the hour comment especially upon the period preceding the response."

(*3*) *The use of Treffermethode after an interval of time.* One month after the conclusion of the last experiment, all *O*s were asked to report again for a series of tests. In the first of these tests, five of the first terms were taken from every series learned during the entire research. They were arranged in haphazard order and then shown to *O* under the following instruction:

"You will be shown some meaningless letter-combinations, the associates of which you are to recall. Consider each combination in its own right."

(*4*) *The test of free association after an interval of time.* For this test five of the first terms of every old series (different from those chosen for the above test) were united with an equal number of neutral syllables in a haphazard arrangement and shown to *O*. The instructions were the same as those given in the free association-test previously described.

(*5*) *The test of retention of serial ligations.* This test follows the procedure of the familiar *method of prompting*. The instructions were:

"You are to try to recall every series which you acquired in this experiment. The members of these series must be given in their exact order. The experimenter will show you the beginning of each series in turn and will prompt you when necessary."

II. Experimental Procedures

Our research is properly divided into four experiments, every one a unit in itself. The general feature differentiating these ex-

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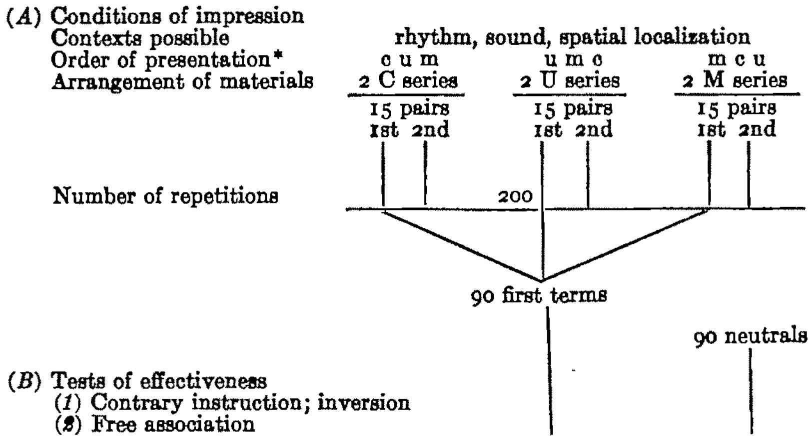
[29] N. Ach, *op. cit.* 33 ff.

periments is the type of context permitted or suggested by the arrangement and setting of the materials. In the first experiment the organism was limited to such extrinsic aids as rhythm and spatial localization. The syllables were carefully chosen and arranged so that few if any intrinsic relations (such as identical elements, rhyme, and the like) would be suggested. The second experiment utilized two representative aids such as were excluded from the first experiment. The third permitted the use of a formal context, which, when once reduced to rule, was designed to recall the second item on the sight of the first. In the fourth experiment the syllables were so chosen and arranged as to suggest the symbolic relations usually carried by a sequence of words. A detailed account of these four experiments is given in connection with the paradigm of the procedure employed in each.

Experiment I. The paradigm of the procedure of Experiment I is as follows:

Experiment I
Characteristic Extrinsic Contexts



*The small letters c, u, and m represent the temporal order of the three types of series: conventional (c), unconventional (u), and mixed (m).

Earlier research has shown that certain rhythms are more economical than others.[10] While no particular rhythm was assigned by instruction, the arrangement of the materials in pairs served to suggest the *iambic*. The six series were repeated 200 times each, after which tests under (*B*) were given. Before the beginning of this experiment, each *O* was given 100 conventional syllables to determine his ability for treating nonsense material as devoid of symbolic significance. The *Os* were given the negative instruction "not to regard the

[10]Cf. G. E. Müller, Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, *Zsch. f. Psychol.*, *Erg.-Bd.*, 5, 1911, 348-301.

combinations as having verbal meaning" and were asked to respond with "the first sound that forms itself."

The six regular *Os* (*Da*, *Dr*, *G*, *B*, *K*, and *J*) served in the experiment. *Da* and *K* repeated the experiment (but with different syllables) after an interval of approximately four months. The purpose of this repetition was to test for the presence of greater economy due to *practice* in learning nonsense syllables.

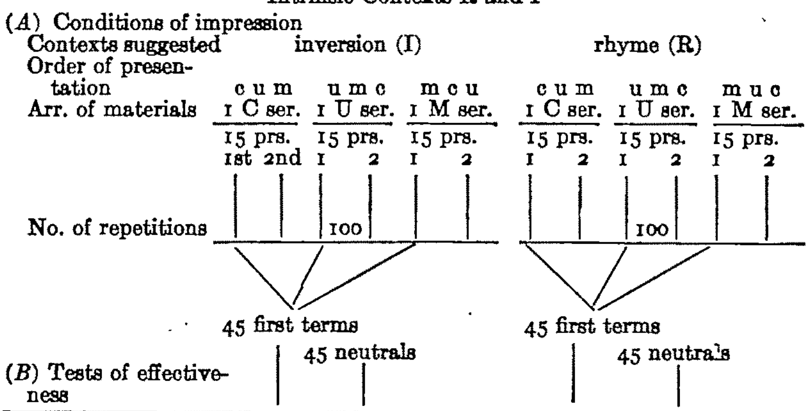
Experiment I_m: (*Melodic contexts*). In Experiment I we found a tendency of one *O* to use different voice-pitches as an aid in grouping the syllables. This performance has been reported previously by Frings,[21] who was able to represent graphically the 'melodies' used by his *Os* as extrinsic aids to organization. In a subsidiary experiment we sought to study directly the economy of such a procedure. Six series of 15-syllable pairs were so arranged as to suggest few internal aids in assimilation. The material was divided into two equal portions. The three series in the first part were shown in a manner identical to that used in Experiment I. The three series in the second part were shown with a melodic accompaniment.

The melody differed for each of these three series. It was played by the experimenter upon a Schoenhut toy piano. One tone was sounded at the presentation of each syllable-pair. Every melody was divided into three phrases of five notes each. The melodies used were original, since it was not desired that *O* should formulate a rule, *e.g.* "that is the 'Home, Sweet Home' series." The tunes played were (1) c-e-g-f-d, e-g-b′-a′-f, e-f-e-d-c; (2) c′-b′-a′-c′-f, a′-g-f-a-d, e-f-g-e-c; and (3) c-d-e-d-e-, d-e-f-e-f, g-f-e-g-c.

The six regular *Os* served in this experiment. It should be noted that they were under no formal instruction that forced even the notice of the melodic sequences. The three series given without accompaniment served as controls. No tests of memorial efficacy were made in this subsidiary experiment.

Experiment II. The paradigm of Experiment II is as follows:

Experiment II
Intrinsic Contexts R and I



[21] G. Frings, Über den Einfluss der Komplexbildung auf die effektuelle und generative Hemmung, *Arch. f. d. ges. Psychol.*, 1914, 30, 426-431.

(A) Contrary instruction; (with reaction time)	rhyme	invert
(B) Free association		
(C) Advantageous condition *vs.* contrary instruction; (with reaction-time)		
Part i	invert	rhyme
Part ii	rhyme	invert

Experiment II. Test C is peculiar to Experiment II and was introduced as an additional check upon the factors actually operative in the reproduction of associative complexes. In Part i of this test, the first members of all series were shown in order as learned (a strip of paper covered up the second syllable as it appeared at the exposure window). *O* was instructed to rhyme rhymes and to invert syllables which had always been previously inverted. In Part ii the same conditions (advantageous to the usual performance) were present; but a contrary instruction was given, *e.g.* rhymes were to be inverted and inversions rhymed.

The six regular *Os* served in the experiment (except that *Da* omitted Test C). An added *O*, *A*, followed the same time-order as *Da* and took the Test. *B* and *G* repeated the experiment (but with new syllables) after an interval of about four months.

Experiment III. The paradigm of Experiment III is as follows:

Experiment III

Formal Extrinsic Contexts A and B

(*A*) Conditions of impression						
Contexts suggested		*Formal A*			*Formal B*	
Order of presentation	c u m	u m c	m c u	c u m	u m c	m c u
Arr. of materials	1 C ser	1 U ser	1 M ser	1 C ser	1 U ser	1 M ser
	15 prs.	15 prs.	15 prs.	15 prs.	15 prs.	15 prs.
	1 2	1 2	1 2	1 2	1 2	1 2
No. of repetitions	100+			100+		

	90 first terms	90 neutrals
(*B*) Tests of effectiveness		
(2) Free association		
Group a	30 wrongly scored	30 scored
Group b	30 rightly scored	30 unscored
Group c	30 unscored	30 unscored

Experiment III. Formal context A. In one series of conventional pairs, one series of unconventional pairs, and one mixed series, a single letter of the first term was set *ahead* one letter in the alphabet to form the second term. The letter of the first term that was shifted was underscored, *e.g.* wof–wog. In the first five pairs of each of these series the first letter was shifted; in the second five pairs the second letter was shifted; and in the third and last group of five the last letter was shifted. Vowels were shifted to the next vowel of the customary order.

Formal context B. In one series each of c, u, and m pairs, a single letter of the first syllable was shifted *backward* one letter in the alphabet to form the second term (vowels were shifted back to the next vowel). The letter shifted was overscored, *e.g.* s̄ul–rul. In the first five pairs of each of these series the last letter was shifted backward; in the second group of five the second letter was shifted; and in the third group the first letter was shifted. The six regular *Os* served in this part of the experiment.

Experiment III_s (Subsidiary experiments): Formal contexts. The rule operative in Experiment III did not greatly aid the 'serial ligation' of the materials. In this subsidiary experiment, therefore, a formal relation was suggested which, if apprehended, would be effective in connecting the syllables both in pairs and in series. Six series of 15 unconventional pairs each were repeated until completely mastered. No tests of effectiveness were made. Occasional *Os A, F*, and *R*, served in the experiment. The contexts suggested were planned to be somewhat analogous to the arithmetic progression of number series and were as follows.

Formal context R (2 series). The series began with a certain syllable whose third letter was colored *red*. The red letter was shifted one (consonantal or vowel) place forward in the alphabet to form the next syllable. The third letter of this second syllable was also colored red and a shift of this letter one place forward formed the next term. This progression of letters was followed throughout the entire series of 15 pairs, *e.g.* olb–olc, old–olf, etc.

Formal context G (2 series). The general plan of the two series arranged to suggest context G was much the same as for context R. The differentiating feature was that a *green* letter was shifted two letters forward in the alphabet to form the next syllable, *e.g.* umb–umd, umf–umh, umj–uml, etc.

Formal context RG (2 series). The two series falling under this rubric were formed by beginning with a given syllable and shifting its third letter (colored red) progressively one place forward in the alphabet and its second letter (colored green) two letters forward *e.g.* ibf–idg, ifh–ihi, ijj–ilk, inl–ipm, etc.

Experiment IV. The paradigm of Experiment IV is as follows:

Experiment IV
Symbolic Contexts

(*A*) Conditions of impression

	symbolic relations		
Contexts suggested	nursery-rhymes, hymns, prose		
Order of presentation	nr, h, p	h, p, nr	p, nr, h
Arrangement of materials	2 M series	2 M series	2 M series

	15 pairs	15 pairs	15 pairs
Number of repetitions		until mastered	
		90 first terms	
(*B*) Tests of effectiveness			90 neutrals
(1) Free association			

In this experiment the 'nonsense' material was so arranged that the *Os* might utilize the symbolic relations usually suggested by the meaningful sequences of (1) nursery rhymes, (2) hymns, and (3) prose. Both conventional and unconventional pairs were used in all series. They were chosen and arranged after the following procedure. A given hymn or nursery rhyme was chosen and its words changed to three-letter nonsense syllables which gave somewhat the same sound as the word when pronounced. These syllables were fitted into series of 15 pairs. The rule determining each formation was that so far as possible no given syllable-pair should itself suggest a symbolic relation between its two members, as it was desired that the symbolic context should refer to the series as a whole. The hymns mutilated after this manner were 'jus–ast, hiy–amb' and 'unt–ter, hif–wim'; the nursery rhymes were 'hik–kri, dik–kri' (dock) and 'tre–lit' (kittens, etc.); the prose included a series of rules on personal appearance (wah–ure, fas–mit, lux–sop) and a 'Milt Gross' version of a telephonic conversation (hel–lov, zat–uhh). The six regular *Os* sat in the experiment.

Results

The results for a given experiment will appear in two parts. Part i will be concerned with the assimilation of the syllabic complexes and will include the rate of assimilation and a brief abstract of the operations of each of the several *Os*. Part ii will be concerned with the tests of memorial efficacy and will include the analysis of the responses made in these tests. A summary of the results for a given experiment will follow its more detailed treatment.

Experiment I. Extrinsic Contexts

Part i. Economy in assimilation. For a short description of the conditions of this experiment the reader is referred to its paradigm on page 183. The individual protocols, which are abstracted below, show the method and rate of assimilation for each of the six *Os*.

Da. Rate of assimilation (R.A.) 55.3 syllable-pairs a day. General attitude (G.A.) excellent. *Da* had considerable experience with nonsense syllables before this time. She maintained an almost constant interest and effort throughout the experiment. The test of her neutrality toward nonsense syllables

showed only 6 *meaningful completions* (m–cs.) out of 100.[33] *Da* went about the task of assimilating the syllables in a systematic manner. The first day was marked by an active search for "any cues which might be utilized" in assimilation. During the second working day "less time was taken for purposes of orientation than before." Her failure to find other cues caused her to place "great reliance on auditory-verbal, and in some cases, visual imagery." Certain series were noted as "more difficult to pronounce and liable to be hard to assimilate." "A tentative *grouping* of the syllable pairs was tried out." On the third day, each series was divided into three groups of five each. At subsequent presentations, *Da* followed this procedure strictly, perfecting the operation as time went on. The groups of five were counted on the fingers and certain 'pillars' (such as the beginning on the middle pair of a group) were noted and "an attempt made to clinch them." Spatial projection of the groups upon a piece of paper also appeared. In writing down the combinations, she noted that "few syllables come which are not localized." As assimilation progressed, such 'mental jottings' as "middle of second group of five" dropped out and one pair seemed to follow the other as "a matter of course."

Dr. R.A. 29.7: G.A. constant. *Dr* had little previous observational practice. She gave 23 m-cs. in the face of negative instruction. She first observed that the more 'euphonious' (conventional) series would be easier to assimilate. After a period of initial confusion and search (repetitions 1-50) she decided to *group* the syllable-pairs into larger complexes of 4, 4, 4 and 3. She "concentrated" first upon the initial pair of each group and later upon entire groups which gave unusual difficulty. The performance was carried to complete mastery.

G. R.A. 32: G.A. constant. *G* had considerable observational experience, though none in this type of work. He gave 33 m-cs. in the face of the negative instruction. The indication from his protocols is that *G* never had a well-formulated plan of procedure. At first he made an attempt to utilize extraneous associations. Later there came the realization that unless the pairs were learned in order there would be no guarantee that every one would eventually be mastered. Beginning with the 100th repetition, various groupings were tried on different series at different times; the one most frequently employed was 4, 4, 4, 3. He reported that the unconventional series were more difficult.

B. R.A. 23.7: G.A. characterized by a lack of confidence in her ability to master the task set. She had no previous observational practice and had to be frequently urged to "do her best." She gave only 3 m-cs. under negative instruction. For about the first 70 repetitions she "simply said them over." After that she "hit upon" dividing each series into groups of 4, 4, 4, and 3. This procedure, while not followed invariably, aided in the ligation of the syllabic complexes.

K. R.A. 50.6: G.A. constant. Without previous observational experience, *K* regarded the experiment as an elimination contest and was determined to make a good showing. He gave 41 m-cs. under negative instruction. *K* employed a systematic procedure from the second repetition to the stage of com-

[33]The term 'meaningful completion' is used in this research to designate the procedure of adding one or more letters to a nonsense syllable in order to make it into a familar word, *e.g.* rup, Rupert.

plete mastery. Recognizing immediately the necessity of devising some scheme to aid him in the assigned task, he imposed a system of rhythmical grouping upon the material presented. Each series was divided into 5 groups of three pairs each. During early repetitions, *K* gave particular attention to the pairs beginning each group. On successive days he would 'concentrate particularly' on certain series, beginning with those which he considered the easiest.

J. R.A. 33.6: G.A. variable. *J* had considerable observational practice, but his work in this experiment might be termed sporadic. He gave only 3 m-cs. in the face of negative instruction. For about the first 60 repetitions, *J* "repeated the syllables mechanically." Then he tried grouping the pairs by twos. When this did not result in immediate improvement, he decided to develop a "different scheme." The one decided upon was an "alphabet scheme" which involved "remembering the initial letters of a series in order and letting the rest drop in if they would." The experimenter heard him group certain series in the 4, 4, 4, 3 rhythm during the period of the 8, 10, 14 and 15th observations.

The rates of assimilation given above may have resulted in part from the lack of practice of certain *O*s. But that this explanation is not sufficient is shown by the following check-experiment. The two *O*s whose rate of assimilation was the most rapid repeated the experiment after an interval of four months. This period was filled with practice in learning syllables. But the rates of assimilation show no significant increase. *Da*'s original rate was 55.3. Her rate after practice was 56.4. *K*'s original rate was 50.6 and his rate after practice was 46.9.

Most *O*s reported that the series of conventional pairs were 'easier' or 'more euphonious' than unconventional pairs. This fact is shown quantitatively in Table I.

TABLE I

NUMBER OF NECESSARY REPETITIONS FOR THE CORRECT RECITAL OF THE THREE TYPES OF NONSENSE SYLLABLE

	Time Order	Conventional		Unconventional		Mixed	
		Ser. 1	Ser. 2	Ser. 1	Ser. 2	Ser. 1	Ser. 2
J	c^1	120	130	140	160	140	150
K	c^2	50	90	110	100	70	100
G	u^1	140	150	140	150	140	150
B	u^2	120	140	170	170	150	150
Da	m^1	40	70	100	110	90	110
Dr	m^2	130	140	150	160	140	150
Average		100	120	135	144.6	121.6	135
		110		139.8		128.3	

Part ii. Tests of effectiveness. Under contrary instruction, no *O* showed a tendency to reproduce an associate at the sight of its first term when he was set to perform in a different manner. Table II (which compares the reaction-time of neutral syllables with those which have associates) indicates that the 'hetero-

geneous activity' (discussed by Ach as producing a lengthening in reaction-time) is as conspicuously absent in our experiments as in those of Lewin.

TABLE II

COMPARISON OF AVERAGE REACTION-TIMES OF RESPONSES MADE TO ASSOCIATED AND NEUTRAL SYLLABLES

	Reaction-time (in sec.) to			
	associated syllables		neutral syllables	
	ave.	m.v.	ave.	m.v.
J	0.82	1.5	0.81	1.5
K	0.83	1.3	0.73	1.2
G	0.86	1.5	0.81	2.5
B	0.90	0.9	0.93	1.7
Da	1.06	1.5	1.05	0.9
Dr	0.75	1.2	0.77	1.5
Average	0.87	1.3	0.85	1.6

Three general procedures were employed in the *free association* test: (1) To remain relatively passive and to let the presented syllable suggest a particular operation (employed by *J* and *Da*); (2) a self-instruction to place the syllables and to reproduce their correct associate (employed by *G* and *K*); (3) a self-instruction to operate upon all the syllables in an extraneous manner (meaningful completion was employed by *Dr* constantly and by *B* part of the time. The responses made by the *O*s are classified below.

		Response		Due to extran's
		Correct	Incorrect	oper'ns
J	(a)*	38	25	27
	(n)†		42	48
K	(a)	50	12	36
	(n)		10	80
G	(a)	60	20	10
	(n)		22	68
B	(a)	22		68
	(n)		3	87
Da	(a)	58	10	22
	(n)		7	83
Dr	(a)			90
	(n)			90
Average	(a)	38	11.1	42.1
	(n)		14	76

*(a) = Previously associated syllable
†(n) = Neutral syllable

It is obvious that the results of *Dr* and *B* will be of little aid in our estimation of the memorial efficacy of the associations formed in Experiment I. The procedure of *G* and *K* is analogous to the attitude assumed in the 'Treffer' tests and provides an excellent measure of efficacy. The prevalence of 'in-

correct associates' in the protocols of J, K, G, and Da is indicatory of the effectiveness of the contexts used in this experiment. Of the syllables learned which were 'incorrectly' given in response to presented terms, most had a sound similar to the 'correct' associate. This explanation accounts for all but 27 of the entire aggregate of incorrect responses to previously associated syllables (a). Only seven of this remaining number may be credited to remote association. These results indicate that there was a fairly strong tendency on the part of most Os to respond after the acquired manner; but the correct responses never total more than two-thirds of the number actually acquired. Less than one-half, on the average, were correctly reported. Possibly the serial ligations were more significant to O than the paired-connections here tested.

Experiment I_m *(Melodic contexts).* In this subsidiary experiment, we suggested several melodies which might or might not be utilized as a means of ligation. A comparison of the rate of assimilation for the 'melodic' series with the control series and recourse to O's reports has enabled us to list three different outcomes from the same experimental setting, as follows.

(*1*) *Melodic means of ligation utilized.* The accompaniments were of service to G and Da. Their average rates of assimilation for the melodic series were 33.6 and 33.3 respectively. Their respective rates for the normal or control series were 20 and 21. G was not entirely sure what his procedure was during assimilation. He divided the series into groups of five according to the suggestion carried by the melodic phrasing. He frequently 'sang' the syllables (roughly approximating the high and low pitches of the melodies). The series with melodic accompaniment were acquired in 40 less repetitions than were the control series. Da grouped the series in fives. Although she had always done this, she reported that the melody seemed to make the task easier. There is very little evidence from her reports that any other use was made of this context. The series with melodic accompaniment were acquired in 20 less repetitions than those used as controls.

(*2*) *Melodic means of ligation ignored.* The accompaniment was ignored by Dr, K, and J. Their average rates of assimilation respectively were 25.9, 21.6, and 26.7 for melodic series and 26.6, 21.5, and 25.4 for control series. All these Os reported that they were puzzled the first few times they heard the melodies. All were inclined to take their introduction as a jest. The actual reason why the melodies were not utilized may be the fact that these Os were quite satisfied to use their former and different procedures.

(*3*) *Melodic means of ligation serving to distract.* B reported that the melodies distracted her attention from the series which they accompanied. It took her 40 extra repetitions completely to acquire these series while her average rates of assimilation for melodic and normal series were 27.1 and 19.7 respectively.

Summary of Experiment I. The elimination of conditions advantageous to the use of intrinsic aids retarded the assimilation

of the material presented in this experiment. As an aid to organization the *Os* supplied various systems of rhythmical grouping. Those who followed this procedure most systematically learned more rapidly than did the others. Series of conventional syllables were assimilated with less difficulty than were those containing unconventional members. This indicates the importance of sound pattern and voci-motor imagery in the acquisition of syllabic complexes under the conditions of this experiment. The same factor is again affirmed by the analysis of the 'incorrect' associates given in the tests for effectiveness. When utilized the melodic aid suggested in the subsidiary experiment proved effective. The test of contrary instruction gave no indication of "heterogeneous activity." Our results support those of Lewin in this respect and cast considerable doubt upon the possibility of automatic reproduction of a second term on the sight of the first.

Experiment II. Intrinsic Contexts

Part i. Economy in assimilation. The conditions of this experiment are given in its paradigm on page 184f. All *Os* had mastered all series at the end of 100 repetitions. The rate and method of assimilation employed by each *O* appears below.

Da. R.A.68.4: G.A. as in Experiment I. She immediately used the intrinsic contexts of rhyme and inversion as aids in assimilation. *Da* reported that the inversions seemed easier. The procedure of dividing a series into three groups of five each was followed, as in Experiment I.

A. R.A.48.9: G.A. excellent. Introduced as an extra *O* in this experiment. *A* reported that he first tried "to get the feel of" the rhymes and inversions; but later he reported a counter-tendency to try to build up "runs and groupings" which would serially integrate the syllables. The connections between pairs were easy; but the process of learning these pairs in order was found to acquire more time.

Dr. R.A.54.3: G.A. moderately constant. The first day was utilized in becoming accustomed to the new series. It was noted that there were three sets of rhymes and three sets of inversions. Upon the second day, *Dr* decided that it would be necessary to employ grouping in addition to the contexts which the occasion supplied. She used the 4,4,4,3 rhythm (as in Experiment I).

G. R.A.55.9: G.A. as in Experiment I. The occasion seemed to have suggested to *G* that he assimilate by individual pairs rather than by series. It was not until the 40th repetition that he attempted to place these pairs in order. As an aid to this task, he fell back upon the 4,4,4,3 plan of grouping. The inversions were reported as giving difficulty, as were series containing unconventional pairs. His rate of assimilating similar formations after four months was 57.8.

B. R.A.48: G.A. much better than in Experiment I. *B* performed with the conviction that these series "are not as difficult as the former ones." She was not very sure of her procedures during assimilation. She began to localize the syllable-pairs in series at about the 30th repetition. Her R.A. for similar formations after four months of 'practice' was 58.1.

J. R.A.47.9: G.A. as in Experiment I. The first hour was taken to "become familiar with the saying of the syllables." It was noted that some series were rhymes and that others were inverted. Visual imagery was said to play a more important part in the assimilation of inversions than in the case of rhymes. No attempt was made to localize syllable-pairs in series until after the 40th repetition. In order to acquire the exact serial order, *J* divided each series into groups of 4,4,4, and 3 and placed each individual pair in this schema.

K. R.A.62.7: G.A. as in Experiment I. He employed the same plan as in the first experiment in order to acquire the serial order of the pairs. He noted that these individual pairs "went together" better than those of the last experiment. His reports indicate that the suggested contexts were utilized throughout assimilation.

The importance of sound-pattern in the assimilation of the syllables is again evidenced by the greater number of repetitions necessary for the correct recital of unconventional pairs in Experiment II. The average number of necessary repetitions for the conventional pairs was only 56.6, as compared with an average of 88.3 for the unconventional rhymes. The average number of necessary repetitions for conventional inversions was 71.6; for unconventional inverted pairs, 98.3.

Part ii. Tests of effectiveness. The results of Experiment I cast considerable doubt upon the validity of the test of contrary instruction and the concept of associative equivalents which underlies it. Little could be said in explanation of such results at that time. In the present experiment, however, we were able to ascertain when there can be conflicting or heterogeneous activity under contrary instruction, and why. By the use of two complementary procedures, one (Test A) similar to that used by Lewin in his first experiment, the other (Test C) somewhat analogous to the conditions of his second, we were able to demonstrate that the only 'conflict' possible is between the suggestion of the occasion to perform in the old manner and a *formal instruction* to do something different. An *O* trained to follow directions will not normally shift to a former performance. It is a question whether he is then aware of any suggestion which the occasion may offer. If for any reason, however, the occasion presents a strong condition favorable to the former performance, the contrary formal instruction may be neglected.

In Test A, although syllables having associates were shown, their arrangement with neutral syllables presented few cues to the performance usually employed in connection with them. The *Os* have no difficulty in rhyming inversions and inverting rhymes. No *O* rhymed when instructed to invert previously-rhymed syllables; and there were only five cases where inverted syllables were inverted under the rhyming instruction in this test.

In Test C, the situation was such that the *O* had much reason to forsake the contrary instruction and to perform in the accustomed manner. When the first terms of all pairs were shown in serial order with a paper concealing their associate, the *Os* frequently neglected to rhyme inversions and to invert rhymes (as instructed). Results of Test C show 12 intended failure reactions with rhymes and 31 with inversions—a total of 43 as compared with the 5 given in Test A.

TABLE III

Comparison of Reaction-Time to Associated and to Neutral Syllables under Different Occasional Instruction (Tests A and C)

		Reaction-Time (sec.)							
	Syll.-Instr'n	Test A				Test C			
		Rhyming		Inversion		Rhyming		Inversion	
		ave.	m.v.	ave.	m.v.	ave.	m.v.	ave.	m.v.
	r*			0.72	1.4	0.89	1.3	1.02	1.8
J	i	1.30	1.9			1.52	2.7	0.73	1.2
	n	1.20	2.0	0.65	2.3				
	r			1.18	0.9	0.6	1.6	1.34	3.3
K	i	1.30	1.5			1.3	2.9	0.43	1.2
	n	1.20	2.2	1.20	0.7				
	r			0.91	1.0	0.57	2.7	1.25	3.9
G	i	0.83	1.3			1.02	4.4	0.54	2.0
	n	0.84	1.1	0.92	0.6				
	r			1.36	1.4	1.02	2.1	1.63	2.8
B	i	1.41	3.0			1.72	3.9	0.92	1.2
	n	1.40	3.6	1.37	2.2				
	r			0.81	1.8	0.39	0.4	0.84	2.1
Dr	i	0.82	2.6			1.54	7.3	0.41	1.1
	n	0.86	2.5	0.78	2.1				
	r			0.92	1.7	0.59	2.0	1.12	2.2
A	i	1.53	3.7			1.31	2.4	0.48	3.8
	n	1.43	2.9	0.90	2.3	2.30			
	r			0.99	1.4	0.68	1.7	1.20	2.7
Ave.	i	1.19	2.3			1.40	3.9	0.59	1.7
	n	1.16	2.1	0.99	1.7				

*r = rhymed; i = inverted; n = neutral.

The discrepancy between the procedures employed in Tests A and C is further shown by a comparison of the reaction-time to the same syllables under different occasional instruction (Table III). Under the conditions of Test A, there was no indication of a lengthening of reaction-time due to 'conflicting' trends. The average time is approximately the same both for neutral and for associated syllables. Under the conditions of Test C, the reaction-time for associated syllables is considerably lengthened by contrary instruction. The average reaction-time for all *O*s is 0.52 sec. more in the case of rhymes inverted and 0.81 sec. more in the case of inversions rhymed.

Free association. The responses given both to formerly rhymed or inverted and to neutral syllables under free-association instruction (Test B) are given below. There were 45 r stimuli, 45 i, and 90 n.

	Stimulus	Response: Rhyme	Response: Inversion	Response: Due to extraneous operation
J	r*	41	4	
	i	3	42	
	n	41	46	3
K	r	43	2	
	i	6	39	
	n	40	50	
G	r	41	4	
	i	2	43	
	n	43	47	
B	r	24	3	18
	i	5	31	9
	n	16	4	70
Dr	r	23	2	20
	i	2	16	27
	n	4	6	80
A	r	42	2	1
	i	3	42	
	n	20	65	5
Ave.	r	35.6	2.9	7
	i	3.5	33.9	6
	n	27.3	36.3	26.3

*r = rhymed; i = inverted; n = neutral.

It should be noted especially that two-thirds of the total responses made to neutral syllables were either rhymes or inversions. This shows the greater effectiveness of these contexts over those

used in Experiment I, where only 165 of the responses to neutrals showed the effect of the acquired contexts.

Summary of Experiment II. This experiment indicates that intrinsic contexts (supplied by the occasion) aid in the assimilation of series of nonsense syllables. About one-half the number of repetitions was necessary as compared to Experiment I, in which such aid was ruled out. Ach's method of the intended-failure reaction is not, therefore, a test of the efficacy of associative formations, since there is no inherent power in a syllable to call up its associate (Test A). The results of Test C show that intended failure reactions result from reformulation of instruction (due to favoring conditions in this test). A more valid test of the efficacy of associations is their appearance in free association. In Test B, the presence of formerly rhymed and inverted syllables was an aid in determining the operation to be employed upon the neutral syllables. Transfer of a functional performance from one material to another is an indication not so much of the efficacy of discrete associative connections as of the performance which aided in their formation.

Experiment III. Formal Contexts

Part i. Economy in assimilation. The conditions of this experiment are given in its paradigm of page 185. The formal extrinsic contexts were noted by five of the *O*s and utilized by them in varying degrees. The rates of assimilation and the analyses of their individual procedure are given below. It may be generally said that while the suggested contexts were an aid in paired ligation, they were quite ineffective in serial formation.

Da R.A.64.1: G.A. as in Experiment I. She noted at once that there was some sort of 'system' to the paired connections but could formulate the rule only after several repetitions. She did not feel that the rule greatly helped, since it implied visual rather than auditory imagery (her usual type).

Dr. R.A.29.5: G.A. inconstant. She noted that there was some significance to the scoring of certain letters but dismissed the notion that the cue might be of use.

G. R.A.38.9: G.A. as in Experiment I. *G* noted the full significance of the scored letters and utilized it in connection with other aids (such as sound pattern, etc.). In reproduction, he used this aid when the sound-pattern of a pair failed to complete itself.

B. R.A.54.9: G.A. as in Experiment II. She noted the significance of the scoring but did not think it of great aid in assimilating the series.

J. R.A.42.9: G.A. one of weariness with the experiment. *J* used the formal context in assimilation. He reported that, as a visual learner, the scoring of letters set for shift was a great aid in the recall of the second terms of syllabic pairs.

K. R.A.32.8: G.A. less constant than in Experiments I and II. There is no indication in *K's* reports that the suggested context was either noted or employed in assimilation. He used the same system of grouping as in previous experiments.

An indirect indication of the effectiveness of the extrinsic contexts suggested in this experiment is provided by a comparison of the economy of assimilating series of unconventional syllables for the individual *Os*. It is worth noting that three of the *Os* (*G, B,* and *Da* who gave some indication of utilizing the suggested aid) acquired the conventional series with about the same rapidity as they did the unconventional—a fact contrary to the results obtained in Experiments I and II. In Experiment I it took these *Os* an average of 30 more repetitions to learn the unconventional series than the same number of conventional pairs. In Experiment III it took these three *Os*, an average, only 10 more repetitions for the unconventional series than for the conventional. In general, it may be said that the use of a logical aid (of the type suggested in this experiment) tends to compensate for the difficulty experienced with the sound of unconventional syllables.

Part ii. Tests of effectiveness. The 90 neutral and 90 associated syllables were divided into three equal groups and presented successively to the *Os*. If the scoring of certain letters was not utilized in assimilation, then the wrong scoring of the presented syllables in Group I should not effect the correctness of the response. If the scoring was utilized, then the correct designation in Group II should result in a greater number of correct responses than in Group I. If the scoring was of little aid or of no aid whatever in assimilation, then its removal from the associated syllable in Group III should not affect the correctness of the response. In this manner each group constitutes a control on the other, and we are able to check by quantitative means the relative effectiveness of the two types of context (logical and sound) which were made possible by the conditions of this experiment.

The number of correct responses to the 30 associated syllables given in each group was as follows:

	Group I	Group II	Group III
J	8	30	15
G	15	28	17
B	12	24	10
Da	16	28	15
K	30	25	26
Dr	10	16	16

These results indicate that the wrong scoring (in Group I) and the removal of scoring (in Group III) had little effect upon the correctness of the responses of *K* and *Dr*. The other four *O*s, who reported the scoring as an aid to assimilation, did much better with Group II (where the presented syllables were properly scored). The fact that these *O*s were depending more or less upon the scoring to determine their responses is shown by the large number of times the scored letter was shifted in Group I.

	Responses (Group I) to	
	30 associated syllables	30 neutral syllables
J	22	30
G	14	16
B	14	13
Da	13	14
K	0	0
Dr	0	0

In Group II (properly scored), the four *O*s who utilized the logical aids gave more correct responses than did the two *O*s who were dependent upon sound alone.

Experiment III_s (Subsidiary experiment). The rate of assimilation of the three *O*s who served in this subsidiary experiment was very rapid. *A* discovered at once that certain letters were shifted progressively and put his knowledge to serve in assimilating the series. The formal shifts of letters did not become significant to either *F* or *R* until the 40th repetition. At this stage *R*'s rate increased 13 times and *F*'s 16 times. The 90 syllable-pairs given in this experiment were acquired by *A* in 10 repetitions, by *R* in 50 repetitions and by *F* in 60 repetitions. Instead of gradual acquisition (as in Experiment I) there were abrupt rises in the learning curves, generally typical of performances which develop through 'insight' or perceived relation.

Summary of Experiment III. Logical aid of the type suggested in Experiment III leads to greater economy in assimilation and in memorial efficacy than does the aid supplied by sound-pattern alone.

EXPERIMENT IV. SYMBOLIC CONTEXTS

Part i. Economy in assimilation. In this experiment (for conditions see paradigm on page 186f) we sought to approximate the setting in which 'meaningful' sequences of the syllables were employed in varying degrees by five of the *O*s. Their rates of

assimilation indicate the great economy of this type of aid. An analysis of the individual procedure appears below.

Da. R.A.70.3: G.A. as in Experiment I. Only one series (hickory–dickory–dock) carried much symbolic significance for *Da*. This was the first series acquired. The other series were 'simply jingles that I don't make much of.''

Dr. R.A.74.1: G.A. much better than in Experiment III. During the first hour *Dr* apprehended certain "snatches of meaning" in practically all series. She then assumed a self-instruction to find out what the meanings of the total series were. She was successful in acquiring two series almost immediately. Another series (jus–ast) was apprehended as "some hymn that I can't quite get." Before the third observation she had inquired as to the completion of this hymn among her friends. The information gained was utilized in the assimilation of this series. One series (unt–ter) "never had much sense."

G. R.A.69.9: G.A. as in Experiment I. *G* apprehended certain 'meaningful phrases' during the first observation. He was able to get certain of the series to mean as a total (hik–jri, hel–lov, and wah–ure). The others were learned as jingles "without much sense."

B. R.A.46.1: G.A. very weary with the experiment. *B* neither recognized nor utilized the symbolic aids, but learned after the method of Experiment I.

J. R.A.74.4: G.A. as in Experiment I. *J* apprehended certain runs of meaningful sequence during his first observation; thereafter he sought to complete these runs. One series (jus-ast) gave difficulty because he could not "make any sense of four pairs in the middle." Two series (hel-lov and unt-ter) were never 'meaningful' and were learned in "mechanical fashion."

K. R.A.86.3: G.A. as in Experiment I. *K* apprehended the significance of one series the second time that he repeated it. This gave him a cue for proceeding with the other series. The first time that he tried to write down the series he reported some difficulty in remembering the actual nonsense syllable that carried a given significance. This difficulty was overcome at the time of the second writing.

Part ii. Tests of effectiveness. Only a limited number of correct associates were given in the free-association test. *K* and *Dr*, whose rates of assimilation were the most rapid, each gave only 7 correct responses out of 90. *J* gave 25, *B* 23, *G* 43, and *Da* 62. Those *Os* who utilized the symbolic aids least had the highest score. None of the *Os*, responses showed any effect of the acquired symbolic contexts.

Summary of Experiment IV. The use of 'meaningful' sequences in the assimilation of nonsense material resulted in rapid assimilation. The test of free-association did not show great effectiveness of these contexts in determining response. This may be due to the fact that the symbolic contexts refer to the whole series rather than to individual pairs. If this supposition be true, it is easy to understand why it is sometimes better to learn by the

'whole' method and again by 'part.' The whole method is without doubt better for the learning of meaningful sequences.[33]

Discussion of Results

(1) *Quantitative measures of efficacy.* The forms of context utilized in this research varied from the intrinsic aid of *sound-pattern* (Experiment I) to the extrinsic aid of *symbolic relation* (Experiment IV). To estimate the relative efficacy of these several contexts six quantitative measures will be used: (*a*) economy in assimilation; (*b*) influence of the contexts in determining responses to formerly associated syllables (under an instruction for free-association just after complete mastery); (*c*) influence of the contexts in determining the responses to similar materials (neutral syllables); (*d*) influence of the contexts upon formerly associated syllables after an interval of time; (*e*) number of associations correctly recalled after an interval of time; and (*f*) number of promptings necessary for the recital of series after an interval of time. The second, third, fourth and fifth of these measures (*b–e*) refer particularly to contexts employed in paired ligation; the sixth measure (*f*) is more concerned with serial organization. All measures will, however, be grouped together, since each experiment employed both modes of connection.

In the general comparison, no measure can reasonably be given more weight than any other. Whether, for example, economy in assimilation is a better index of effectiveness than is retention after a period of time is itself an interesting question; yet its answer still remains an arbitrary matter. Investigators who have used certain tests in preference to others have frequently assumed that their method is the most adequate measure. We have utilized all tests which were practical and our method of rating these does not involve us in futile controversy.

The contexts represented in the four major experiments to be compared may be classed as follows:

	Intrinsic Contexts	Extrinsic Contexts
Exp. I	sound (paired ligation)	rhythm (serial ligation)
Exp. II	rhyme or inversion (paired ligation)	rhythm (serial ligation)
Exp. III		formal system (paired ligation)
Exp. IV		symbolic relation (serial ligation)

[33]Cf. L. Steffens, Experimentelle Beiträge zur Lehre vom ökonomischen Lernen, *Zsch. f. Psychol.*, 22, 1900, 321-383.

(a) *Economy in assimilation.* According to the rating shown in Table IV (below), Experiment IV is the most efficacious from the standpoint of economy. The rank order of Experiments II and III is not very significant, since Experiment III is ranked higher than Experiment II in the case of *G* and *B*. The individual records of the other four *Os* follow the same rank-order as that given the averages of all scores.

TABLE IV

INDIVIDUAL RATES OF ASSIMILATION IN THE FOUR EXPERIMENTS
(All rates based upon 100 repetitions)

	Exp. I	Exp. II	Exp. III	Exp. IV
J	33.6	47.9	42.9	74.4
K	50.6	62.7	32.8	86.3
Da	55.3	68.4	64.1	70.3
Dr	29.7	54.3	29.5	74.1
B	23.7	48.0	54.9	46.1
G	32.0	55.9	58.9	69.9
Average	35.8	56.2	47.2	68.5
Rating	4	2	3	1

The reports of the *Os* indicate why the material of one experiment is assimilated more easily than that of another. This economy is referred by them as due to differences in procedure employed.[34] But since the rating follows somewhat the order in which the experiments were completed, we must consider alleged 'practice' effects. Investigators in the past have been far too prone to assign such results as ours to the undefined term 'practice' without due consideration of the difficulties involved.[35] It is, of course, very difficult to control a situation in which the organism is given a task and kept at it (under certain conditions) for a period of hours, days, or months. But the assumption commonly made that the performance acquires greater precision with time is not always justified. On the contrary, 'practice' may lead to a cumulative effect which is detrimental to performance (in such a case many have had recourse to the concept of 'fatigue'). As a matter of fact, we can present evidence for both states of affairs in our experiments.[36] But such results are not conclusive one way or the other. The repeated experiments (arranged especially to check practice effects) are more impressive. These results showed no increase in the rate of assimilation with time (the difference is less than the P.E.). Certainly, in our major experiments, additional economy came by the way of changes in

[34] See the detailed analyses under each of the several experiments.

[35] For a discussion of certain of these difficulties see W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 1923, 3, 115-175.

[36] *Da* reported (in Experiment IV) that practice lent a certain 'precision' to her method of organizing the syllables; while *Dr* (in Experiment IV) and *G* (in Experiment III) stated that they had assimilated so many syllables already that the acquisition of further materials was inhibited or impeded. But *Da* required as many repetitions in Experiment IV (the last) as was necessary for her assimilation of the materials in Experiment II. In the case of *Dr* the material in Experiment IV was acquired *more rapidly* than any occurring in previous experiments (we might have expected her record to show the 'fatigue' effect).

procedure rather than by simple practice in 'memorizing' syllables. A very significant study in this connection is that of Fracker who found that acquisition improves by virtue of specific tricks of performance rather than as a result of formal memory training.[37]

(b) *Responses to formerly associated syllables after mastery.* The average ratings received by the several experiments in Table V differ somewhat from those accorded them under the first measure. Experiment IV, which ranked first for economy in assimilation, assumes the lowest rank in this test, and Experiment I ranks third. Those experiments (II and III) which emphasized *paired* connection are rated as superior. This is to be expected, since we have seen that both modes of connection are not equally significant in all experiments.

TABLE V

NUMBER OF RESPONSES SHOWING THE INFLUENCE OF ACQUIRED CONTEXTS UPON FORMERLY ASSOCIATED SYLLABLES IN FREE ASSOCIATION

	Exp. I	Exp. II	Exp. III	Exp. IV
J	63	90	85	43
K	62	89	79	70
G	80	88	76	49
B	22	63	68	23
Da	68	89*	72	70
Dr	0	23	40	49
Ave.	49.1	76.8	70	36.5
Rating	3	1	2	4

*Since *Da* did not take this test in Experiment II we are forced to use the score of the extra *O*, *A*, in this computation.

Again, as in Test 1, there is no significant difference in the averages of Experiments II and III. Experiment II ranks first for *J*, *K*, *G*, and *Da*; while Experiment III holds first place for *B* and *Dr*. On the basis of the averages in the table above, we may conclude that intrinsic contexts of rhyme and inversion and extrinsic contexts referring to letter changes are superior in the free reproduction of former associations. The reader should note that in obtaining the individual averages for the above table both correct and incorrect associates were counted.

(c) *Transfer of the acquired functions to similar materials.* The transfer of an operation from the material originally connected with its use to material of similar character is a test of efficacy very seldom applied; yet the fact is of common enough occurrence. Rules and performances acquired in connection with certain materials are utilized by the organism in meeting somewhat similar situations. Such procedure is a characteristic feature of the teaching of mathematics. Table VI shows that Experiment III suggested operations which were utilized after this manner.[38]

[37]G. C. Fracker, On the transference of training in memory, *Psychol. Monog.*, 9, 1908, (no. 38), 56-102.

[38]Two reasons may explain the lowered efficacy of Experiment III in this test. The average is reduced by the fact that two *O*s (*K* and *Dr*) performed in ways by no means connected with the suggested contexts. The other reason is that the arrangement of the neutral syllables themselves did not always facilitate the use of the context. In spite of this latter fact, the logical contexts were more influential than those of Experiment II for *B* and about as influential for *J*.

TABLE VI

NUMBER OF RESPONSES (TO NEUTRAL SYLLABLES) IN FREE ASSOCIATION WHICH SHOW THE INFLUENCE OF THE CONTEXTS SUGGESTED BY THE FOUR EXPERIMENTS

	Exp. I	Exp. II	Exp. III	Exp. IV
J	42	87	74	2
K	10	90	(0)*	0
G	22	90	32	0
B	3	20	38	(0)*
Da	7	85	14	3
Dr	14	10	0	1
Ave.	14	63.6	26.3	1
Rating	3	1	2	4

*Exception

The marked superiority of the contexts of rhyme and inversion in this test may be due to the ease with which they attach to the material. By instruction, *O* is doing the easiest thing he can. Neutral syllables occurred in the tests given in all experiments. The average number of rhymes and inversions given in response to such syllables in Experiments I, III, and IV was as follows.

	Exp. I	Exp. III	Exp. IV
Rhymes	28.3	33.3	39.3
Inversions	0	12.3	14.1

A comparison of these averages with those of Table VI shows a superiority of these contexts over such as might have been suggested by the associated syllables of the given experiment; *e.g.* only one out of 90 neutral syllables was influenced by the contexts of Experiment IV while over 39 of the rhymes were given in response to these syllables upon the same occasion.

These averages afford a further measure of the effectiveness of two functional dispositions (rhyme and inversion) over a period of time. Evidently rhyming was no new procedure with the *Os*; yet this record shows that they came to employ it more after its use in connection with Experiment II than before. In the case of inverting, we have an even clearer picture. Here an operation quite new to the organism (in Experiment I) becomes functional in connection with certain materials (in Experiment II) and then, divorced from the original items, serves with increasing effectiveness with similar materials (in Experiments III and IV).

(*d*) *Influence of context after an interval of time.* A question frequently asked is what associations are retained the longest. The answer from nonsense syllable experiments has usually been "the ones repeated most *frequently* and those which have been given most *recently*." Test 4 offers possibility of checking the alleged importance of these conditions of impression. As shown in Table VII, when formerly associated syllables are given after an interval of time under free association neither the most frequent nor the most recent associations appear in great number. The associations formed in Experiments I were repeated approximately twice as frequently as those of Experiments II and III; yet this experiment is ranked below the other two. Similarly, the associations of Experiment IV, which are the most recently formed, receive fourth and lowest rank.

TABLE VII

NUMBER OF RESPONSES SHOWING THE INFLUENCE OF ACQUIRED CONTEXTS UPON 30 FORMERLY ASSOCIATED SYLLABLES AFTER AN INTERVAL OF TIME

	Exp. I	Exp. II	Exp. III	Exp. IV
J	9	24	27	7
K	12	21	(16)*	3
G	15	21	21	9
B	4	7	3	(6)*
Da	11	16	25	5
Dr	14	19	21	7
Ave.	10.8	18	18.9	6.1
Rating	3	2	1	4

*Exception

It should be noted that the rating given the general averages compares favorably with the individual records of all *Os* except *K* and *B*. The irregularity in *K's* rating is probably due to his lack of insight into the context suggested by Experiment III at the period of assimilation. The easiest thing for *B* to do in this test was to 'give meanings.' The other *Os* found the readiest response to be in some way connected with the former association. Not all of the responses numbered in the above table were entirely correct. Occasionally the wrong rhyme was given to a formerly rhymed syllable, or a similar sounding response was given to a formerly associated syllable of Experiment I, *e.g.* ets–sto instead of ets–stu. The number of such 'incorrects' were: *Da*, 9; *Dr*, 4; *K*, 4; *B*, 2; *G*, 2; and *J*, 2. These so-called 'incorrect' responses provide another index of the importance of context in associative formation.

(*e*) *Number of associations correctly recalled after a time. Treffermethode* has long been one of the stock procedures with nonsense syllables. Our use of this test after a considerable lapse of time provided an excellent measure of the efficacy of various types of paired associations. Table VIII shows the number of correct associates given to the first terms of correlative experiments. The rating on this test follows the same rank order as the preceding. This is natural, since those types of formation which are easy to recall under instruction for free association should be even more efficacious under the more specific direction.

Experiment III is the only one which shows a significant increase over the score of Test 4. The correct associate was given in practically all cases (except with *K* who did not utilise the contexts suggested by Experiment III). It is

TABLE VIII

NUMBER OF RIGHT ASSOCIATES GIVEN AFTER AN INTERVAL OF TIME TO 30 FIRST TERMS IN EACH OF THE FOUR EXPERIMENTS

	Exp. I	Exp. II	Exp. III	Exp. IV
J	11	20	30	7
K	12	19	(18)*	15
G	13	15	29	14
B	11	18	30	6
Da	9	18	23	9
Dr	16	19	23	5
Ave.	12	18	25.5	9.3
Rating	3	2	1	4

*Exception

also significant that the rank order of Experiment II and that of Experiment III are reversed after an interval of time. In Test 2 and 3, Experiment II is ranked the highest, but with time the contexts suggested in Experiment III become the more efficacious. The reports (taken in connection with Test 5) showed that the superior efficacy of these associations was due to the operation of a mnemotechnical device or context resolved from the assimilative process. All that was needed to provide correct associates in Experiment III was to remember the rule applying to the shift of underscored and overscored letters. The implication contained in these results is that organization, or context, is more significant for retention than are the absolute connections between discrete pairs of syllables.

(*f*) *Amount of prompting necessary for serial recitals.* The method of prompting was used to measure the efficacy of serial ligations after an interval of time. As was to be expected, those experiments (II and III) which emphasized paired connections especially received the lowest rank in this test. Experiments I and IV ranked high by virtue of their well-defined serial integrations. In both of these experiments, the pairs had little significance as isolated units; they belonged rather to a larger organization. A single syllable would not suggest its associate when it was taken from its place in a rhythmical or symbolic sequence. Naturally, Experiments I and IV have been rated rather low in many of the other tests. We cannot properly discount such measures of efficacy on such grounds. These experiments did present incentives for paired as well as serial ligation. The difference between efficacy of these two modes of connection is relative and depends upon the context employed.

TABLE IX

NUMBER OF PROMPTINGS NECESSARY FOR THE RECITAL OF ALL SERIES IN THE FOUR EXPERIMENTS

	Exp. I	Exp. II	Exp. III	Exp. IV
J	24	45	79	41
K	12	26	42	8
G	34	51	39	29
B	53	72	72	(68)*
Da	37	73	46	65
Dr	41	73	74	50
Ave.	33.5	56.6	58.6	43.5
Rating	1	3	4	2

*Exception

It should be noted that the rating of the averages in the above table is not typical of either *K* or *G* (for whom Experiment IV receives first rank) and that the large number of promptings given to *B* (whose procedure was really analogous to those of Experiment I) raises considerably the average for Experiment IV.

For statistical purposes we have arranged the ratings obtained by the four experiments in each of the six tests discussed above. The rank order of these experiments according to this group rating is: II, III, I, and IV. This does not do justice, however, to the individual differences between the several *O*s. The average individual ratings are as follows:

	Average ratings received for all tests by Exp. I	Exp. II	Exp. III	Exp. IV
J	3	1	2	4
K	4	1	(3)*	2
G	3	1	1	(3)*
B	3	2	1	4
Da	3	1	2	4
Dr	3	1	2	2
Ave.	2.63	1.1	1.63	2.33
Rating	4	1	2	3

*Exceptions

The average ratings obtained by this procedure show that the symbolic contexts of Experiment IV are superior to such as were employed in Experiment I. This is a reversal of the rank order obtained by the method employed in obtaining the *group* ratings. Experiments II and III receive the same rating by both methods. A survey of the *individual* ratings reveals an astonishing diversity of relative efficacy. The ratings of *J* and *Da* are the only ones that approximate the *group* rating. The ratings of *G* and *Dr* more nearly approximate the average rating of the individual scores. The rating of *B* and *K* cannot be counted properly, since for *K*, Experiment III, and for *B*, Experiment IV, were acquired in a manner similar to Experiment I.

We need not be too willing to accept either the group or individual ratings as conclusive. The danger of statistical treatment is always that certain important factors are masked, while others less important are augmented by virtue of the quantitative determinations. We shall attempt to interpret these ratings by reference to the nature of the associations formed in each experiment and the adequacy of the various tests to these formations. Only in this manner can we actually be sure that we are weighing relative degrees of efficacy.

(2) *Efficacy of contexts of rhythm and sound-pattern.* The contexts of rhythm and sound pattern, utilized in Experiment I, were the most ineffective from the standpoint of economy in assimilation. They ranked next to last in all the tests of paired ligation. But when the serial ligations were tested after an interval of time, those series in Experiment I were reported with fewer promptings than any other. The position of this experiment relative to the efficacy of Experiment IV is a question. But of the 5 *O*s whose records may be cited, 3 show Experiment IV to be superior to Experiment I.

(3) *Efficacy of contexts of rhyme and inversion.* According to the average ratings, obtained by both methods, the contexts of Experiment II were the most efficacious. Aside from these quantitative measures the *O*s' reports indicate that the contexts of rhyme and inversion are readily accessible in connection with nonsense syllables. Experiment II rated first in two tests, second in three tests, and third in only one. It is probable that

the effectiveness of this experiment lies in the fact that the contexts employed were intrinsic (and so did not involve reading into the material any outside or extraneous significances.) Compared to the *extrinsic* aid of sound pattern alone (Experiment I), these contexts are immensely superior. Compared to the *extrinsic logical* aids of Experiment III, their relative efficacy is more equivocal. If we exclude Tests 1 and 6 (on the grounds that they do not refer especially to paired ligation) we find their rating equally balanced. The individual ratings show that Experiment II is rank first for five out of six *O*s.

(*4*) *Efficacy of logical contexts.* Experiment III ranks a close second to Experiment II throughout the course of our research. In all the measures it will be noted that the average differences between the two are very slight. A more decisive test of the relative efficacy of these two types of context could not be made at the time. But if neutral syllables were given to the *O*s under an instruction for free association after an interval of a year, then we might determine which of the contexts had become more functionally effective. Test 4 indicates that the logical contexts tend to be utilized more freely with time.[39]

(*5*) *Efficacy of symbolic contexts.* It is generally assumed that meaningful sequences of words are the most efficacious of associative formations. Our results do not bear out this assumption. Only in the matter of economy does the symbolic context rank first (Experiment IV, Test 1). It ranks second to sound pattern and rhythm in Test 6 and last in all the other tests. The average individual ratings place it second for three *O*s, and fourth for the other three. It may be argued that our version of symbolic contexts in Experiment IV is not exactly equivalent to those carried by a series of words. But even if this were granted, our results would serve to obliterate the wall which some psychologists would build to separate these *meaningful* sequences from nonsense syllables in general.

(*6*) *Significance of context.* The reports of our *O*s throw considerable light upon the problem of organization and context, which was discussed briefly in the introduction. Two things were

[39]The underscoring of letters offered, of course, a strong tendency to perform in this manner. On the other hand, results from an unpublished research of our own show that the logical shift of letters may go on just as effectively without their objective scoring, both in connection with formerly associated syllables and with new ones.

said to happen during the assimilation of a series of items. The first was the development of the integration of these items. The second was the development of the context of the integrations. We have first to inquire how items are integrated and later how context develops from this process.

It is the common opinion of modern associationists that 'bonds' arise out of repetition of items connected in space and time. But all these factors are external. They are quite foreign to the actual processes underlying the integration of the items. 'Association' itself must also be assumed to be foreign to the items, for by it they are brought together. Our results bear out a contrary contention, namely, that it is the *procedures* during assimilation that give rise to associative organizations.

Ample support for the view that *secondary operations* are essential for associative formation was furnished in Experiment I. There, in the absence of ready-to-hand contexts, *O* created an extrinsic aid in the form of a rhythmical pattern. This was done from apparent necessity. In subsequent experiments the suggested contexts made for increased effectiveness. In every experiment we found the *O*s actively trying to organize the materials in order to get them to adhere in reproduction. The procedures employed in this task are organic. They are functionally related to the items. By making them the factors responsible for the integration of this material we escape the use of external agents and forces. Here 'association' is simply the end product of organization and not an external initiator and 'binder.'

The exact relation of context to the procedures employed in assimilation is, in many instances, far from clear. We could of course begin with the assumption that items are already organized (have a context) and that in assimilation the organism simply apprehends this significance. But this will not explain those cases where no particular organization is suggested. If the organization is always inherent in the arrangement of the material, why will so many differing modes of operation be used in its assimilation? The answers to these questions are not easy. They involve a careful survey of the nature of the assimilative process. Prior to making a detailed study of the development of context we shall quote briefly and at random from the reports of our *O*s. These reports support the contention that *context is simply the result of organic operation.*

J. At first the syllables have little meaning. But as the syllables become more familiar, a way of associating them seems to come in automatically. This is what I should call the second stage of assimilation; the importation of a context that unites the pairs.

Da. I find that I always localize the series in groups. At reproduction, few syllables come that are not within some such (contextual) setting.

Dr. Before the hour was over I suddenly hit upon the idea of making the syllables fit a rhythm. As I said them over in this way, they seemed to take on added significance.

B. Each syllable had its place in this rhythmical cadence.

A. The tying together of the total series seems to have something to do with the rhythm I employ as I say them. That is, the syllables seem to hang together by virtue of the rhythmical 'run' of the sounds.

G. I found that later I was trying to get a meaning to refer to the series as a whole. The change in the way I was working upon the syllables hindered getting their exact form in some cases. I found also that the rhythm of the series followed the pauses in meaning rather than the system I used in the former experiments.

K. I was able to get the significance of all the series today. Having obtained it for one, I set myself to see if there was any (symbolic) meaning to the rest of the series. Only snatches came at the same time and the groupings of the syllables followed these snatches rather than the usual form.

It will be recalled that the *O*s were asked to report upon the various 'stages of assimilation.' We did not have, of course, any definite idea of what these stages were. But we assumed that if assimilation was largely a process of organizing presented materials, certain changes in the course of this process would be observable. A survey of the *O*s' protocols reveals a strong tendency to utilize contextual aids almost from the initial repetition of a series. While individuals vary widely as to specific procedure used, the general progress of assimilation is much the same in all cases. (1) Apprehension of the material, *e.g.* as a series of syllables to be assimilated. (2) An active search for any aids or cues to performance likely to be useful in assimilating the material. (3) A period in which various procedures are tried out in a tentative manner. (4) The selection of a particular performance and the discarding of other extraneous aids. (5) A period in which the selected procedure is perfected and the total performance becomes organic.

It is not our idea that all stages of assimilation are equally well marked. We must also allow the possibility for a progress directly from the first stage to the last (this is true in case an *O* has settled upon a method of organizing the materials prior to their presentation.)

The following are descriptions of the several stages listed above. (1) During the first few repetitions of a new series of syllables *O* is occupied with apprehending the significance of the syllables as discretes. That is, he becomes

familiar with their individual sight and sound. During this time he notes that certain of them are 'harder to say' than others. This is followed by a more specific orientation to the series. It is, for example, "the same length as those of last experiment" or it is found to have new or "novel" features. The following report of *Da* is typical of this initial stage of assimilation.

Da. The first few repetitions were taken merely for purposes of orientation; an attempt was made to familiarize myself with the sound of the syllables, with the number in a series and with the number of series in the experiment. Not until all this information is collected can any definite procedure be settled upon. The syllables are apprehended as being in a series but no associations have been built up as yet.

(2) Very often some cue as to procedure has come out of the initial apprehension of a series of syllables. They frequently suggest their own organization of context (intrinsic.) But in the absence of such aid, the versatile *O* will turn all his functional resources to the task of finding some method (or extrinsic aid) by which the items may be integrated. The second stage of assimilation is primarily a period of search. It is the time when extraneous associations are most abundant. The series reminds the *O* of a "group of Scandinavian words" or it is noted that "the first letters of the last five pairs spell a word."

(3) In the preceding stage various possibilities for organization were turned up for consideration. The next period, which may be either of long or short duration, is occupied with the *trial* of several procedures. A reference to the operations tried by the various *O*s in Experiment I shows how diverse these procedures may be within a single experiment.

(4) As a result of trial, certain procedures are *selected* as holding possibilities while others are discarded from further consideration. In case there is a strong suggestion (in the arrangement of the material) for a particular type of operation, this procedure may be chosen immediately as the most advantageous. But a reference to the detailed treatment of results shows that even here *O* may select some other procedure than the one so carefully arranged for. The following reports are typical of the progress of assimilation up to this point.

Da. Again certain secondary cues were utilized as aids in attempts to memorize; a slight attempt was made to group the syllables. An attempt was also made to link each pair with its absolute number in a series, etc. . . . Among the number of means tried, the matter of grouping seems to offer the greatest possibilities.

Dr. This rhythm works fairly well, I think. I am going to keep at it and see if it will help.

J. The thing that struck me was that there was some pattern or arrangement to the series. First I noted that the letter marked was changed to form the next syllable. I do not see the exact significance of the line as yet, but I have decided to work on it in the hope that it will provide an easy means of learning the syllables.

(5) The stages described above usually take place in a short interval of time. Now there occurs a longer period in which the selected procedure becomes a functional part of the total performance. The performance is very crude at first, but it acquires *perfection* with time. Various embellishments are dealt with successfully. The following reports of *Da* are representative of the development of a context (rhythmical grouping) in connection with serial

ligation of syllables. The reports of *A* are also included as typical of the difficulty occasionally arising from a clash of contexts uniting syllables in pairs and in series.

Da. The larger unit of fifteen was first divided in three smaller units of five each. The first and last member of each of these groups was attended to, and at subsequent repetitions more and more syllables within the group were attached to these pillars.

Today the middle member of each group of five was particularly noted and stressed, so that by the last repetition a great number of pillars were recognized in their respective positions. Then, in order to differentiate more clearly the three groups of five in a series, the first group was projected to the right, the second in the middle and the third to the left.

I found today that special projection upon a piece of writing paper was more effective for reproduction than is the system formerly used. I have found it convenient to count the members of a group of five upon my fingers during the presentations.

Particular attention is still given the initial pair of each group, for I find that if this pair is retained, the rest of its group can generally be reproduced.

A. In general I find that the significance of the pairs as rhymes and inversions gets in the way of the context uniting the total series. I try to get the feel of these rhymes, as rhymes, and yet there seems to be a counter tendency to get them into runs and groupings. Certain runs hang and are a unit irrespective of the paired connections. Again, another principle of group formation comes out. I feel that a group is put together in the same way as we learn to pronounce a long word. We put different sounding syllables together and make a whole of it. Somehow, as soon as we get the merging it is then a unit and means a particular thing.

Orientation, search, trial, selection, and perfection—all of these general performances may be involved in assimilation. But most important, from the standpoint of the organization of the items themselves, are the specific *secondary operations* used in their integration. These procedures are intimately connected with the context which the organized items assume. In fact, association is impossible without a particular organization or contextual setting. But it is not enough to go only this far into the significance of *context*. We must ask how it is developed, whence it comes. We have suggested in this connection that the contexts of associative formations are the results of the secondary operations employed in their organization. The organism sets about the task of assimilating a series of items in a certain way. Sooner or later it becomes aware of its procedure; or the significance may be identified in the items themselves. This exposition is, of course, only a hypothesis. But it offers the possibility of leading us out a bit from that blind alley into which most considerations of context (or meaning) usually take us. Certainly the time has passed when we can treat *context* as a mystical factor in associative formation—a thing to be brought quietly into the realm of psychological thought only where logical distinction fails to provide adequately for its absence.

Conclusions

(1) The alleged conditions of association need to be restated to take into account the organic variables underlying associative formation. Their present form is altogether too gross.

(2) Context is an essential feature of associative formations. The alleged linkage of meaningless items through repetition simply does not occur. The contexts of association result apparently from the procedures or secondary operations used in their organization. Assimilation is simply an active progress towards complete organization, involving many specific changes in procedure and in formulated instructions.

(3) Relative economy in assimilation and in memorial efficacy results from differences in operation and not (as has frequently been alleged) from differences in stimulus material. Nonsense syllables may be used to study many types of contextual aid usually attributed only to meaningful material.

(4) The results of an elaborate investigation of relative efficacies show the intrinsic aids of rhyme and inversion slightly superior to all other aids included in the tests. Logical contexts are almost as effective as the intrinsic aids; they tend to become even more effective with time. Symbolic contexts (typical of 'meaningful' sequences) result in rapid and flashy assimilation but are less organic after an interval of time than are many of the sound rhythms acquired in Experiment I. The efficacy of these latter contexts has been somewhat underrated.

(5) The strength of associations cannot be reliably measured by the method of *associative equivalents*. Intended failure reactions are determined by reformulation of instruction and not according to the 'frequency' concept of *Ach*. Traces of contextual organization are more important in determining what will be reproduced than are the absolute 'bonds' between individual items. *O* frequently reproduces similar organizations which are incorrect so far as the identity of the members is concerned. These functional residues are present whenever conditions are similar to those operative in the original acquisition.

PERCEPTION IN THE REGION OF THE OPTIC DISK[1]

By CARLETON F. SCOFIELD, University of Buffalo

Perception of light when the rays fall upon the blind spot and the interpretation of such phenomena as due to the diffusion of light through the refracting media of the eye have been well established in the tradition of physiological optics. In 1925 two investigators, Stern[2] and Feinberg,[3] challenged this tradition, assured that their results were destructive of any explanation of such visual phenomena upon the basis of diffuse illumination or "benachbarten (falsch) erleuchteten Netzhautpartien." Moreover, Koffka (1924) concluded, in a preliminary report of Stern's investigation, that "it is impossible to explain these movement-impressions by 'higher psychological functions' built on the simple sensations, as, in the case described, these supposed simple sensations cannot be produced."[4] It is with these interpretations that the experimental investigation presented in the following pages is concerned. Disregarding altogether the question of "higher psychological functions," some doubt arises as to whether the simple sensations productive of the movement impression were experimentally excluded in the investigations of Stern and Feinberg. It seems possible that the visual phenomena described by their subjects may be successfully demonstrated to result from a diffusion of light through the refracting media of the eye, with consequent stimulation of sensitive retinal areas surrounding the blind spot. There are several types of this illumination of the eye from indirect excitation.

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[1]The writer wishes to express his gratitude to Professor Raymond Dodge, Institute of Psychology, Yale University, under whose guidance this investigation has been carried out; and to acknowledge his indebtedness to Dr. Harold C. Bingham, Dr. C. M. Louttit, and Dr. Sidney M. Newhall for valuable suggestions and assistance, and to Dr. Ernest M. Ligon, Dr. Roland C. Travis, and Mr. E. R. Hilgard for their services as subjects.

[2]K. Koffka and A. Stern, Die Wahrnehmung von Bewegungen in der Gegend des blinden Flecks, *Psychol. Forsch.*, 7, 1925, 1-16.

[3]K. Koffka and N. Feinberg, Experimentelle Untersuchungen über die Wahrnehmung im Gebiet des blinden Flecks, *idem.*, 16-44.

[4]K. Koffka, The perception of movement in the region of the blind spot, *Brit. J. Psychol.*, 14, 1924, 273.

(1) The specific astigmatic diffusion images due to the inaccuracies of the refracting media of the eye.

(2) A general hazy illumination, such as the halo or balloon of light commonly seen around a bright spot of illumination like an arc light, has been frequently observed and described. This may be experimentally produced by illuminating the cornea from the side. It is probably due, in large part, to the illumination of the numerous semi-opaque spots with which the eye abounds. Vogt (1913) mentions the particles in the lens and humours, comparing their reflections with the actual fluorescence of the human lens: "Die menschlichen Linse, auch die normale, ist nie optisch leer, d.h. in ihrem Innern finden sich stets Teilchen, die eine Reflexion auf die treffender Strahlen herbeiführen."[5] Raman (1919) points out that such visual phenomena "have to be referred to the diffraction of light by a large number of particles of more or less uniform size included in the structure of the refractive media of the eye . . . These structures in the living eye are presumably to be localised in the cornea and in the vitreous humor, as histological evidence of the existence of cellular structures in these bodies is available."[6] Sheard (1919) studied the colored rings and coronae surrounding luminous sources and found them to be due to the diffraction of light by constituent parts of one (or more) of the ocular media.[7] He attributed the rings and the parts of the coronae to diffraction by specific fibers and cellular layers. Michelson (1924) describes the muscae volitantes as due to the impression produced on the retina by the diffraction patterns created by particles in the vitreous humor.[8] Troland (1922) says that "the concentrically laminated structure of the crystalline lens is responsible for the 'rays' seen on points of light against a dark background; general diffusion of light within the eye appears to result from the slight turbidity of all of the ocular media; and the highly specialized structure of the retina itself may produce many detectible modifications in the image."[9] He summarizes the situation as follows: "it is not at all certain that we can arrive at a correct analysis of the retinal functions without taking into consideration the effects of intraocular scatter, diffraction, and absorption."[10]

(3) The extra-orbital illumination, which may proceed from any partially transparent or reflecting object within the total field of vision. Particularly important are such anatomical objects as the nose, cheek, eyebrows, eyelashes, and the edges of the eyelids. The lids are most disturbing, for their mirror-like surfaces give rise to phenomena of extreme brilliance—long bands of light streaming down from the image, or streaking upward. They may be recognized

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[5]A. Vogt, Analytische Untersuchungen über die Fluoreszens der menschlichen Linse und der Linse des Rindes, *Klin. Monatsbl. f. Augenhk.*, 51, 1913, 129ff.

[6]C. V. Raman, The scattering of light in the refractive media of the eye, *Phil. Mag.*, 38, 1919, 568-572.

[7]C. Sheard, Diffraction in the human eye and the phenomenon of colored rings surrounding luminous sources, *Amer. J. Ophth.*, 2, 1919, 185-195.

[8]A. A. Michelson, On the effect of small particles in the vitreous humor, *J. Opt. Soc. Amer.*, 9, 1924, 197-200.

[9]L. T. Troland, The present status of visual science, *Bull. Nat. Res.*, 5, 1922, (no. 27), 65.

[10]*Idem.*

by the fact that their position and brightness with relation to each other change as the head is turned on its vertical axis; their length varies as the eyelids twitch.

(4) The intraocular reflections from the surfaces of the cornea and the lens. These probably become obvious only in the entire absence of other factors. Tscherning (1894) lists seven images within the eye due to these reflections.[11]

Thus it seems quite likely that a beam of light directed solely upon the blind spot may have fractions of itself so distorted by these various refractive anomalies that sensitive elements of the retina are stimulated. The working hypothesis was adopted, therefore, that the visual phenomena which occur when stimulating light-points fall within the optic disk are the result of diffuse stimulation of sensitive retinal elements surrounding the disk, as a consequence of the dispersion of the stimulating light by and through the refracting media of the eye. Confirmatory evidence of this hypothesis would seem to be indicated if the visual phenomena were found to be dependent upon the intensity of the stimulating light-point and upon its position relative to the surrounding retinal areas. That is to say, a beam of light falling upon the center of the blind spot would be attended by less perceptible diffusion than one of equal intensity falling within the disk, but close to its edge, for the latter would carry diffuse illumination to retinal areas more distant from the blind spot and more intense illumination to those nearby. If, then, when maintaining a fixed size of the object, it should be found that the intensive threshold for the edge of the blind spot is lower than that for the center, diffusion would appear a not unreasonable basis for this perception of light when sensitive retinal elements are not directly stimulated. Further, if a ray of light falling within the optic disk, and close to its edge, is suddenly increased in intensity, there will probably ensue a quick increase of diffusion, which may readily be perceived as a sudden spreading out of illumination, or even as movement in the general direction which the image occupies relative to the center of the disk. These represent the more specific problems with which this investigation is concerned.

I. Thresholds for Intensity

Apparatus. With the first problem thus defined as a quantitative study of intensive thresholds for two different areas of the optic disk, it was clear that the technique must be designed to

[11] M. Tscherning, Un reflet intra-oculaire, *Arch. de. Physiol.*, 6, 1894, 158-162.

control particularly the following variables: (1) the intensity of the stimulating light; and (2) the specific area of the blind spot affected, a factor dependent upon adequate control of fixation.

The apparatus, which may be called a tachistoscope for illumination of the optic disk, is shown in Figs. 1 and 2. It consists of five parts: a base (Fig. 2 A, A_1, A_2), which serves merely to support the other parts and to bring about their proper spatial interrelationship, a head-rest (B), a field-screen (C), a control-shutter (D), and a light-box (E).

The light-box is 143 cm. by 19 cm. by 19 cm., open at one end, the other end equipped with a milk-glass window (a), 5.5 cm. by 12 cm. The source of illumination is a 75-w. mazda lamp, so fastened to a wooden base as to lie horizontal, with the plane of its horseshoe filament parallel to the surface of the milk-glass. A long wooden rod attached to the lamp-base and extending through the open end of the light-box permits movement of the lamp toward or from the milk-glass window, with consequent variation of the illumination at that surface. The upper surface of the rod bears a scale of light-units (constructed according to the Inverse Square Law), which is read against a beveled brass strip across the open end of the light-box.

The field-screen (C), a black screen large enough to conceal the light-box and control-shutter and to present a relatively homogeneous visual field, stands between the light-box and the subject, 6.4 cm. from the milk-glass surface. In the screen, concentric with the approximate center of the milk-glass window, is a circular aperture, 5 cm. in diameter. To the *S*'s left of this aperture lies the fixation-object. It consists of a square of black cardboard, 9 cm. by 9 cm., mounted on a black thread which passes around the field-screen. This part of the device is invisible against the background of the screen, and at the same time is adjustable either vertically or horizontally by movement of the thread in appropriate directions. A rubber band inserted in the course of the thread facilitates this adjustment. On the black square is a white cross, whose arms are 8.7 cm. long by 0.5 cm. wide and are traversed by black lines 0.5 cm. apart. At the junction of the white arms is a small black cross, 0.5 cm. square, which serves as the actual fixation-point, while the arms of the white cross are a device for centering the 5 cm. aperture within the optic disk.

The head-rest for *S* stands about a meter distant from the field-screen and is constructed to bring *S*'s eyes on a level with the 5-cm. aperture. It is so attached to the base as to form an angle of approximately 20 degrees with the surface of the field-screen, thus directing *S*'s line of sight for the normal position of the eye in the general direction of the fixation-object rather than toward the aperture. Three points of contact fix the position of the head—the forehead, the bridge of the nose, and the under surface of the chin. The forehead-rest is a wooden block cut to fit the forehead. To its under surface is attached, by means of a thumb screw, a small piece of wood shaped to fit the bridge of the nose, and adjustable thereto. The chin-rest is a wooden block brought up tight under the chin after forehead and nose have been properly fixed.

Vision is excluded from the left eye (only the right eye was utilized throughout the investigation) by means of a small square of black cardboard attached to the under surface of the forehead-rest.

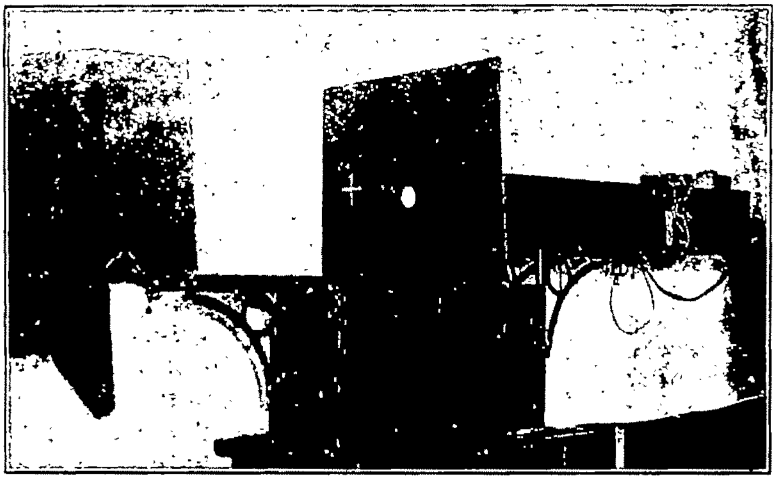


Fig. 1. Photograph of the Apparatus

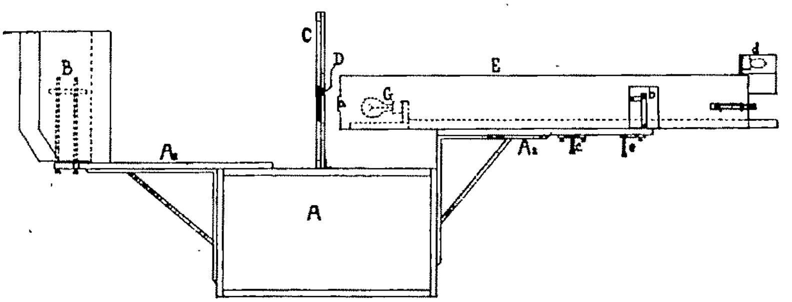


Fig. 2. Outline Drawing of the Apparatus

Reflection of light from the eyelashes and eyelids is eliminated by the use of a small screen of blackened tin, 4 cm. by 7.5 cm., its edge to S's right pivoted to the horizontal arm of a right angle, the vertical arm of which is pivoted to the forward surface of the forehead-rest. The screen is thus brought within 2 cm. of the cornea of the right eye. In the upper left corner of this screen is a horizontal slit, 8 mm. by 3 mm., which permits inclusion of both the fixating device and the stimulus-aperture within the visual field. Sufficient vertical and horizontal adjustment of the slit is secured by the pivotal attachments at the screen and forehead-rest. The distance from the eye-slit to the field-screen is 87.2 cm.

The control-shutter (D) determines the position which the stimulus-object occupies within the 5-cm. aperture. It is a strip of black cardboard containing a circular hole 4 mm. in diameter and sliding either vertically or horizontally immediately behind the aperture in the field-screen, 5 mm. from the screen and 5.8 cm. from the milkglass. A projecting arm from the shutter carries a pointer which passes over a scale calibrated with the position of the image in the aperture.

Experimentation took place in a dark-room, in a general illumination produced by a 60-w., carbon-filament lamp, hanging in a reflecting shade against the wall, behind and above *S*. The perpendicular distance from the lamp-filament to the level of the center of the stimulus-aperture is 135.5 cm., and the horizontal distance from the lamp-filament to the plane of the field-screen is 209.6 cm.

Since the open end of the light-box lay in the shadow of the field-screen, there was attached to the upper surface of the box at that point a 10-w. carbon-filament lamp (d in Fig. 2), so screened as to permit reading of the scale without throwing light in the direction of *S*. A double-throw switch (c) controls both this reading-lamp and the source-lamp, the wiring being such that both can never be illuminated at the same time.

Except for the necessary observation-opening, the *S*'s head is inclosed, so that all extraneous light, which might cause reflection from the various parts of the head-rest, is excluded. All parts of the apparatus were painted a dull black.

Control of fixation. Control of the specific area of the blind spot affected is naturally of vital importance in the problem. What assurance is there that the stimulus-light actually falls within the blind spot? If one attempts to have the stimulus-image fall within the disk, but close to its edge, how can one be certain that unavoidable shifts of fixation do not bring the image upon sensitive areas of the retina? The problem of fixation has been a

source of error and discouragement in much visual experimentation, and has yet, it seems, to be adequately solved.

The device used in this investigation has been relatively satisfactory. At the initial experimental session with an *S*, two adjustments of it are necessary. A piece of white cardboard is inserted immediately behind the 5-cm. aperture in the field-screen, so that *S* faces a black field, in whose center is a 5-cm. white circle. In the first, or crude, adjustment the fixation-device is merely brought to a position where fixation of its central black cross causes the 5-cm. aperture to disappear in the blind spot. *S* is then instructed to fixate each of the black transverse lines of the right horizontal arm of the white cross, beginning at the center, reporting at which line, or between which lines, he first gets a glimpse of the white circle. He then does the same on the left horizontal arm. If he reports the white first glimpsed at the second line to the right and at the fifth line to the left, the blind spot is clearly not horizontally centered about the aperture; and the fixation-device is moved until the white is first glimpsed at the same spot on either horizontal arm. When this same process has been gone through for the vertical, one may assume that the 5-cm. aperture is pretty well centered within the blind spot so long as fixation is held upon the central black cross of the fixation-device. Thus it follows that if the control-shutter is so adjusted that the stimulus-object is in the center of the aperture, that object falls reasonably close to the center of the optic disk; or if it is near the edge of the aperture, it falls rather near to the edge of the disk.

During the course of experimentation the blind spot was centered at the beginning of each group of presentations, and its centering verified at the end of each group; but it was rarely found to be more than half-a-division off, and had usually not wavered perceptibly. It was also evident that if the fixation wavered at the moment of presenting the stimulus, the phenomenon observed was so different as to bring from *S* an immediate report of lost fixation.

A further advantage of the device is what might be called a self-contained check. The negative after-image of the white cross clearly appears whenever the fixation wavers, and so *S* may be instructed to use its appearance as a check upon his fixation.

After an initial session, measurements of the positions of the fixation-device, of the nose-piece, and of the chin-rest are taken for each *S*, so that they may be re-set for succeeding sessions.

A somewhat similar device for fixation, involving a means of centering the blind spot about a stimulus area, was used by Feinberg in 1925. In the screen which his *S* faced were cut, at the left, a horizontal slit, behind which the fixation-point could be moved, and, at the right, a system of slits radiating in eight directions from a circular center. "Der Fixationspunkt konnte so eingestellt werden, dass das Zentrum des blinden Flecks mit dem Zentrum des Schlitzsternes zusammenfiel." Just how this was accomplished is not described; but since Feinberg's first procedure was to map out the blind spot along the rays of the system of slits, it is probable that having measured the distance from the center of the mapped area to the position of the fixation-point used in the mapping process, he then moved his fixation-point so that the center of the mapped area coincided with the center of the "Schlitzstern."

Procedure. Preliminary experimentation with eight *S*s gave immediate evidence of a difference between the thresholds for the center and edge of the optic disk and confirmed the adequacy of the fixation-device; but it also revealed the frequent occurrence of bi-retinal rivalry during the course of observation. To clear it, *S* was instructed and signaled to wink immediately before the presentation of the stimulus. This control, combined with *S*'s own awareness and report of the presence of the rivalry, proved fairly satisfactory, but by no means complete.

Using two *S*s from this preliminary group and three who were without experience in the experiment, intensive thresholds for the center and for the edge of the optic disk were secured by means of the Method of Constant Stimuli. For three of the *S*s, limens were secured for the center of the blind spot and the edge to the *S*'s right, *i.e.* the right of the projected blind area; for the other two, they were secured for the center and the edge to the *S*'s left.

The experimental procedure was relatively constant, varying only with the exigencies of the session. After a fixed period of dark-adaptation, adjustments of the head-rest were made, and *S* proceeded to center the blind spot around the stimulus-aperture in the manner already described. The instructions to him were as follows:

> You will please keep your head as immovable as possible throughout the session.
>
> Maintain *constant* fixation upon the central black cross of the fixation-device, using the position of the after-image as a check.
>
> Direct *attention*, without loss of fixation, toward the periphery.[13]
>
> When *E* says 'Wink,' do so and quickly regain fixation, *opening your eyes as widely as possible.*
>
> You will then hear the following warning signal (two short raps), and one second later there may, or may not, be a change in the illumination of your right eye. If you perceive any such change, report by rapping. If not, remain silent.
>
> After about five observations the experimenter will say 'Series,' after which you may wink your eyes naturally as frequently as you wish, until you again receive the signal to wink.
>
> If at any time your fixation has been poor or you were in any other way not 'set' for the stimulus, report to that effect.

The detailed procedure in presenting each stimulus, then, was: (1) signal to wink, two seconds; (2) warning signal, one second; (3) stimulus on, one second; (4) stimulus off. These times were determined merely by watching the moving hand of a stop watch.

Five equally spaced stimulus-values were selected for each of the two positions of the object, and each value was presented one hundred times,

[13]The *S*s understood this to mean that, although fixation was upon the black cross, attention was to be directed toward the stimulus-aperture.

making a total of one thousand observations, and allowing each limen to be calculated upon the basis of five hundred cases. This was accomplished in seven sessions for each *S*. In addition to ten practice exposures, 150 (75 for each position) were presented in each of the first six sessions, and 100 in the seventh. These were presented in four groups of 35 and 40 each, the *S* being permitted a 3-min. rest between each group. Within the group the stimuli were presented in chance order in series of 5, with a few seconds rest between each series. The sessions were usually held at the same hour upon successive days.

Results. Calculation of the limens from the data obtained gave the results shown in Table I. These values are in terms of the arbitrary scale of light-units. Their equivalents in meter-candles may be determined by reference to the values in the table of photometric readings which appears at the conclusion of this paper.

TABLE I

INTENSITY THRESHOLDS FOR THE CENTER AND MARGIN OF THE BLIND SPOT

S	Lc	PE	Lr	PE	Ll	PE	Diff.	PE
H	281.47	6.29	165.87	5.79			115.60	8.59
Li	131.39	4.64	104.64	3.56			26.75	5.85
Lo	259.74	5.07	186.76	4.56			72.98	6.82
N	137.73	5.31			104.77	3.95	32.97	6.62
T	66.15	4.10			51.73	3.80	14.43	5.59

Lc = limen for center of blind spot; Lr = limen for edge of blind spot to *S*'s right; Ll = limen for edge of blind spot to *S*'s left; Diff. = difference between the two limens.

There is a significant difference between the two thresholds. The curves of the frequencies obtained experimentally coincide consistently with the theoretical psychometric function curves based upon the data obtained, except in the case of the Lr for *H*. Here there occurs an inversion of the first order. The difference between his two thresholds—greater than in the case of any other *S*—is so great, however, as to demand recognition, despite the unreliability of the calculated Lr. With the exception of the results for *H*, the ratio of central to marginal threshold is approximately constant.

Effects of practice. Although no special attempt was made to study these effects, the observations were scattered over a prolonged period and are in sufficient quantity to permit approximation of a practice curve. There was nothing, either in the exploratory or later investigation, which could be unequivocally interpreted as a lowering of the threshold after practice. Whatever increase in sensitivity occurred was immediate (within the first

session or two), and seemed due to the attainment of familiarity with the phenomenon to be observed. This is in agreement with Seashore and Kent's (1905) conclusion that there is "no noticeable gain from practice in simple perception and simple discrimination, after the observer has clear knowledge of the nature of the stimulus."[13]

For one *S*, *Lo*, the data superficially gave something like the appearance of a drop in the threshold due to practice. The drop, however, was sudden rather than gradual and accompanied by an introspective report which suggested something different. There are, for this *S*, preliminary data gathered over a period of seventeen days, observations having been made daily. For the first thirteen days the threshold for the center of the optic disk ran along at a point varying between 300 and 400 light-units. On the fourteenth day it suddenly dropped to a point between 200 and 260 and remained there, not only for the remaining four days of this particular period, but throughout the investigation. The effects of practice, of course, are not evidenced by so sudden a shift. The introspective report mentioned was volunteered by the *S*, and came at a time in the extended period of experimentation when he had long since ceased to report on the nature of the phenomenon, but had become familiar with it and settled down to quantitative response. The report was as follows:

> "Light is diffused and thrown into whole eye, while previously it has been more of a beam. Now conscious of it more when it goes out than when it comes in."

Evidently some sort of subjective shift took place. What happened? Did the *S* change his "Aufgabe?" His criteria? His attitude? Was there a sudden "Einschnappen" and closure of the "Gestalt?" Whatever occurred, it can scarcely be attributed to practice.

Observers' reports. The following facts represent a summary of oral and written reports obtained from ten *S*s after or between experimental procedures.

There was general agreement that the light seemed to issue indirectly rather than from a direct source; some *S*s suggested that it was reflected rather than direct light. Usually the phenomenon was reported as only a change in illumination, presenting no shape other than an indefinite, roughly circular haze. "Big balloon," "flash," "bright haze," "glow," "indirect illumination of the whole retina," "distinct shine," and "irradiation within the eye," were some of the descriptive terms used in the retrospective accounts. When the stimulus-object was on the edge of the blind spot, three *S*s occasionally reported it to be crescent-shaped. One reported that at the lowest intensities his cue for a change seemed, in rare instances, to be of a tactual nature rather than visual, because there seemed "no definite visual sensation, yet the major portion of the retina seemed affected."

[13] C. E. Seashore and G. H. Kent, Periodicity and progressive change in continuous mental work, *Psychol. Monog.*, 6, 1905, (no. 28), 46-101.

The seat of stimulation was not readily localized by any of the *Ss*. Some localized it within the eye, others at the screen; two were able to localize the change at times out in the field, in the general vicinity of the screen, and at other times actually within the eye. Indeed, they sometimes found it necessary to concentrate attention upon either one of the two places.

Of particular interest was the quite universal report that at the lower intensities the change in illumination was most frequently perceived as the light "went off" rather than as it appeared.

Two of the *Ss* reported considerable disturbance from the fluctuations of the field, particularly at the lower intensities.

II. Apparent Movement

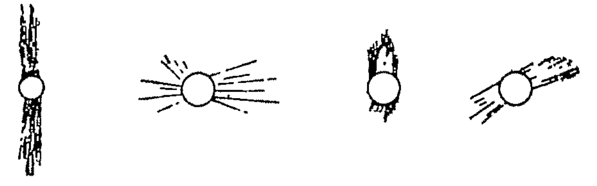
Apparatus. The apparatus described above was used in this second part of the investigation, with modifications to produce the sudden increase in the intensity of the stimulus.

A second source-lamp, mounted upon a carriage similar to that of the source-lamp already described, and moved in the same manner, *i.e.* with a wooden rod bearing a scale of light units, was placed in the light-box, immediately behind the original lamp. The base upon which this second lamp was mounted was so elevated that its illumination would not be interfered with by the shadow of the forward lamp. The wiring of the two lamps was such that closure of one switch illuminated lamp A, while closure of a second switch illuminated lamp B, permitting lamp A to remain lighted. The intensity of the stimulus-light could thus be instantly increased from that of lamp A to that of A plus B. Since the second lamp could not be constructed to overlap completely the forward one, the maximal intensity obtainable from the former was 525 light-units, making a total obtainable intensity of 1525 units. The same factor of structure limited the size of the intensive differences obtainable, but not sufficiently to restrict experimentation; for it was possible to increase the intensity of the stimulus from threshold by any number of units up to 1000. It must be clearly understood, however, that this summation of the stimulus values of the two lamps upon the basis of their respective positions on the scale is pure assumption. The combined brightness of the two lamps, each set at 500 units, is probably not 1000 units; but for our purposes it is a sufficiently close approximation to twice the value of either one of them.

Exploration. In preliminary experimentation it was found that for two *Ss* familiar with the expected response a quick increase in the intensity of the stimulus-light caused a spread of illumination easily interpreted as movement, a movement which seemed to be to the left when the stimulus-light was exposed on the left margin of the projected blind spot, and to the right when exposure was on the right margin. Whether the uninformed *S* would get such an illusion was questionable. When the situation was presented to four of the *Ss* used in the previous part of the

investigation, three of them perceived movement in 50% to 60% of the cases; but the fourth persistently failed to perceive the phenomenon. In the cases of movement reported, 55% to 85% were reported as occurring in the expected direction.

Since the stimulus-light had thus far been presented only upon the horizontal diameter of the blind spot, the *S*s were next asked to view it foveally, and to draw on paper as accurately as possible the image seen, attending particularly to radiating streamers and other irregularities of contour. They made drawings for a number of different intensities, ranging from 65 to 1525 light-units. Some of the drawings are reproduced in Fig. 3. The *S* who saw move-



(1) (2) (3) (4)

FIG. 3. TYPICAL FORMS OF THE STIMULUS-LIGHT AS VIEWED FOVEALLY

ment most frequently, and in the expected direction 85% of the time, showed clear evidence of horizontal astigmatic lines, a factor which might readily be conceived as facilitating the diffusion of light horizontally, and so increasing the illusion of movement. Moreover, the *S* who failed to see movement showed equally clear evidence of vertical astigmatic lines, which might conceivably tend to inhibit the illusion of movement when the stimulus-light fell on the right or left edge of the blind spot, but to facilitate it if the light fell on the upper or lower edge. When the stimulus was later presented to this *S* on the vertical diameter of the disk, he perceived movement. In view of these observations it seemed desirable to submit the *S*s to this test of their astigmatic images, and having determined the dominant direction thereof, expose the stimulus-light on the corresponding edges of the disk.

Procedure. Under the suggested hypothesis it seemed possible that if seven equidistant positions were marked off on either the horizontal or vertical diameter of the disk, three on each side of the center, the number of cases seen as movement would increase for each position as one approached the edges, provided the intensity were maintained constant relative to the threshold of the

particular position. In other words, regardless of whether the *S* saw movement when first subjected to the conditions of the impression, if he could be trained by means of proper *Aufgabe* to see movement, the problem would then be to obtain a relatively objective and quantitative measure of this perception. If the selected positions across the diameter of the blind spot were numbered from left to right or from below upward, 1, 2, 3, 4, 5, 6, 7, and it were found that for positions 1 and 7 movement was seen in the expected direction in, for instance, 90% of the cases, for positions 2 and 6 in 75% of the cases, for positions 3 and 5 in 60%, and 50% in position 4, there would seem to be some ground for the hypothesis.

Assuming the thresholds for positions 1, 2 and 6, 7 to be lower than those for positions 3, 4, 5, the intensity values indicated in Table II were empirically and arbitrarily selected for the two degrees of illumination. For all *S*s the setting of lamp A was well above liminal intensity.

TABLE II

INTENSITIES SELECTED TO PRODUCE THE TWO DEGREES OF ILLUMINATION

S	Lamp A Positions		Lamps A plus B Positions	
	1,2,6,7	3,4,5	1,2,6,7	3,4,5
H	250	300	1250	1300
Li	300	350	1300	1350
Lo	400	500	1400	1500
N	300	350	1300	1350
T	125	150	1125	1150

The experimental procedure was relatively constant, varying only with the exigencies of the session. After a fixed period of dark adaptation, adjustments of the head-rest were made and *S* proceeded to center the blind spot around the stimulus-aperture in the manner described above. The instructions to him were as follows:

> You will please keep your head as immovable as possible throughout the session.
>
> Maintain *constant* fixation upon the central black cross of the fixation-device, using the position of the after-image as a check.
>
> Direct attention, without loss of fixation, toward the periphery.[14]
>
> When *E* says 'Wink,' do so and quickly regain fixation, *opening your eyes as widely as possible.*
>
> You will then hear the following warning signal (two quick raps), and one second later there will be an illumination of your right eye which

[14]As before, in Part I, the *S*s understood this to mean that, although fixation was upon the black cross, attention was to be directed toward the stimulus-aperture.

may, or may not, move to the right or left. If the movement is to your right, rap once; if to your left, twice. If the image appears not to move, rap three times. If you perceive no illumination, remain silent.

In the interval between your report and the signal to wink you may wink your eyes naturally as frequently as you wish.

If at any time your fixation has been poor or you were in any other way not 'set' for the stimulus, report to that effect.

The detailed procedure in presenting each stimulus, then, was: (1) signal to wink, two seconds; (2) warning signal, one second; (3) lamp A on, one second; (4) lamp B on, one second; (5) stimulus off. This succession and these time intervals were empirically found to be optimal.

Seven hundred stimuli were presented, 100 for each position of the stimulus-light. This required ten sessions, each consisting of 10 practice and 70 regular presentations. In each session the 80 stimuli were presented in three groups, with a 3-min. rest-period between each group; while within the group they were presented in series of five, with a few seconds rest between each series. The stimuli were presented in chance order. The sessions were usually held on successive days and at approximately the same hour.

Results. Guided by the astigmatic images of the individual, the seven positions along the horizontal axis of the blind spot were used for two of the *S*s, and the positions along the vertical axis for the other three.

The results are shown in Tables III to V. Table III gives the frequency with which the *S*s reported a perception of movement for each of the seven positions across the axes of the blind spot. These results are by no means positive. Although the totals show a tendency for the perception of movement to decrease as the stimulus-light recedes from the edges of the blind spot, it is readily seen that *Lo* and *T* are the only *S*s who show an unequivocal tendency of that nature. The former shows an almost ideal progressive decrease toward the center, but his data are based on only 50 cases, as compared with 100 for the other *S*s.

TABLE III

PERCENT OF TIMES THAT THE *S*S PERCEIVED MOVEMENT AT EVERY ONE OF THE SEVEN POSITIONS

	Cases	Axis of Blind Spot	Positions 1	2	3	4	5	6	7
H	100	Horizontal	79	82	79	81	83	93	88
Li	100	Vertical	87	87	87	88	78	82	80
Lo	50	Vertical	94	60	40	8	16	60	78
N	100	Horizontal	68	67	74	67	47	59	76
T	100	Vertical	81	75	61	43	63	77	79
Totals			82	74	68	57	57	74	80

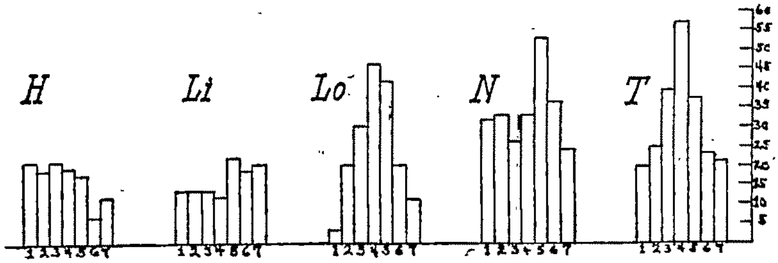


FIG. 4. SHOWING FOR EVERY *S* THE FREQUENCIES OF THE REPORTS OF NO MOVEMENT FOR EVERY ONE OF THE POSITIONS OF THE STIMULUS

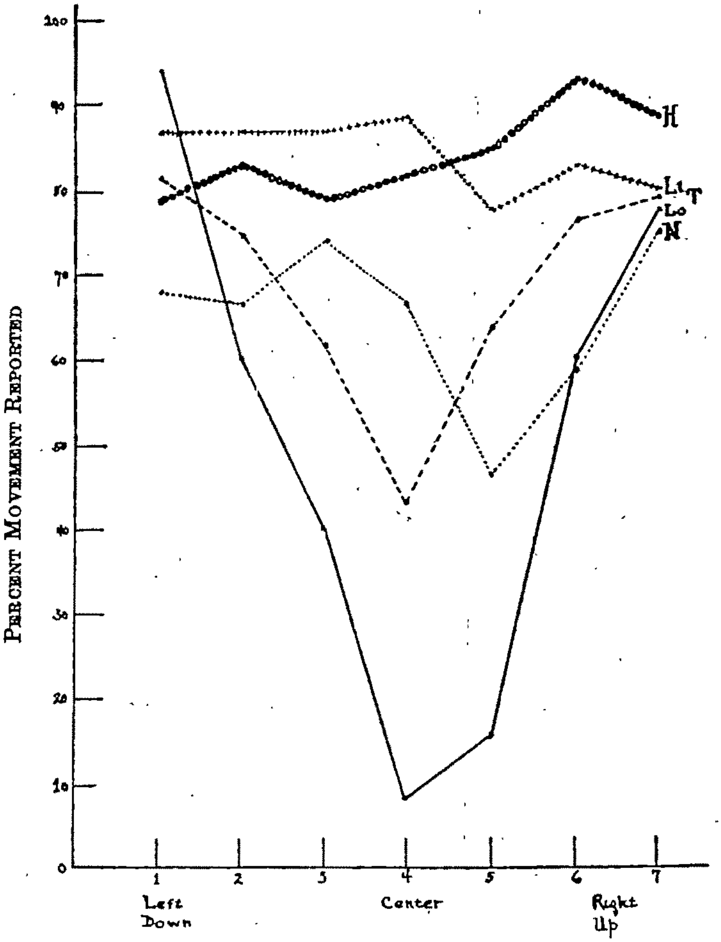


FIG. 5. SHOWING FOR EVERY *S* THE PERCENTAGES OF MOVEMENT REPORTED FOR EVERY ONE OF THE POSITIONS OF THE STIMULUS

The frequency-graphs for the individual *Ss* (Fig. 4) show more clearly, perhaps, the status of the results. In these has been plotted, not the frequency of movement perceived, but the frequency of the absence of the perception of movement. Theoretically, then, on the basis of the suggested hypothesis, these graphs should show decreasing frequency with the distance from the central position. It is evident that *H* and *Li* show nothing positive. *N* shows a slight, but rather equivocal, tendency toward a central predominance of no-movement frequency. *Lo* and *T*, on the other hand, show unambiguous evidence of this tendency. The curves of the percentage of movement reported for each of the seven positions appear in Fig. 5. Here again the evidence is equivocal.

Turning to the data concerned with the direction of the movement reported, the evidence grows a little more positive. Table IV shows the frequencies and percentages of the movement reported as "Right" or "Upward;" Table V the frequencies and percentages of movement reported as "Left" or "Downward." The data for the two axes are grouped in this manner, since, for position 1, movement to the left is anticipated on the horizontal axis and movement downward on the vertical axis; while, for

TABLE IV

MOVEMENT REPORTED AS TO THE RIGHT OR UPWARD

S	Axis of Bl.Sp.	Frequency 1	2	3	4	5	6	7	Percentage of the Movement Reported 1	2	3	4	5	6	7
H	Horiz.	11	24	41	57	65	73	66	14	29	52	63	78	78	75
Li	Vert.	35	39	45	57	58	63	64	40	45	51	65	74	77	80
Lo	Vert.	0	1	0	4	6	28	39	0	3	0	100	75	93	100
N	Horiz.	11	15	24	29	19	38	63	16	22	32	43	40	64	83
T	Vert.	10	7	10	36	57	69	69	12	9	16	16	90	90	87
Totals		67	86	120	177	205	271	301	16	22	30	57	71	80	85

TABLE V

MOVEMENT REPORTED AS TO THE LEFT OR DOWNWARD

S	Axis of Bl. Sp.	Frequency 1	2	3	4	5	6	7	Percentage of the Movement Reported 1	2	3	4	5	6	7
H	Horiz.	68	58	38	30	18	20	22	86	71	48	37	22	22	25
Li	Vert.	52	48	42	31	20	19	16	60	55	49	35	26	23	20
Lo	Vert.	47	29	20	0	2	2	0	100	97	100	0	25	7	0
N	Horiz.	57	52	50	38	28	21	13	84	78	68	57	60	36	17
T	Vert.	71	68	51	7	6	8	10	88	91	84	84	10	10	13
Totals		295	255	201	106	74	70	61	84	78	70	43	29	20	15

position 7, movement to the right and upward are anticipated. The percentages in these tables are not percentages of the total stimuli presented, but percentages of those stimuli for which movement was reported, *i.e.* percentages of the frequencies given in Table III.

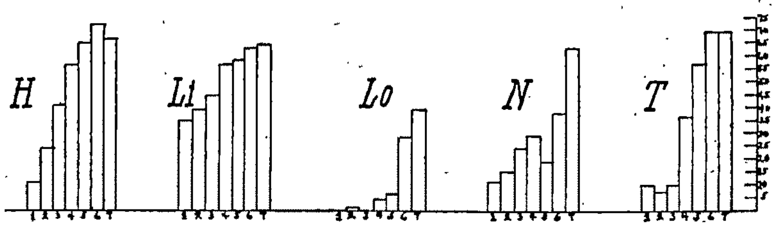


FIG. 6. SHOWING FOR EVERY *S* THE FREQUENCIES OF MOVEMENT REPORTED AS RIGHT OR UPWARD FOR EVERY ONE OF THE POSITIONS OF THE STIMULUS (MOVEMENT IN THE CASES OF *H* AND *N* WAS TO THE RIGHT, AND IN THE CASES OF *Li*, *Lo*, AND *T* IT WAS UPWARD)

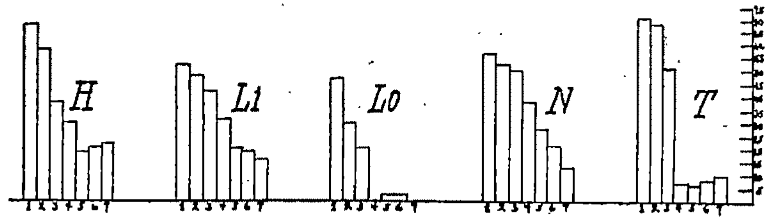


FIG. 7. SHOWING FOR EVERY *S* THE FREQUENCIES OF MOVEMENT REPORTED AS LEFT OR DOWNWARD FOR EVERY ONE OF THE POSITIONS OF THE STIMULUS (MOVEMENT IN THE CASES OF *H* AND *N* WAS TO THE LEFT, AND IN THE CASES OF *Li*, *Lo*, AND *T* IT WAS DOWNWARD)

Frequency-graphs of these data are presented in Figs. 6 and 7. Here the evidence seems clear that the apparent movement is seen more frequently in the anticipated direction as one approaches the edge of the blind spot in that direction. The progression is not always gradual; but in every case there is an unambiguous difference between the frequencies for positions 1 and 7. The percentage-curves plotted for the same data are shown in Fig. 8. These curves are complementary to each other, since they are based upon the percentage of the percentages of movement reported. If the two curves are combined and only the parts of each above the 50% point considered, one has a fairly clear picture of the general trend of the perception of the direction of movement in relation to the particular area of the blind spot affected.

It will be noticed that in no case does the 50% junction-point of these curves fall precisely upon position 4, the center of the blind spot; indeed, in some cases it falls some distance from the

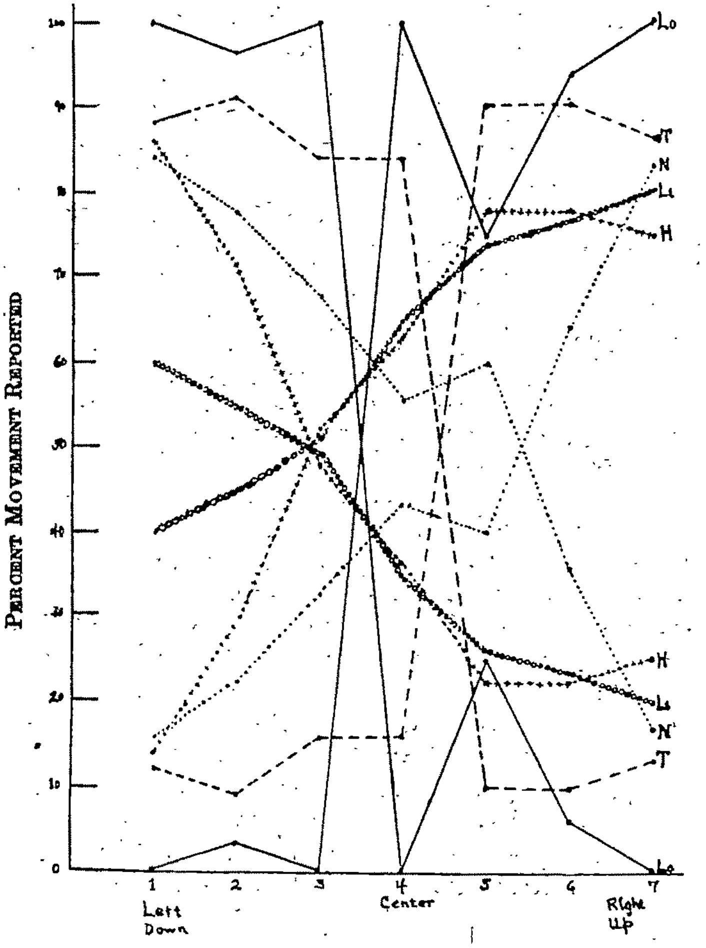


FIG. 8. SHOWING FOR EVERY S THE PERCENTAGES OF MOVEMENT REPORTED FOR EVERY ONE OF THE POSITIONS OF THE STIMULUS

center. It ought to be suggested, perhaps, that such a result might be due to an inadequate centering of the blind spot around the stimulus-aperture. Although carefully centered by the fixating

device, it is conceivable that the irregularities in the contour of the disk might so distort that centering as to produce the shifts of the 50% mark shown on the curves.

Observers' reports. The reports from this part of the investigation are relatively meager. So far as the illumination itself is concerned, the *S*s' descriptions of it were not different from those cited above (cf. page 221). The apparent movement was universally indistinct and uncertain, yet all agreed that there was a spreading-out of the illumination which most of the time seemed to have a dominant direction.

One *S* reported the phenomenon as having "shot out to the right and back;" another reported, "quite sure of the movement; see it distinctly." Others spoke of it merely as a "swelling out on one side only" or a fading of the part of the field opposite to the direction of apparent movement. A review of the *S*s' reports leaves one a little skeptical of the value of discussing the implications of a phenomenon so ephemeral. The quantitative figures, however, seem to dispel this skepticism. *S* was given an alternative of two responses, direction-of-movement or no-movement; yet, in spite of the uncertain nature of the movement-impression, the data recorded above display a relatively high frequency of reported movement, and in the anticipated direction.

Effects of practice. Here again, as in the experiments upon the threshold, there is superficial indication of a practice-effect; but it seems likely that whatever increased proficiency occurred in the ability to perceive movement was even more certainly a result of a greater familiarity with the stimulus rather than a true progressively increased proficiency in observation or response. The spreading-out of illumination may readily be interpreted as movement by one *S*, but only with great difficulty by another. Particularly do the *S*s vary in their ability to abstract from objects in perceiving movement. These differences are probably indicative of varying apperceptive systems; but once such systems are drawn upon by instructions or preliminary training, and once the *S* has set up his criteria of judgment, the proficiency of observation maintains its level.

Conclusions

Perception of movement and various attendant phenomena have been extensively investigated since the rise of the *Gestalt-Theorie*; but most of the experimentation has been carried out under that theory. These workers have made assumptions analogous to that already quoted from Koffka. Since the blind spot is an area insensitive to light, simple sensations, he believes,

cannot be produced there, and hence the movement-impressions obtained cannot be conceived as built on simple sensations. It is implied that in such an experience the blind spot alone could be the source of sensational factors upon which the movement impression might be built. Waiving all concern with the simplicity or elementariness of sensations, it is here merely contended that a sensitivity exists which might account for the impression of movement. The blind spot has not yet been proved sensitive to light; but it is immediately surrounded by retinal areas which are sensitive—areas, too, of higher spatial value than the rest of the retina, according to such investigators as Ferree and Rand,[15] Werner, [16] and Lohmann.[17] Moreover, an optical system cannot be devised which will introduce a beam of light into the eye without illumination of the refractive system.

The $\frac{1}{4}$° stimulus-area utilized throughout our investigation disappeared entirely within the blind spot; but we believe that the illumination from it was so diffused through the refracting media of the eye as to affect, probably, a considerable part of the retina. It was assumed that this stimulation would be imperceptible until an intensity was reached far above the threshold for direct stimulation, and that its perceptibility would decrease with the distance from the center of the diffusion area. Since the center of the diffusion-area would vary with the position of the stimulus-light, the sensitive retinal elements immediately surrounding the blind spot would be stimulated by a brighter portion of the diffusion area as that light approached the edge of the disk. Therefore, a lower intensity would be necessary to make the diffuse stimulation perceptible when the stimulus-light fell upon the edge of the optic disk than when it fell in the center. This was confirmed by the results obtained in the first part of the investigation.

Similarly, in the second part, the sudden increase in the intensity of the illumination caused the diffusion-area to spread out and carry supraliminal stimulation to retinal elements further from the edges of the blind region, as well as much stronger stimu-

[15]C. E. Ferree and G. Rand, The spatial values of the visual field immediately surrounding the blind spot and the question of the associative filling-in of the blind spot, *Amer. J. Physiol.*, 29, 1912, 398-417.

[16]H. Werner, Untersuchungen über den blinden Fleck, *Arch. f. d. ges. Physiol.*, 153, 1913, 475-490.

[17]W. Lohmann, Der blinde Fleck in seinen Beziehungen zu den Raumwerten der Netzhaut, *Arch f. Augenhk.*, 81, 1916, 183-196.

lation to those elements immediately surrounding the disk. So far as is known, the perception of an actually moving object is nothing more than this successive stimulation of retinal points. Movement phenomena following upon increased intensity of illumination have been observed by other investigators. Exner (1886) wrote in an early article "Hier scheint der Netzhautperipherie als Anhaltspunkt zu ihrer Bewegungsdeutung der Umstand zu dienen, dass in der That die Ringe merklich gleicher Helligkeit bei der Abnahme der Beleuchtung, nach dem Centrum rücken, bei Zunahme nach der Peripherie."[18] . L. W. Stern (1894) discovered that "Die enge Assoziation, die zwischen der veränderten Reizung und der Bewegungsvorstellung besteht, bewirkt auch, dass uns eine blosse Intensitätsveränderung eines bestimmten Objektes (ohne jede objektive räumliche Verschiebung), dennoch den Eindruck macht, 'als rühre sich etwas im Sehfeld'. . . Im seitlichen Sehen, wo diese Erscheinung besonders stark ist, mag auch noch die Irradiation mitspielen, welche bei Helligkeitzunahme die Ränder des Bildes verbreitert, und umgekehrt."[19]

Finally, some preliminary results which the writer has obtained in experimenting with the phi-phenomenon in the region of the blind spot suggests a third source of evidence confirmatory of the 'diffusion' hypothesis.

All of the phenomena reported by Stern, by Feinberg, and by the writer are faint, indefinite, and difficult to describe in any detail. Figures which show clear contours when exposed on the periphery of the retina, even when surrounding the region of the blind spot, become vague hazes when exposed within the disk. Undeniably, there is diffusion of this illumination throughout the eyeball. If this diffused light is of sufficiently high intensity in the neighborhood of the point of exposure to stimulate the sensitive parts of the retina immediately surrounding the blind spot, the phenomenon produced is certain to be diffused, faint, and without contour. Despite the suggestion of both Stern and Feinberg that explanation of the observed results of stimulation of the blind spot upon the basis of "false" lights is inadequate, and the assertion of Feinberg that the doctrine of the complete insensitivity

[18]S. Exner, Über die Functionsweise der Netzhautperipherie und den Sitz der Nachbilder, *Arch. f. Ophth.*, 32, 1886, 233-252.

[19]L. W. Stern, Die Wahrnehmung von Bewegungen vermittelst des Auges, *Zsch. f. Psychol.*, 7, 1894, 321-386.

of the physiological blind spot must undergo new investigation, there appears little in their results which is inconsistent with the hypothesis of diffusion.

Visual phenomena resulting from the diffusion of light through the refracting media of the eye have been frequently observed and reported in the history of experimentation in vision. An adequate review of the literature may be found in a recent article by Helson.[20] The hypothesis for which we have attempted to find further evidence is sufficiently established in the tradition of physiological optics to cause the burden of proof to rest upon any other assumption.

The recent investigation carried on by Helson, amasses a quantity of impressive evidence against 'diffusion' hypotheses and in favor of direct excitability of the optic disk. The investigation was entirely qualitative, and consequently but suggestive of further lines of experimentation. Helson's *Ss* report phenomena which our *Ss* did not observe; but the conditions of observation in the two investigations were probably at no time comparable.

Summary

(1) Impingement upon the optic disk of a circular light-point subtending a visual angle of $\frac{1}{4}°$ produces sensations of light.

(2) The occurrence of these sensations is dependent upon the intensity of the impinging light and the area of the disk upon which the light-point falls. The threshold for light-intensity is significantly lower when the light ray falls upon the edge of the disk than when it falls in the center.

(3) A sudden increase in the intensity of the stimulus-light falling upon the optic disk results, under certain conditions, in an impression of movement. (a) This impression of movement is more frequent when the stimulus-light falls near the edge of the disk than when it falls in the center. (b) The direction of the movement perceived corresponds to the position of the light relative to the center of the blind spot, *e.g.* movement to the right when the stimulus-light is exposed to the right of the center of the projected blind spot. (c) Movement is seen more often in the anticipated direction as the position of the light-point approaches the edges of the blind spot.

(4) There is no clear evidence of practice.

[20] H. Helson, The effects of direct stimulation of the blind-spot, this Journal, 40, 1929, 345-397.

(5) These observations seem to offer confirmatory evidence for the hypothesis that the visual phenomena which occur when the blind spot is stimulated by light are due to the affection of sensitive retinal areas as a result of the diffusion of light by the refracting media of the eye.

PHOTOMETRY

The results of the foregoing investigation have been expressed in terms of an arbitrary scale of light-units, but photometric readings were taken to make possible the conversion of these units into standard terms. The method of photometry was essentially that used by Newhall and Dodge;[11] *i.e.* comparison of the brightness of the stimulus-light with that of a standard mat surface illuminated by a standard lamp. The writer is indebted to Dr. S. M. Newhall for the use of his lamp, which was rated by the Electrical Testing Company of New York City as 10.7 candles. Fifty judgments of equality were obtained from each of four *Ss*. The results of the total 200 readings are shown in Table VI.

TABLE VI

STIMULUS INTENSITIES IN STANDARD UNITS

Scale units	Distance of standard lamp from surface Ave. (cm.)	A.D. (cm.)	Meter candles
10	58.8	1.12	30.92
50	27.2	0.63	144.59
75	23.1	0.63	200.37
100	20.6	0.39	252.36
150	17.1	0.41	366.44

Unfortunately, the candle-power of the standard lamp prevented the obtaining of values for the higher range of the arbitrary scale used; but it is clear that those secured approximate a straight line. The equation for the straight line curve which best fits the data, and upon which the higher range of the scale may be extrapolated, is $y = 2.36x + 17.4$, in which x = the arbitrary light units and y = the meter candles.

[11] S. M. Newhall and R. Dodge, Colored after-images from unperceived weak chromatic stimulation, *J. Exper. Psychol.*, 10, 1927, 1-17.

SPEARMAN'S CORRECTION FOR ATTENUATION AND ITS PROBABLE ERROR

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The purpose of this paper is to examine the assumptions underlying the various formulas proposed for estimating the true correlation between variables measured by fallible observations, to select the most useful of these formulas from the standpoints of both accuracy of estimation and simplicity of computation, to determine the probable error of the formula so selected, and to present tables to facilitate the computation of this formula and of its probable error.

Let x and y be two variables whose true correlation is r_{xy}. Let x be measured by sets of fallible observations x_1 and x_3 (using odd subscripts), and let y be measured by sets of fallible observations y_2 and y_4 (using even subscripts). Let δ_1 and δ_3 be errors of measurement in x, and let e_2 and e_4 be errors of measurement in y. Then

$$x_1 = x + \delta_1 \qquad y_2 = y + e_2$$
$$x_3 = x + \delta_3 \qquad y_4 = y + e_4.$$

Of the quantities x, y, δ_1, δ_3, e_2 and e_4, assume that x and y alone are correlated; *i.e.* that the errors of measurement are uncorrelated with one another and with the true variables. The quantities x, x_1, x_3, y, y_2, and y_4 are to be taken as deviations from their respective means; and δ_1, δ_3, e_2, and e_4 are to be assumed to take positive and negative values equally often as a matter of chance. Then

$$\left.\begin{aligned}
\Sigma\, x_1 x_3 &= \Sigma\,(x+\delta_1)(x+\delta_3) = \Sigma\, x^2\\
\Sigma\, y_2 y_4 &= \Sigma\,(y+e_2)(y+e_4) = \Sigma\, y^2\\
\Sigma\, x_1 y_2 &= \Sigma\,(x+\delta_1)(y+e_2) = \Sigma\, xy\\
\Sigma\, x_1 y_4 &= \Sigma\,(x+\delta_1)(y+e_4) = \Sigma\, xy\\
\Sigma\, x_3 y_2 &= \Sigma\,(x+\delta_3)(y+e_2) = \Sigma\, xy\\
\Sigma\, x_3 y_4 &= \Sigma\,(x+\delta_3)(y+e_4) = \Sigma\, xy
\end{aligned}\right\} \;\ldots\ldots.[1]$$

and

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$$\left.\begin{aligned}\sigma_x^2 &= \frac{\Sigma\, x^2}{N} = \frac{\Sigma\, x_1\, x_3}{N} = r_{13}\, \sigma_1\, \sigma_3 \\ \sigma_y^2 &= \frac{\Sigma\, y^2}{N} = \frac{\Sigma\, y_2\, y_4}{N} = r_{24}\, \sigma_2\, \sigma_4\end{aligned}\right\}\ldots\ldots\ldots\ldots[2]$$

$$r_{xy} = \frac{\Sigma\, xy}{\sqrt{\Sigma\, x^2\, \Sigma\, y^2}} = \frac{\Sigma\, x_1\, y_2}{\sqrt{\Sigma\, x_1 x_3\, \Sigma\, y_2 y_4}} = \frac{\Sigma\, x_1\, y_4}{\sqrt{\Sigma\, x_1 x_3\, \Sigma\, y_2 y_4}}$$

$$= \frac{\Sigma\, x_3\, y_2}{\sqrt{\Sigma\, x_1 x_3\, \Sigma\, y_2 y_4}} = \frac{\Sigma\, x_3\, y_4}{\sqrt{\Sigma\, x_1 x_3\, \Sigma\, y_2 y_4}}.$$

Each of the last four numerators varies from $\Sigma\, xy$ by chance terms only, so the most probable value of $\Sigma\, xy$ should be their arithmetic mean, and we may write

$$r_{xy} = \frac{\Sigma\, x_1 y_2 + \Sigma\, x_1 y_4 + \Sigma\, x_3 y_2 + \Sigma\, x_3 y_4}{4\sqrt{\Sigma\, x_1 x_3\, \Sigma\, y_2 y_4}}\ldots\ldots\ldots[3]$$

or from [2]

$$r_{xy} = \frac{r_{12}\, \sigma_1\, \sigma_2 + r_{14}\, \sigma_1\, \sigma_4 + r_{23}\, \sigma_2\, \sigma_3 + r_{34}\, \sigma_3\, \sigma_4}{4\sqrt{r_{13}\, \sigma_1\, \sigma_3\ \ r_{24}\, \sigma_2\, \sigma_4}}\ldots\ldots\ldots[4]$$

Formulas [3] and [4], which are equivalent, give the best estimate of r_{xy} that may be obtained from two sets of fallible measures of each variable. They involve only one basic assumption; namely, lack of correlation between errors. They are rather long, however, for purposes of computation; their probable errors are not known, and in all likelihood these probable errors would be too long for practical computation if they were known. By introducing certain further assumptions, much simpler formulas may be derived, but all such formulas involve certain systematic errors in addition to the chance errors due to limited sampling and fallible measures.

Assume that $\sigma_1 = \sigma_3$ and $\sigma_2 = \sigma_4$. (This is an assumption of strict numerical equality; not of essential equality within the sampling errors). Then $\sqrt{\sigma_1\,\sigma_3} = \sigma_1 = \sigma_3$, $\sqrt{\sigma_2\,\sigma_4} = \sigma_2 = \sigma_4$, and from [4]

$$r_{xy} = \frac{r_{12} + r_{14} + r_{23} + r_{34}}{4\sqrt{r_{13}\, r_{24}}}\ldots\ldots\ldots\ldots\ldots\ldots[5]$$

If σ_1 and σ_3 are not exactly equal, $\sqrt{\sigma_1\,\sigma_3}$ will be their geometric mean, which is closer to the smaller of the two values than to the larger; and if we substitute this geometric mean twice for the

smaller and twice for the larger value in the numerator, the latter will be spuriously reduced somewhat. The denominator already contains this geometric mean and therefore remains correct. The same thing is true if σ_2 and σ_4 are not equal, so that in general, formula [5] will tend to underestimate the value of r_{xy}. The same thing is true also if we take $r_{xy} = \frac{r_{12}}{\sqrt{r_{13}\ r_{24}}}$ or use any other single intercorrelation or any additive combination of less than four of these as the numerator, except that in such cases the chance errors will be increased.

Assume that $\Sigma\, x_1\, y_2$, $\Sigma\, x_1\, y_4$, $\Sigma\, x_3\, y_2$ and $\Sigma\, x_3\, y_4$ are numerically equal. In this case their geometric mean will equal their arithmetic mean, and from [3]

$$r_{xy} = \frac{\sqrt[4]{(\Sigma\, x_1\, y_2)\ (\Sigma\, x_1\, y_4)\ (\Sigma\, x_3\, y_2)\ (\Sigma\, x_3\, y_4)}}{\sqrt{(\Sigma\, x_1\, x_3)\ (\Sigma\, y_2\, y_4)}}$$

$$= \frac{\sqrt[4]{r_{12}\,\sigma_1\,\sigma_2\ r_{14}\,\sigma_1\,\sigma_4\ r_{23}\,\sigma_2\,\sigma_3\ r_{34}\,\sigma_3\,\sigma_4}}{\sqrt{r_{13}\,\sigma_1\,\sigma_3\ r_{24}\,\sigma_2\,\sigma_4}}.$$

Since we are dealing entirely with products, this reduces immediately to

$$r_{xy} = \frac{\sqrt[4]{r_{12}\ r_{14}\ r_{23}\ r_{34}}}{\sqrt{r_{13}\ r_{24}}} \quad \ldots\ldots\ldots\ldots\ldots\ldots [6]$$

If $\Sigma\, x_1\, y_2$, $\Sigma\, x_1\, y_4$, $\Sigma\, x_3\, y_2$, and $\Sigma\, x_3\, y_4$ are not exactly equal, their geometric mean will be less than their arithmetic mean, and the numerator of [6] will be spuriously reduced and the value of r_{xy} underestimated, the denominator remaining correct as in the case of formula (5). It should be noted that the factor causing a systematic error in the estimation of r_{xy} in the case of formula [5] is inequality of standard deviations, while in the case of formula [6] it is inequality of product-moments. In each instance the tendency is toward underestimation, while both formulas have the same denominator, which is not subject to any systematic error. The numerator of [5] is the arithmetic mean of the four intercorrelations, while that of [6] is their geometric mean. Hence [5] will always yield the less underestimated value. The formula proposed by Yule,[1] $r_{xy} = \sqrt{\frac{r_{14}\ r_{23}}{r_{13}\ r_{24}}}$, is subject to the same type of systematic error as is formula [6], and also to larger chance errors.

[1] G. U. Yule, *An Introduction to the Theory of Statistics*, 5th ed., 1919, 213-214.

The estimation of r_{xy} from formula [5] involves the computation of six correlation coefficients, and its probable error as derived by Shen[2] is lengthy and laborious to calculate. It has been shown by Kelley,[3] however, that it is not necessary to calculate r_{12}, r_{14}, r_{23}, and r_{34} if we know the value of $r_{(1+3)(2+4)}$, the correlation between the sums of the pairs of estimates of x and of the corresponding pairs of estimates of y. The proof is as follows:

$$r_{(1+3)(2+4)} = \frac{\Sigma\,(x_1 + x_3)\,(y_2 + y_4)}{\sqrt{\Sigma\,(x_1 + x_3)^2\,\Sigma\,(y_2 + y_4)^2}}$$

$$= \frac{r_{12}\,\sigma_1\,\sigma_2 + r_{14}\,\sigma_1\,\sigma_4 + r_{23}\,\sigma_2\,\sigma_3 + r_{34}\,\sigma_3\,\sigma_4}{\sqrt{(\sigma_1^2 + \sigma_3^2 + 2\,r_{13}\,\sigma_1\,\sigma_3)\,(\sigma_2^2 + \sigma_4^2 + 2\,r_{24}\,\sigma_2\,\sigma_4)}}$$

Assuming as in formula [5] that $\sigma_1 = \sigma_3$ and $\sigma_2 = \sigma_4$,

$$r_{(1+3)(2+4)} = \frac{(r_{12} + r_{14} + r_{23} + r_{34})\,\sigma_1\,\sigma_2}{\sqrt{2\,\sigma_1^2\,(1 + r_{13})\,2\,\sigma_2^2\,(1 + r_{24})}}$$

$$r_{(1+3)(2+4)} = \frac{r_{12} + r_{14} + r_{23} + r_{34}}{4\sqrt{\frac{1 + r_{13}}{2}}\sqrt{\frac{1 + r_{24}}{2}}} \quad\ldots\ldots\ldots\ldots\ldots[7]$$

and

$$\frac{r_{12} + r_{14} + r_{23} + r_{34}}{4} = r_{(1+3)(2+4)}\sqrt{\frac{1 + r_{13}}{2}}\sqrt{\frac{1 + r_{24}}{2}}.$$

Substituting in [5]

$$r_{xy} = \frac{r_{(1+3)(2+4)}}{\sqrt{\frac{2\,r_{13}}{1 + r_{13}}}\sqrt{\frac{2\,r_{24}}{1 + r_{24}}}} \quad\ldots\ldots\ldots\ldots\ldots\ldots[8]$$

This formula is implied in Kelley's derivation, and has been published in this form without formal proof by Hull.[4] It is evident that formula [8] is identical with formula [5] in its underlying assumptions and in the numerical estimates of r_{xy} which it yields. It may safely be employed wherever the difference between the two standard deviations for each variable is small compared to the absolute magnitudes of these standard deviations.

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[2] E. Shen, The standard error of certain estimated coefficients of correlation, *J. Educ. Psychol.*, 15, 1924, 462-465.
[3] T. L. Kelley, *Statistical Method*, 1923, 209-210.
[4] C. L. Hull, *Aptitude Testing*, 1928, 243, formula [5].

Before this formula can be of any considerable value, its probable error must be known, and tables must be available to facilitate the calculation of both its value and the value of its probable error.

For convenience, we will call $r_{(1+3)(2+4)} = r$. Then formula [8] becomes

$$r_{xy} = \frac{r}{\sqrt{\frac{2r_{13}}{1+r_{13}}}\sqrt{\frac{2r_{24}}{1+r_{24}}}}.$$

Taking logarithmetic differentials, squaring, summing, and dividing by the theoretical infinite population, we have

$$\begin{aligned}\frac{\sigma^2_{r_{xy}}}{r_{xy}^2} &= \frac{\sigma^2_r}{r^2} + \frac{\sigma^2_{r_{13}}}{4r^2_{13}} + \frac{\sigma^2_{r_{13}}}{4(1+r_{13})^2} + \frac{\sigma^2_{r_{24}}}{4r^2_{24}} + \frac{\sigma^2_{r_{24}}}{4(1+r_{24})^2} \\ &- \frac{r_{r_{13}r}\,\sigma_{r_{13}}\,\sigma_r}{r_{13}\,r} + \frac{r_{r_{13}r}\,\sigma_{r_{13}}\,\sigma_r}{r(1+r_{13})} - \frac{r_{r_{24}r}\,\sigma_{r_{24}}\,\sigma_r}{r_{24}r} + \frac{r_{r_{24}r}\,\sigma_{r_{24}}\,\sigma_r}{r(1+r_{24})} \\ &- \frac{\sigma^2_{r_{13}}}{2r_{13}(1+r_{13})} + \frac{r_{r_{13}r_{24}}\,\sigma_{r_{13}}\,\sigma_{r_{24}}}{2r_{13}r_{24}} - \frac{r_{r_{13}r_{24}}\,\sigma_{r_{13}}\,\sigma_{r_{24}}}{2r_{13}(1+r_{24})} \\ &- \frac{r_{r_{13}r_{24}}\,\sigma_{r_{13}}\,\sigma_{r_{24}}}{2r_{24}(1+r_{13})} + \frac{r_{r_{13}r_{24}}\,\sigma_{r_{13}}\,\sigma_{r_{24}}}{2(1+r_{13})(1+r_{24})} - \frac{\sigma^2_{r_{24}}}{2r_{24}(1+r_{24})} \quad \ldots [9]\end{aligned}$$

To evaluate this expression, we require; in addition to σ_r, $\sigma_{r_{13}}$, and $\sigma\, r_{24}$, which may be readily written from the usual formula for the standard error of a correlation coefficient; $r_{r_{13}r}$, $r_{r_{24}r}$, and $r_{r_{13}r_{24}}$. Filon and Pearson[5] give the formula for the correlation between two correlation coefficients, $r_{r_{13}r_{24}}$. Noting that $r_{r_{13}r} = r_{r_{13}r_{(1+3)(2+4)}}$, substituting subscripts and simplifying,

$$\begin{aligned}r_{r_{13}r_{(1+3)(2+4)}} &\doteq \frac{1}{(1-r^2_{13})(1-r^2_{(1+3)(2+4)})}[r_{1(2+4)}\,r_{3(1+3)} + r_{1(1+3)}\,(r_{3(2+4)} \\ &- r_{3(1+3)}\,r_{(1+3)(2+4)} - r_{13}\,r_{1(2+4)}) - r_{3(2+4)}\,(r_{1(2+4)}\,r_{(1+3)(2+4)} \\ &+ r_{13}\,r_{3(1+3)}) + \frac{r_{13}\,r_{(1+3)(2+4)}}{2}(r^2_{1(1+3)} + r^2_{1(2+4)} \\ &+ r^2_{3(1+3)} + r^2_{3(2+4)})] \ldots\ldots\ldots\ldots\ldots\ldots [10]\end{aligned}$$

To evaluate this expression we need; in addition to $r_{(1+3)(2+4)}$, which is the r computed from the data; $r_{1(2+4)}$, $r_{3(2+4)}$, $r_{1(1+3)}$, and $r_{3(1+3)}$.

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[5] K. Pearson and L. N. G. Filon, On the probable errors of frequency con, stants and on the influence of random selection on variation and correlation- *Philos. Trans.*, 191, 1898, 229-34.

$$r_{1(2+4)} = \frac{\Sigma\,[x_1\,(y_2 + y_4)]}{\sqrt{\Sigma\, x_1^2}\,\sqrt{\Sigma\,(y_2 + y_4)^2}} = \frac{\sigma_1\,\sigma_2\,r_{12} + \sigma_1\,\sigma_4\,r_{14}}{\sigma_1\,\sqrt{\sigma_2^2 + 2\,\sigma_2\,\sigma_4\,r_{24} + \sigma_4^2}}.$$

Assuming that $\sigma_1 = \sigma_3$ and $\sigma_2 = \sigma_4$,

$$r_{1(2+4)} = \frac{r_{12} + r_{14}}{\sqrt{2 + 2\,r_{24}}} \qquad [11]$$

Similarly,

$$r_{3(2+4)} = \frac{r_{23} + r_{34}}{\sqrt{2 + 2\,r_{24}}} \qquad [12]$$

From [7],

$$r_{(1+3)(2+4)} = \frac{r_{12} + r_{14} + r_{23} + r_{34}}{2\,\sqrt{1 + r_{13}}\,\sqrt{1 + r_{24}}}.$$

Assuming that $r_{12} + r_{14} = r_{23} + r_{34}$,

$$r_{(1+3)(2+4)} = \frac{r_{12} + r_{14}}{\sqrt{1 + r_{13}}\,\sqrt{1 + r_{24}}} = \frac{r_{23} + r_{34}}{\sqrt{1 + r_{13}}\,\sqrt{1 + r_{24}}},$$

and

$$r_{12} + r_{14} = r_{23} + r_{34} = r_{(1+3)(2+4)}\,\sqrt{1+r_{13}}\,\sqrt{1+r_{24}}.$$

Substituting in [11] and [12], and calling $r_{(1+3)(2+4)} = r$,

$$r_{1(2+4)} = r_{3(2+4)} = r\sqrt{\frac{1 + r_{13}}{2}} \qquad [13]$$

Note that if $r_{12} + r_{14}$ and $r_{23} + r_{24}$ are not equal, $r_{(1+3)(2+4)} \times \sqrt{1+r_{13}}\,\sqrt{1+r_{24}}$ is their arithmetic mean, so that one of the quantities, $r_{1(2+4)}$, $r_{3(2+4)}$ will be underestimated on account of this substitution to exactly the same extent as the other is overestimated.

Now,

$$r_{1(1+3)} = \frac{\Sigma[x_1(x_1 + x_3)]}{\sqrt{\Sigma x_1^2}\,\sqrt{\Sigma(x_1 + x_3)^2}} = \frac{\sigma_1^2 + r_{13}\,\sigma_1\,\sigma_3}{\sigma_1\,\sqrt{\sigma_1^2 + \sigma_3^2 + 2\sigma_1\sigma_3 r_{13}}}.$$

Assuming that $\sigma_1 = \sigma_3$,

$$r_{1(1+3)} = r_{3(1+3)} = \sqrt{\frac{1 + r_{13}}{2}} \qquad [14]$$

Substituting the values of $r_{1(1+3)}$ and $r_{1(2+4)}$ from [13] and [14] in [10], calling $r_{(1+3)(2+4)} = r$, and simplifying,

$$r_{r_{13}r} = r/2 \qquad [15]$$

and by a similar line of reasoning

$$r_{r_{24}r} = r/2 \qquad [16]$$

Note that if $r_{12} + r_{14} \neq r_{23} + r_{34}$, one of these values will be too large and the other correspondingly too small, but since $r_{r_{12}r}$ and $r_{r_{34}r}$ enter equally and additively in [9], there will be no systematic error introduced.

From Filon and Pearson's[6] formula for the correlation between two correlation coefficients, substituting subscripts and multiplying by the standard errors of the coefficients,

$$r_{r_{12}r_{34}}\,\sigma_{r_{12}}\,\sigma_{r_{34}} = \frac{2r^2}{N(2-r)^2}(1 - r_{13} - r_{24} + r_{13}r_{24}) \ldots [17]$$

Substituting the values from [15], [16], and [17] in [9] and simplifying,

$$\text{P.E.}_{r_{xy}} = \frac{.6745 r_{xy}}{\sqrt{N}}\left[\left(\frac{1-r^2}{r}\right)^2 + \left(\frac{1-r_{13}}{2r_{13}}\right)^2 + \left(\frac{1-r_{24}}{2r_{24}}\right)^2 - (1-r^2)\left(\frac{1-r_{13}}{2r_{13}}\right) - (1-r^2)\left(\frac{1-r_{24}}{2r_{24}}\right) + \left(\frac{r}{2-r}\right)^2\left(\frac{1-r_{13}}{r_{13}+r_{13}^2}\right)\left(\frac{1-r_{24}}{r_{24}-r_{24}^2}\right)\right]^{\frac{1}{2}} \ldots [18]$$

Changing the notation to conform to that usually employed in considering problems involving intercorrelations and reliability coefficients, but retaining the symbol r for the correlation between the sum of the pairs of corresponding estimates of each variable, formula [8] becomes

$$r_{\infty\omega} = r\sqrt{\frac{1+r_{1\,\mathrm{I}}}{2r_{1\,\mathrm{I}}}}\sqrt{\frac{1+r_{2\,\mathrm{II}}}{2r_{2\,\mathrm{II}}}} \ldots [19]$$

and formula [18] becomes

$$\text{P.E.}_{r_{\infty\omega}} = \frac{.6745\, r_{\infty\omega}}{\sqrt{N}}\left[\left(\frac{1-r^2}{r}\right)^2 + \left(\frac{1-r_{1\,\mathrm{I}}}{2r_1}\right)^2 + \left(\frac{1-r_{2\,\mathrm{II}}}{2r_{2\,\mathrm{II}}}\right)^2 - (1-r^2)\left(\frac{1-r_{1\,\mathrm{I}}}{2r_{1\,\mathrm{I}}}\right) - (1-r^2)\left(\frac{1-r_{2\,\mathrm{II}}}{2r_{2\,\mathrm{II}}}\right) + \left(\frac{r}{2-r}\right)^2\left(\frac{1-r_{1\,\mathrm{I}}}{r_{1\,\mathrm{I}}+r_{1\,\mathrm{I}}^2}\right)\left(\frac{1-r_{2\,\mathrm{II}}}{r_{2\,\mathrm{II}}+r_{2\,\mathrm{II}}^2}\right)\right]^{\frac{1}{2}} \ldots [20]$$

It should be noted that each radical factor in [19] and each expression in parentheses in [20] is a function of a single correlation coefficient. In the accompanying table these functions are given

[6] *Op. cit.*

TABLES FOR THE CALCULATION OF P.E.$_{r_{\infty\omega}}$

r	A	B	C	D	E	F	G
.00	∞	∞	∞	1.000	∞	.0000	∞
.01	7.106	9998.	2450.	.9999	49.50	.0000	98.02
.02	5.050	2498.	600.3	.9996	24.50	.0001	48.04
.03	4.655	1109.	261.5	.9991	16.17	.0002	31.39
.04	3.606	623.0	144.0	.9984	12.00	.0004	23.08
.05	3.240	398.0	102.5	.9975	9.500	.0007	18.10
.06	2.972	275.8	61.36	.9964	7.833	.0010	14.78
.07	2.763	202.1	42.81	.9951	6.543	.0013	12.42
.08	2.598	154.3	33.06	.9936	5.750	.0017	10.65
.09	2.461	121.5	25.56	.9919	5.056	.0022	9.275
.10	2.345	98.01	20.25	.9900	4.500	.0028	8.182
.11	2.246	80.66	16.36	.9879	4.045	.0034	7.289
.12	2.160	67.46	13.45	.9856	3.667	.0041	6.548
.13	2.085	57.19	11.20	.9831	3.346	.0048	5.922
.14	2.018	49.04	9.431	.9804	3.071	.0057	5.389
.15	1.958	42.47	8.026	.9775	2.833	.0066	4.928
.16	1.904	37.09	6.891	.9744	2.625	.0076	4.526
.17	1.855	32.63	5.958	.9711	2.441	.0086	4.173
.18	1.810	28.90	5.189	.9676	2.278	.0099	3.861
.19	1.770	25.74	4.545	.9639	2.132	.0110	3.582
.20	1.732	23.04	4.000	.9600	2.000	.0123	3.333
.21	1.697	20.72	3.538	.9559	1.881	.0138	3.109
.22	1.665	18.71	3.144	.9516	1.773	.0153	2.906
.23	1.635	16.96	2.802	.9471	1.674	.0169	2.722
.24	1.607	15.42	2.289	.9424	1.513	.0186	2.544
.25	1.581	14.06	2.250	.9375	1.500	.0204	2.400
.26	1.557	12.86	2.025	.9324	1.423	.0223	2.259
.27	1.534	11.79	1.828	.9271	1.352	.0243	2.129
.28	1.512	10.83	1.654	.9216	1.286	.0265	2.009
.29	1.491	9.975	1.498	.9159	1.224	.0288	1.898
.30	1.472	9.201	1.362	.9100	1.167	.0311	1.795
.31	1.454	8.502	1.239	.9039	1.113	.0336	1.699
.32	1.436	7.868	1.130	.8976	1.063	.0363	1.610
.33	1.420	7.292	1.071	.8911	1.015	.0390	1.526
.34	1.404	6.766	.9421	.8844	.9706	.0419	1.449
.35	1.389	6.286	.8623	.8775	.9286	.0450	1.375
.36	1.374	5.846	.7901	.8704	.8889	.0482	1.307
.37	1.361	5.442	.7249	.8631	.8514	.0515	1.243
.38	1.348	5.070	.6655	.8556	.8158	.0550	1.183
.39	1.335	4.727	.6117	.8479	.7821	.0587	1.125
.40	1.323	4.410	.5625	.8400	.7500	.0625	1.071
.41	1.311	4.117	.5177	.8319	.7195	.0665	1.021
.42	1.300	3.845	.4768	.8236	.6905	.0707	.9725
.43	1.289	3.593	.4459	.8151	.6628	.0750	.9273
.44	1.279	3.359	.4050	.8064	.6364	.0795	.8840
.45	1.269	3.141	.3734	.7975	.6111	.0843	.8428
.46	1.260	2.937	.3446	.7884	.5870	.0892	.8041
.47	1.251	2.749	.3179	.7791	.5638	.0944	.7674
.48	1.242	2.571	.2934	.7696	.5417	.0997	.7318
.49	1.233	2.405	.2708	.7599	.5204	.1053	.6986

r	A	B	C	D	E	F	G
.50	1.225	2.250	.2500	.7500	.5000	.1111	.6667
.51	1.217	2.105	.2308	.7399	.4804	.1171	.6365
.52	1.209	1.970	.2130	.7296	.4615	.1235	.6072
.53	1.201	1.841	.1966	.7191	.4434	.1300	.5797
.54	1.194	1.721	.1814	.7084	.4259	.1368	.5533
.55	1.187	1.608	.1674	.6975	.4091	.1439	.5278
.56	1.180	1.502	.1544	.6864	.3929	.1512	.5038
.57	1.173	1.403	.1423	.6751	.3772	.1589	.4802
.58	1.167	1.309	.1311	.6636	.3621	.1668	.4582
.59	1.161	1.221	.1208	.6519	.3475	.1751	.4371
.60	1.155	1.138	.1111	.6400	.3333	.1837	.4169
.61	1.149	1.060	.1022	.6279	.3197	.1926	.3969
.62	1.143	.9859	.0939	.6156	.3065	.2018	.3784
.63	1.137	.9164	.0863	.6031	.2937	.2115	.3601
.64	1.132	.8510	.0791	.5904	.2813	.2215	.3433
.65	1.127	.7894	.0725	.5775	.2692	.2318	.3261
.66	1.121	.7313	.0664	.5644	.2576	.2426	.3102
.67	1.116	.6766	.0607	.5511	.2463	.2538	.2946
.68	1.111	.6250	.0554	.5376	.2353	.2654	.2803
.69	1.107	.5765	.0504	.5239	.2246	.2775	.2672
.70	1.102	.5308	.0459	.5100	.2143	.2899	.2523
.71	1.097	.4878	.0417	.4959	.2042	.3029	.2387
.72	1.093	.4474	.0378	.4816	.1944	.3164	.2262
.73	1.089	.4094	.0342	.4671	.1849	.3304	.2139
.74	1.084	.3738	.0309	.4524	.1757	.3450	.2017
.75	1.080	.3403	.0278	.4375	.1667	.3600	.1903
.76	1.076	.3089	.0249	.4224	.1579	.3757	.1796
.77	1.072	.2790	.0223	.4071	.1494	.3919	.1689
.78	1.068	.2521	.0199	.3916	.1410	.4088	.1584
.79	1.064	.2264	.0177	.3759	.1329	.4262	.1486
.80	1.061	.2025	.0156	.3600	.1250	.4444	.1389
.81	1.057	.1803	.0138	.3439	.1173	.4633	.1298
.82	1.053	.1596	.0121	.3276	.1098	.4830	.1209
.83	1.050	.1405	.0105	.3111	.1024	.5032	.1120
.84	1.047	.1228	.0091	.2944	.0952	.5244	.1033
.85	1.043	.1066	.0078	.2775	.0882	.5464	.0951
.86	1.040	.0917	.0066	.2604	.0814	.5690	.0876
.87	1.037	.0781	.0056	.2431	.0747	.5928	.0797
.88	1.034	.0657	.0047	.2256	.0682	.6174	.0723
.89	1.030	.0546	.0038	.2079	.0618	.6429	.0656
.90	1.027	.0446	.0031	.1900	.0556	.6694	.0584
.91	1.024	.0357	.0025	.1719	.0495	.6969	.0518
.92	1.022	.0278	.0019	.1536	.0435	.7256	.0435
.93	1.019	.0211	.0014	.1351	.0376	.7554	.0389
.94	1.016	.0153	.0010	.1164	.0319	.7864	.0330
.95	1.013	.0105	.0007	.0975	.0263	.8186	.0272
.96	1.010	.0067	.0004	.0784	.0208	.8520	.0214
.97	1.008	.0037	.0002	.0591	.0155	.8869	.0157
.98	1.006	.0016	.0001	.0396	.0102	.9223	.0101
.99	1.003	.0004	.0000	.0199	.0051	.9607	.0050

to four significant figures for values of the corresponding correlations from 0.00 to 0.99. Representing these functions by successive capital letters with appropriate subscripts,

$$r_{\infty\omega} = rA_{1\,I}\,A_{2\,II} \ldots\ldots\ldots\ldots\ldots\ldots [21]$$

$$\text{P.E.}_{r_{\infty\omega}} = \frac{.6745\, r_{\infty\omega}}{\sqrt{N}}\left(B + C_{1I} + C_{2II} - DE_{1I} - DE_{2II} + FG_{1I}G_{2II}\right)^{\frac{1}{2}} [22]$$

In [21] and [22], the letters represent the corresponding columns in the table. Where a letter appears without subscripts, the table is entered with r; where it appears with the subscript 1I, with $r_{1\,I}$; and where it appears with the subscript 2II, with $r_{2\,II}$.

Shen[7] gives an example comparing the standard error of formula [5] as given by his formula with that of formula [6] as given by Kelley.[8] He assumes $r_{12} = 0.40$, $r_{1\,II} = 0.50$, $r_{I\,2} = 0.50$, $r_{I\,II} = 0.60$, $r_{1\,I} = 0.50$, $r_{2\,II} = 0.50$, so that $r_{\infty\omega} = 1.00$ (by formula 5). He finds $\sigma_{r_{\infty\omega}} = \sqrt{.7238/N}$ by his formula, and $\sqrt{.7500/N}$ by Kelley's. The writers computed $r_{(1+I)(2+II)}$ from his data by formula [7], finding the value 0.6667. They then computed the standard error of $r_{\infty\omega}$ by formula [22] (omitting the .6745), and found it to be $\sqrt{.7504/N}$. They then computed $r_{\infty\omega}$ by formula [6], and found its value to be 0.9898, and its standard error by Kelley's formula (which was derived for this case) to be $\sqrt{.7200/N}$.

From unpublished data, the writers computed the following correlations: $r_{12} = 0.48$, $r_{1\,II} = 0.52$, $r_{I\,2} = 0.51$, $r_{I\,II} = 0.57$, $r_{1\,I} = 0.59$, $r_{2\,II} = 0.61$, $r_{(1+I)(2+II)} = 0.65$, $(r_{12} + r_{1\,II} + r_{I\,2} + r_{I\,II})/4 = 0.52$, $N = 79$, $r_{\infty\omega} = 0.867$ (by formula [21], checking by formula [5]. They then computed $\text{P.E.}_{r_{\infty\omega}}$, finding its value by Shen's formula to be 0.0521; by Kelley's formula, 0.0540; and by formula [22], 0.0538. The value of $r_{\infty\omega}$ was then recalculated by formula [6], and its value by this formula was found to be 0.865 with a probable error (by Kelley's formula) of 0.0538. The computation of the above probable errors consumed 6 minutes by formula [22], 16 minutes by Kelley's formula, and 63 minutes by Shen's formula. The writers had never previously computed a probable error by any of these formulas, but were undoubtedly more familiar with formula [22] than with the other two.

In handling a problem by formulas [21] and [22], the following analysis may be of value:

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[7] *Op. cit.*

[8] *Op. cit.*, 210, formula [161 d].

(1) Compute $r_{1\,\mathrm{I}}$ and $r_{2\,\mathrm{II}}$ from the data. In case only two sets of observations are available, these may be $r_{\substack{1\,\mathrm{I}\\ \bar{2}\,\overline{\mathrm{II}}}}$ and $r_{\substack{2\,\mathrm{II}\\ \bar{2}\,\overline{\mathrm{II}}}}$. Compute $r_{(1+\mathrm{I})(2+\mathrm{II})}$ from the data. This may reduce to r_{12}. Call $r_{(1+\mathrm{I})(2+\mathrm{II})}$ (or r_{12}) $= r$.

(2) In the r-column of the table find the value of $r_{1\,\mathrm{I}}$, and in the A-column opposite, read $A_{1\,\mathrm{I}}$. Then find the value of $r_{2\,\mathrm{II}}$ in the r-column, and in the A-column opposite read $A_{2\,\mathrm{II}}$. Multiply $r \cdot A_{1\,\mathrm{I}} \cdot A_{2\,\mathrm{II}}$ (formula [21], to obtain $r_{\infty\omega}$.

(3). Calculate $\frac{.6745\ r_{\infty\omega}}{\sqrt{N}}$.

(4). In the r-column find the value of r. In the B-column opposite, read B; in the D-column, D; and in the F-column, F. Then find the value of $r_{1\,\mathrm{I}}$ in the r-column, and read $C_{1\,\mathrm{I}}$, $E_{1\,\mathrm{I}}$ and $G_{1\,\mathrm{I}}$ in the appropriate columns. Find the value of $r_{2\,\mathrm{II}}$ in the r-column, and look up $C_{2\,\mathrm{II}}$, $E_{2\,\mathrm{II}}$ and $G_{2\,\mathrm{II}}$ in the appropriate columns.

(5). Substitute in formula [22] to get $\text{P.E.}_{r_{\infty\omega}}$.

Summary

(1) No formula involving correlation coefficients alone gives an unbiased estimate of the correlation corrected for attentuation.

(2) Formulas involving the arithmetic mean of the observed intercorrelations underestimate the value of $r_{\infty\omega}$ less than those involving the geometric mean. If the intercorrelations are not widely different, this effect is slight.

(3) A formula implied in a derivation by Kelley, and proposed by Hull, has been suggested as the best for practical computation. This formula involves the computation of only three correlation coefficients from the data instead of the six required by other formulas.

(4) The probable error of this formula has been derived.

(5) Tables have been prepared to facilitate the computation of $r_{\infty\omega}$ and its probable error by this formula. The use of these tables has been shown to save considerable time on the part of the computer.

THE PERCEPTION OF FORM IN PERIPHERAL VISION

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Numerous investigations have been undertaken for the purpose of setting forth the nature and conditions of perception in the field of direct vision, but there are only occasional studies which in any way deal with the problem of the general character of perception in indirect vision. If we set aside those investigations which have to do with the character of color sensitivity and the perception of movement in peripheral vision, there remain only a few studies which deal with other and more general aspects of perception in the marginal field of vision.

Purkinje was the first to record an interest in the nature of peripheral vision.[1] In order to demonstrate the importance of marginal vision in the successful execution of certain everyday perceptual reactions, he restricted the visual field, practically eliminating peripheral vision, by wearing glasses with opaque lenses in the center of each of which there was a small hole. Under these conditions he discovered that he was unable satisfactorily to find his way about in his own room, and that in such activities as dancing, where prompt reactions must be made, faulty adjustments frequently occurred. Poor adjustments were also found to be characteristic when the visual field was restricted by wearing a mask.

Casual observation affords convincing evidence of the considerable extent to which indirect vision is employed in everyday reactions of varying degrees of complexity. In such situations as handshaking, serving a tennis ball, fielding a batted ball, passing a basket or hockey ball, operating a motor car (particularly in approaching an intersection filled with moving vehicles), walking across a busy thoroughfare, assorting mail, etc., peripheral vision plays an important rôle in effecting appropriate reactions. It is true that, if time permits, the several items in any one of these complex situations may be brought in turn into the field of direct vision by the act of turning the head and eyes. However, at the moment when the reaction is actually made the several items cannot fall simultaneously in the line of regard. In many situations the necessity of prompt action tends to prevent the possibility of extensive head and eye movements. Moreover, owing to the influence of habituation, the tendency to execute such movements is inhibited in familar situations; and, as a consequence, peripheral vision is called into operation in places where originally central vision was used.

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[1] J. Purkinje, *Beytrag zur Physiologie der Sinne*, 1825, II, 28.

Although these everyday situations and considerations serve to show that peripheral vision functions as an auxiliary to central vision in effecting adjustments to complex situations, and perhaps to suggest certain of the points of difference between marginal and central vision, yet, the more precise description of the nature of perception in indirect vision must be provided by experiments which are limited to specific items of perception. Studies of color sensitivity and of discrimination of movement in the field of indirect vision have been made in sufficient numbers adequately to indicate the general facts in regard to these points. Studies in regard to discrimination of form in peripheral vision have been made by several investigators, but in every case the quantitative determination of that point, or the number of degrees from the fixation-point, at which the true form of the stimulus can be perceived seems to have been the sole object in which these investigators were interested.

Aubert employed the method of moving a form slowly inward over the visual field, while the eye was directed upon a fixation-point, until the form was accurately perceived.[2] He used one form only, a black square upon a white background. This was presented in different sizes at such distances that the squares all subtended the same visual angle. The only significant result discovered was that the field of perception, in spite of constant size of stimulus at the eye and of constant illumination, is greater at the nearer than at the more remote distances. Freeman has recently summarized the literature upon this anomaly in peripheral vision;[3] however, this anomaly is irrelevant to the point of our study.

Kleitman and Blier mapped the visual fields of form discrimination for four figures—circle, square, triangle and five pointed star.[4] The figures were white upon a black background and were presented on a perimeter on which they were moved slowly inward until the subject could name them. Each figure was presented in five sizes. The diameters of the circles were 3, 5, 10, 15 and 20 mm. Each of the other figures was inscribed in an imaginary circle whose diameter in all cases was 1 mm. larger than that of the corresponding circle figure. This was done in order to equalize roughly the surface area of the different figures of a given magnitude. The limits of perceptibility of the four figures of a given size were found to be practically identical, "crossing and recrossing many times, in the records of the two subjects in the experiment." The average extension for the figures of a given size was roughly the same. Visual acuity falls off more rapidly in certain meridians than in others. The extension was greater horizontally than vertically, and the temporal field was larger than the nasal.

At the meeting of the American Psychological Association in Ithaca (1925), Geissler reported upon his experiments of form perception in indirect vision.[5] He used five white forms—square, triangle, diamond, sector, and circle—which

[2]H. Aubert, *Physiologie der Netzhaut*, 1865, 240.

[3]E. Freeman, Anomalies of peripheral visual acuity, *J. Exper. Psychol.*, 12, 1929, 324-340.

[4]N. Kleitman and Z. A. Blier, Color and form discrimination in the periphery of the retina, *Amer. J. Physiol.*, 85, 1928, 178-190.

[5]L. R. Geissler, Form perception in indirect vision, *Psychol. Bull.*, 23, 1926, 135-6.

had areal surfaces of about 80 sq. mm. each and which were presented on a black cardboard background. The *Os* looked through an observation tube at the fixation-point and the forms were exposed in different trials at various distances from the fixation-point on the meridian of the perimeter for a period of two or three seconds. The *O* was required to name and describe briefly what he observed. The results of the 4 *Os* show "a gradual but not uniform increase in percent of wrong judgments toward the periphery, with few inversions." The circle was most frequently correctly perceived, and the sector least frequently. Fewer errors in judgment occur in the horizontal than in the vertical meridian.

While these studies have settled satisfactorily to their authors the points which they had in mind to determine, they are for several reasons inadequate, singly and in the aggregate, to give a satisfactory account of the nature of form perception in the peripheral field of vision. In the first place, the number of forms used in every one of these studies is very limited. No more than five different forms were used in any of these investigations. In the second place, the forms used were not sufficiently diversified in nature. They belong essentially to the class of familar forms, such as letters and geometrical figures. In the third place, too few *Os* were employed. Finally, the primary aim of every one of these studies seems from the records to have been the determination of the outer limit of the visual field at which the figure can be accurately named. Writers, without making an experimental determination in regard to the facts in the case, generally manifest the tendency to regard perceptions at all points of indirect vision as necessarily having an equally incomplete and unclear character. Even a very recent writer labels all of these perceptions "confused,"[6] implying that in all parts of the peripheral field the same general character probably exists.

Two methods have been used in investigations on this problem. Aubert, and Kleitman and Blier used the *moving method*. Here the stimulus is moved in slowly from extreme peripheral vision to the center of the visual field. Geissler used the *stationary method*, by which a form was momentarily illuminated at different positions in the indirect field. Aubert and Förster used the latter method much earlier.[7] They presented cards, each of which bore a letter or number, in different cards at different distances from the point of fixation, and thus determined the distance from the line of regard at which letters and numbers can be perceived.

The Experiments

The principal object of the present study is to determine whether qualitative differences in the perception of form can be found at different regions in the peripheral field of vision. For this purpose we adopted the *moving method*, since it allows the possibility of noting changes in the perceptual datum from the

[6] W. Köhler, *Gestalt Psychology*, 1929, 242.

[7] H. Aubert and R. Förster, Beiträge zur Kenntnis des indirekten Sehens, *Arch. f. Ophthal.*, III, 1857, 1.

extreme point in peripheral vision where anything can be seen to that at which the form is finally clearly preceived.

Forty-eight different figures appeared in the original set of stimuli. They may be classified in two ways. First, in regard to *pattern*, there were (1) outline, (2) solid or filled, (3) partially filled and (4) incompletely outlined figures. Second, as regards *familiarity and classification*, there were (1) concrete objects, such as fish, camel, moon; (2) certain Arabic numerical digits; (3) capital letters; (4) geometrical forms, such as oval, diamond, triangle, hexagon; (5) mutilated or altered forms, such as the capital J with the hook turned in the wrong direction, a figure as a whole disoriented 180 degrees, the letter N with the diagonal connecting the terminals usually free, an incomplete triangle; and (6) certain of the nonsense figures given in Kline's *Psychology by Experiment* (pp. 216 f.). The members of these six classes were drawn, one-half in white upon a black background and one-half in black upon a white background, an equal number of each in every one of the varieties of pattern indicated above. There were a dozen members in each of the pattern classes, and eight in each of the other classes. The different figures used were too variegated to permit us to equate the areal size of all forms. We arbitrarily decided to make the main dimension of all forms 15 mm. These were drawn on backgrounds of white or black, disks of 30 mm. diam., and presented on a Meyrowitz perimeter. Movement in all cases was at the rate of about one degree to the second, and in all trials in the direction from extreme peripheral to central vision. The distance from the *O*'s eye to the stimulus, as well as to the fixation-point, in our perimeter is 26 cm. The observations were made in a room equipped with a skylight. The field of vision of the right eye of all *Os* was explored in the four principal meridians, temporal, nasal, superior, and inferior.

After the preliminary period of training, during which the *Os* acquired the ability to inhibit to a satisfactory extent the tendency to eye shifting, the regular series of stimuli was presented. When this series had been presented twice, we realized that time would not permit us to present every one of these stimuli in a number of repetitions sufficient to warrant quantitative calculation of the regions of change in the perceptual phenomena. Accordingly, we reduced our series by selecting two figures from each of the following stimulus classes: (1) concrete (pig, crescent), (2) geometrical (hexagon, oblong), (3) capital letters (M, R), (4) mutilated or altered, (triangle with open apex, capital P with curved part disoriented 180°), and (5) nonsense ("Madrid" and "Sicily" from Kline's *Psychology by Experiment*, p. 217). This series was presented ten times to every *O*.

Eleven *Os* served in these experiments. Eight were undergraduates majoring in psychology, and the other three were members of the staff. Excepting the senior author of this study, the *Os* were not aware of the object of the investigation except as it may have been suggested by the general setting of the experiment. Two *Os* were slightly nearsighted. The other *Os* had normal vision.

It should be pointed out that the conditions under which these observations are made are somewhat novel and difficult.

Ordinarily an object, in catching the attention in the peripheral field of vision, is promptly shifted to the center of vision for clear observation. In our experiment the *O* was required to inhibit the strongly ingrained tendency to shift the object to the center of vision and under these conditions to describe in detail its appearance at all positions at which it manifested changes. To give attention to an object at any point in indirect vision without bringing it into central vision is a rather artificial and unsatisfactory condition of observation. The three *Es*—the three students whose names appear under the title of this paper—took such a position as would permit them to note eye-shifting in their *Os* and constantly reminded them of such shifts until eye control was acquired. The *Os* found, as Aubert long ago reported, that control of eye-movements may be acquired under these experimental conditions. In this mode of observation, the attention is not focalized or directed to the center of the visual field but is directed outward from the point of fixation. To distinguish it from the more normal mode of focalized attention, we may designate this mode as marginal attention. In still another respect observation in our experiments was artificial in that it was at all times exclusively monocular.

Results

As soon as the *Os* acquired the ability to inhibit eye-movements, it became apparent that there are in the peripheral field of vision several regions in each of which a figure manifests a different mode of appearance. It was also evident that, for a given *O*, all kinds of stimuli tend to make the transition from one of these regions to the next adjacent at roughly the same position. Kleitman and Blier also found that the different figures which they used became perceptible at approximately the same point. In view of this finding we have decided to simplify our tabular presentation by throwing together the results of the ten members of our abbreviated series, instead of making separate calculations for every figure. There is so much overlapping and relatively so little scatter as to render this procedure justifiable. The quantitative treatment of results includes the work with the abbreviated series only, but the qualitative descriptions cover the experiments with the original series as well.

Four different modes of appearance may be distinguished as a figure is moved in over the visual field in any meridian. In order

of appearance, as the stimulus-figure is moved inward, we have labelled them (1) none, *i.e.* the field was figureless, (2) formless figure, (3) form-like figure and (4) clear figure. Table I shows the average point (in degrees) at which the ten figures enter these zones for the different *Os*. The averages appear in the upper row of figures and the mean variation in each case is placed immediately below the average to which it belongs. The combined average of all *Os* is given in the last column of figures.

A study of the table shows that all of these regions extend farther in the temporal quarter than in any of the others, and, aside from a slight exception in the limit of the clear-figure region, that they are most restricted in the superior field. The nasal and inferior fields have approximately the same extents, although the former tend to be slightly greater in most cases. It may be pointed out that, in regard to color sensitivity, roughly the same variations in asymmetrical distribution of color zones have been found. However, in a comparison of our results with those of color sensation, a notable difference occurs in the fact that all kinds of form tend in general to manifest the same change at roughly the same points.

The table also indicates that the clear-figure field is restricted more for certain *Os* than for others. With fair consistency, in the case of these *Os*, the other regions also tend to be more restricted. While we have made no experiments for the purpose of throwing light directly upon such a practical problem, we none the less suggest that these *Os* with restricted peripheral fields may work under sensory handicap in complex acts, such as operating the motor car in heavy traffic, where a number of scattered visual items must be brought into the field of clear vision within very brief temporal compass.

The qualitative characterizations of the modes of appearance in each of the four regions are as follows.

(*1*) *Figureless field.* When the stimulus pattern first becomes perceptible, the background is perceived without any figure upon it. The background lacks outline and definition and is described as a vague, blurry, hazy or cloudy, shapeless field or spot. Occasionally it is described as lacking surface character, devoid of detail and as having an apparent softness. Several *Os* reported that, as compared with its appearance farther in, the brightness of the spot is toned down somewhat, *i.e.* it is less bright or less dark than in more central regions. This variation in brightness takes place so gradually that the change is not perceptible, but the *O* recalls that a few moments ago the stimulus looked less bright or dark.

TABLE I

SHOWING FOR EVERY O AND FOR THE FOUR PRINCIPAL FIELDS OF VISION THE AVERAGE POINT AND M.V. (IN DEGREES), AT WHICH THE STIMULUS-FIGURES WERE PERCEIVED IN THE FOUR DIFFERENT MODES OF APPEARANCE

Field	Figure	Observer											General Average
		A	B	C	D	E	F	G	H	I	J	K	
Temporal	None	83.6	81.1	86.4	87.3	78.9	88.4	80.6	88.0	70.9	74.1	68.4	80.7
		2.2	1.2	1.1	0.9	1.2	1.1	0.9	1.7	0.9	1.0	0.8	
	Formless	65.0	63.7	68.2	70.5	55.4	72.9	68.8	69.0	58.4	62.0	54.2	64.3
		1.2	0.9	1.6	1.5	0.9	0.7	1.1	1.2	0.7	1.5	0.8	
	Formlike	48.3	45.5	52.1	51.2	42.6	50.4	46.3	48.6	35.6	36.4	36.1	44.8
		0.8	1.1	1.0	0.7	0.9	0.6	0.8	0.7	0.5	1.5	0.9	
	Clear	10.2	9.3	11.2	14.5	8.5	14.2	9.7	10.3	7.4	8.2	6.8	10
		1.4	1.6	0.9	1.6	0.7	1.2	0.1	1.0	1.5	1.2	0.8	
Nasal	None	64.8	60.4	70.3	58.4	62.1	70.2	64.4	72.0	63.7	68.0	55.9	64.5
		1.3	0.3	1.0	0.7	1.3	0.8	1.9	1.4	1.1	1.3	1.3	
	Formless	38.2	39.5	45.2	48.1	35.8	49.2	42.1	45.3	35.2	39.6	36.5	41.3
		1.4	0.7	1.0	0.7	0.8	0.8	1.0	0.8	0.6	0.8	1.2	
	Formlike	29.2	28.9	31.2	33.2	25.3	26.4	28.0	30.3	26.4	23.3	24.9	27.9
		1.0	1.1	0.9	1.0	1.6	1.1	0.8	1.0	0.8	0.5	0.8	
	Clear	9.4	11.3	12.4	10.2	6.4	12.6	8.4	10.3	9.0	7.8	6.4	9.4
		1.8	1.0	1.5	1.8	0.5	0.8	0.7	1.0	1.1	0.5	0.8	

TABLE I (Continued)

SHOWING FOR EVERY *O* AND FOR THE FOUR PRINCIPAL FIELDS OF VISION THE AVERAGE POINT AND M.V. (IN DEGREES) AT WHICH THE STIMULUS-FIGURES WERE PERCEIVED IN THE FOUR DIFFERENT MODES OF APPEARANCE

Field	Figure	Observer											General Average
		A	B	C	D	E	F	G	H	I	J	K	
Superior	None	45.7	46.1	49.4	55.6	45.9	54.6	50.6	60.2	44.7	44.8	45.0	49.3
		1.3	2.2	0.9	2.4	1.3	1.9	1.2	0.6	1.2	1.2	1.3	
	Formless	32.4	31.8	36.2	41.1	29.7	42.0	34.9	29.3	28.4	29.6	30.2	34.1
		1.3	1.3	0.8	1.0	1.4	1.6	0.9	1.6	1.0	1.2	0.6	
	Formlike	25.2	24.3	27.2	28.8	20.1	27.6	25.2	28.4	20.1	21.4	20.2	24.4
		1.1	1.4	1.2	0.8	1.4	0.9	1.0	1.1	0.9	1.1	0.7	
	Clear	8.4	9.6	9.2	10.2	7.5	11.6	8.3	7.6	7.6	9.1	8.1	8.8
		0.9	1.1	0.6	0.8	0.9	0.9	0.6	0.5	0.9	0.5	0.7	
Inferior	None	61.2	62.4	68.4	59.2	67.2	64.0	71.4	58.4	69.1	62.2	63.2	64.2
		0.8	1.1	1.1	1.4	0.8	1.2	0.9	3.0	0.7	1.0	1.4	
	Formless	34.2	37.1	39.1	33.6	35.0	34.6	38.2	30.8	36.0	32.1	30.1	34.6
		1.0	0.7	0.8	1.8	1.2	1.0	0.9	1.1	1.2	1.1	0.7	
	Formlike	27.6	27.2	28.1	25.4	29.0	24.4	30.1	24.6	23.6	22.2	23.2	25.9
		1.3	0.8	1.2	1.4	1.0	1.5	0.5	1.3	0.9	1.0	1.7	
	Clear	6.9	7.2	8.6	9.3	11.2	7.4	8.4	10.6	8.8	6.4	6.1	8.2
		0.7	1.0	0.8	0.6	1.2	0.7	1.0	0.9	0.8	0.8	0.9	

(*2*) *Formless figure.* This region begins at the point at which the *O* first reports that he notices anything upon the background. In the outermost part of this region, most of the *Os* report fairly regularly a faint spot, which even in outline figures is seen as filled in, and which seems to be smaller than the actual figure which appears when the stimulus reaches a more central point. The probable explanation of the apparent reduction in the size of the spot is to be found in the fact that a vaguely perceived object is normally at a great distance and therefore subtends a smaller angle upon the retina than the angle normally subtended by that object when clearly perceived. The fact that in this region outline figures appear filled in may be explained if we suppose that the stimulated retinal elements are not supplied by enough different optic fibers to provide differentiation within the perceptual pattern. Cajal found that in the central part of the retina almost every cone and rod is supplied by a separate optic fiber, whereas in the outlying regions of the retina a single optic fiber supplies a number of retinal terminals.[5] It is probable that in the formless field of perception the stimulus fails to excite enough elements belonging to an adequate number of optic fibers to give differentiation in the visual pattern.

Farther inward but still in this formless area, the faint spot or splotch breaks up into lines which are characterized as a jumble of lines or an unorganized heap of marks. There is now an initial tendency to differentiation within the figure or pattern. However, the lines are still so unclear that they fail to give a suggestion of formal relationship. The figure is described as novel and unclassified, and there is no tendency to name it. The only indication that the figure at this stage bears likeness to the figure which, at more central points, this stimulus evokes consists in the fact that frequently the *O* reports the major dimension of the figure. This dimension has acquired a higher degree of salience than the other lines of the formless mass and its direction is often accurately reported. The other lines, however, are still so intermingled that the figure is perceived as a disorganized figure. We have in this stage, at least, the incipient tendency to correspondence of object and precept.

(*3*) *Form-like figure.* Throughout this region the figure is acquiring organization, although it is not yet perceived clearly enough so that it is accurately named and characterized. The region begins at the point at which any suggestion in regard to shape is first made. Two subdivisions may be distinguished in this region, although they are not in every case clearly distinct. In the outer one, the jumble of lines of the adjacent outer region rather suddenly becomes organized into a figure, which is essentially unclassified. The *Os* experience difficulty in characterizing it, except in regard to the most general items of shape. They say, for example, it is straight-sided, or round, or it has a body with elongated processes or tentacles. A scallop-shell figure is seen as round; the letters M and H as straight-sided. Many details of a figure are unnoticed or filled in. If a label is applied, it is used with uncertainty and qualification.

The second subdivision of this region begins when the figure acquires organization sufficient to suggest one or more tentative labels. It is still not seen clearly enough to be accurately described and named. Certain details

[5] R. y Cajal in L. Luciani's *Human Physiology*, 1917, IV, 335-6.

which are unnoticed in the outer region may now be reported, but the descriptions are still liable to error. Salient features, such as angles, parallel sides, etc. serve as cues to suggest one or more tentative classes. In certain *Os*, there is a tendency to mention a number of names, all of which correspond roughly with the salient features of the figure, and to wait expectantly to apply the most fitting label when the figure has acquired greater clearness. Other *Os* manifest the tendency to label the figure by the first suggested name, which often is incorrect, and to adhere to this until it becomes clear enough to confirm or reject it. Owing to the fact that the first suggested class corresponds roughly with the salient features of the figure and that memory overlay plays an important rôle in the perceptions of these *Os*, the first suggested label is usually accepted until the figure has reached the outer limit of the region of clear perception. To novel and mutilated stimuli are assigned one or a series of classifications just as readily as to the familiar ones. The former are no less promising of classification than the latter in this region.

Certain of the *Os* take the attitude of anticipating the figure; others are more objective and tend to resist anticipations until the percept has acquired sufficient clearness to suggest an acceptable form. Each group of *Os* is fairly consistent with his own standard in this particular. All report a higher degree of attention, combined with increase in muscular tension, during the time when the figure is becoming auspiciously integrated and familiar than either at the time when it is perceived as disorganized or when it passes eventually into the field of clear vision.

As the novel figure begins to acquire, or to give promise of acquiring, a figure classification, most of the *Os* report certain movements in the figure. An *O* is uncertain whether a figure is the square or the oblong, and reports a series of alternations of elongating and telescoping movements in the figure until it has reached a position at which it can be clearly seen. One end of the ellipse makes similar alternations between a pointed and a rounded end as an *O* is uncertain whether the figure is an ellipse or a true oval. As a rule, these changes take place suddenly, although there are occasional reports of delayed fluctuations of this sort.

In a similar manner, certain details of a figure are altered, shifted or ignored so as to effect correspondence of perceived lines with the imagined figure. The vaguely perceived lines usually suggest several tentative figures. In each case, the vaguely perceived pattern looks different from moment to moment, corresponding with each change in imagined figure. This tendency is shown by *Os* who tend to classify the figures in one way only as well as by those who successively assign the figures to different classes. As examples of the latter type, the letter K is first reported by an *O* as H in which the two verticals with the connecting line are described, then as X in which the vertical side of the figure is altered so as to form this figure, then as two triangles with apices joined in which the vertical side is again bent outward so as to form regular triangles, then as the letter R in which there is filling in at the superior opening, then as P in which the lower diagonal is completely ignored, and finally correctly as K. Likewise, another *O* sees the pig first as a blimp in which its feet are ignored or shifted to the center of the figure to form the cabin, then as a dog in which the legs and body of the pig are greatly lengthened, then as a

cartridge on its side in which the legs are totally overlooked and the anterior part modified so as to form the point of the cartridge, then as the dog again and lastly as a pig. Instances could be cited in which the correct classification is reported twice in such a series of labels, once early or near the middle of the series and again at the end as the figure acquires sufficient clearness to be definitely seen. The accidental suggestion of the correct classification at an early point in this stage does not serve to inhibit the emergence of other labels, owing to the fact that the figure is not yet clearly seen.

Our experiments in this region of the field of indirect vision demonstrate clearly the intimate way in which central and peripheral processes are interwoven in the perceptual function. The fact that the figure is presented in such a position that it is unclear and that it acquires clearness only gradually serves to effect a retardation in the development of a definite class label, and thus reveals the manner in which a vague objective pattern undergoes apparent changes in correspondence with shifts in subjective content. Under these conditions, it appears that, within certain limits, the objective content is not perceptually determinative and positive but is capable of manifesting variable modes of appearance. The most conspicuous changes include: (1) the ignoring of a part, or parts, of the figure; (2) the addition of something to the figure; and (3) the shifting or altering of the figure, in part or as a whole, in regard to direction, extent, position, magnitude, etc. These findings bear resemblance to the illustrations given by Köhler in Chapter III of his *Gestalt Psychology*.

(4) *Clear figure.* This region begins at the point at which the figure is accurately seen and positively named. The determination of the outer limit of this field was the task of earlier investigators (*vide supra*). As the stimulus approaches the outer limit of this field, the *Os* who manifested the tendency to multiple classification in the previous region tended either to eliminate certain of the tentative classifications or to select one or two of the most promising ones and thus continue until the correct figure was apprehended. The other *Os* adhered to the classification first tenatively adopted until it was positively confirmed or they abandoned it for a newly suggested form. In the case of all *Os* the final clarification of the figure was characterized as taking place gradually rather than suddenly. Even within the region of clear perception, the figures grow gradually clearer until they reach the center of the visual field. When the figure is clearly seen and definitely named, the *Os* report a general relaxation in muscular tension. Details, such as mutilations, are accurately described, and novel figures to which tentative labels have been assigned are accurately described and the labels promptly dismissed. No type of figure manifests a consistent advantage over others in the matter of being perceptible farther afield.

Classification

All of the stimulus forms are unclassified in the outermost parts of the field of vision, despite the fact that in central vision most of them are classified. Accordingly we attempted to secure reports of the experiences at the moment when the figure made the change from an unclassified to a classified form. In spite of the fact that

the stages in transition from unclassified to classified figure are frequently drawn out and consist in distinct steps, the occurrence of each step in the sequence is uniformly characterized as sudden rather than gradual. In certain trials the number of steps in the sequence are few, in others there are many. Almost invariably increased kinaesthesis is reported to occur during the transition from unclassified to classified figure. In the extreme periphery, where the figure is unclassified, the *O* is passive, but as suggestions of class come there is increased muscular tension until a definite class is accepted, whereupon relaxation takes place. As a rule, the general class to which a figure belongs—letter, geometrical form, etc.—is reported earlier than the specific figure name. Most frequently incipient speech activity is reported just preceding the appearance of the general class name, as is also the case just preceding the appearance of the specific form name. Imagery of various kinds, mostly visual, is sometimes reported but seems to play a subordinate rôle in the situation.

Four stages in the development of classification may be differentiated. First, there is the *pre-perceptive stage* in which a form is seen but is not referred to a class, general or particular. The *O* has a vague notion that at any moment he will be able to cognize the figure. This anticipation of cognition is supported by increase in muscular tension which now sets in and continues until the figure is eventually identified.

In the second place, the *general class name*, is suggested. In certain *Os* a vaguely described kinaesthetic or visual schema of localization is called into operation in this stage. The different general classes of form occupy definite positions in a vaguely experienced kinaesthetic or visual field. Only two *Os* rely upon this sort of aid in initially localizing the figures. The other *Os* merely give the general class name verbally in this stage.

The second stage is sometimes omitted in cases where the third stage, the *specific figure name*, appears immediately following the initial pre-perceptive stage. The specific verbal label appears before the figure is sufficiently clear to be positively identified, and frequently a series of specific names within a general class is tentatively assigned.

The last stage is that of *verification* or *positive identification*. The figure is now clearly seen and recognized, and mutilations are accurately noted and nonsense forms are first perceived as incapable of classification and accurately described.

2. Physiological Theory

Several physical or physiological factors may be mentioned as contributing more or less to the inadequacy of perception in the peripheral field of vision. In the first place, the mechanism of accommodation may be perfect for the foveal region only. In the second place, the light rays from an object in indirect vision strike the cornea and lens less directly and for that reason undergo greater spherical aberration or astigmatic effect. Thirdly, the density of retinal elements stimulated by the figure and the number of optic fibers which supply these retinal terminals has been suggested as a principle of explanation. Fourthly, the retinal terminals in the field of indirect vision, may, as one proceeds from center to periphery in the retina, become more and more undeveloped, rudimentary or ineffective, perhaps in part from lack of use.

While all of these factors may contribute to the condition of inadequate perception in the periphery, we are inclined to believe that the principal factor is to be found in the retina. If the explanation were essentially in terms of optical defect, greater irregularity might be expected in our results. The results of the two *O*s who are slightly near-sighted might also be expected, if that theory were correct, to show anomalies which are not apparent in the results of two *O*s, E and K. As between the third and fourth views, the histological examination of retinal elements fails to indicate any radical difference between those in the center and in the periphery of the retina. Consequently we must fall back upon the fact of difference in nerve supply which Cajal discovered (*vide supra*).

There is also no doubt that central or cerebral factors play a part, since in the form-like region a tentative classification tends to produce alterations in the appearance of a vaguely perceived figure. However, the prepotency of retinal function is indicated by the fact that the several forms tentatively assigned in this region are invariably reduced to the true form as soon as the figure is clearly seen.

Summary

Four different fields of the apprehension of form are demonstrated in the visual field in the four major quarters. In the outermost one—*figureless field*—the background only is perceived; there is no figure. In the adjacent field—*formless figure*—an un-

organized mass of marks or lines which are meaningless is vaguely seen, and the principal dimension of the figure may be reported. In the next zone—*form-like figure*—there is a figure formation which suggests several names in succession, and this figure may manifest different modes of appearance to correspond with the successive arousals of names tentatively accepted. Mutilated figures and alterations of details of figures are not perceived in this region because of indistinctness of the figure. In the innermost region—*clear figure*—the details of the figure are accurately perceived.

The four regions just named are most extensive in the temporal and least so in the superior quarter. The nasal and inferior fields have roughly the same extent in all cases. For a given *O*, all figures tend to make the transition from one region to the next adjacent at approximately the same point, that is to say, the zones for all figures are roughly coextensive. Individual differences appear in the extensions of the various fields.

In view of the fact that in these experiments a figure passes gradually from an unclassified to a classified position, several steps and conditions in the process of classification are indicated. Increased kinaesthesis takes place during the transition and vague localization schemata are mentioned. The figure first appears formed but unfamiliar, then the general class name is suggested, then the specific name (or names) appears, and finally the figure is positively identified and recognized.

The explanation of inadequate vision in the indirect field may be attributed to (1) optical defects, astigmatic conditions, etc., or to (2) some retinal factor. In view of the consistency of our results, the retinal factor appears to be the more probable explanation. The reduction in density of retinal terminals in the periphery and the fact that an optic fiber in indirect vision supplies a number of terminals, whereas in the fovea each optic fiber supplies only one or at most two terminals, is probably a sufficient explanation of the differences existing between central and peripheral perceptual adequacy.

A PHENOMENOLOGICAL DESCRIPTION OF RETINAL RIVALRY

By Max Meenes, Lehigh University

Although a great deal of work has been done on retinal rivalry, interest has rarely been shown in the phenomenological character of the process itself. Rivalry has often been considered as a hindrance to binocular mixture or as an obstacle to the fusion of the monocular fields into a coherent binocular field.[1]

For Dobrowolsky[2] rivalry occurred under bad conditions of mixture and "the disappearance of mixture and the appearance of rivalry of the fields of vision depended upon the fluctuations of convergence and unprecise fixation." Chauveau was interested in eliminating rivalry, as was Trendelenberg, since it stands in the way of fusion.[3] Mixture, and the extent to which it occurred in rivalry was also the main concern of Dawson.[4] Kuroda[5] looked upon rivalry as a phenomenon intermediate between dominance and mixture which to him were the two extremes of a series of binocular phenomena. Other investigators were concerned with the relation of attention to rivalry. Helmholtz, Volkmann, and Ruete maintained that rivalry may be voluntarily controlled and is subordinate to attention, while Hering and Panum took an opposing position; Fechner and Aubert upheld the middle ground.[6] Breese maintained that rivalry is a case of psychological inhibition and found attention to be a conditioning factor.[7]

Another matter of controversy for the early investigators was the question of the extent to which mixture or rivalry occurs in the common field of vision when two different colors are presented simultaneously, one to each eye. Wheat-

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[1]E. Hering, *Beiträge zur Physiologie*, 1861, 313; also Hermann's *Handbuch* III, (i), 1879, 384; C. G. T. Ruete, *Das Stereoscop*, 1867, 131 f.

[2]W. Dobrowolsky, Über binoculare Farbenmischung, *Pflüger's Archiv*, 10, 1875, 58.

[3]A. Chauveau, Sur la fusion des sensations chromatiques perçues isolément par chacun des deux yeux, *Comptes rendus*, 113, 1891, 361 f.; W. Trendelenberg, Versuche über binokulare Mischung von Spektralfarben, *Zsch. f. Sinnesphysiol.*, 48, 1913-14, 202 f.

[4]S. Dawson, The experimental study of binocular colour mixture, *Brit. J. Psychol.*, 8, 1917, 510 ff.

[5]G. Kuroda, Zur Grenzbestimmung der binokularen Phänomene, *Psychol. Forsch.*, 6, 1925, 296.

[6]H. Helmholtz, *Handbuch der physiologischen Optik*, 1866, 767 ff.; A. W. Volkmann, *Neue Beiträge zur Physiologie des Gesichtssinnes*, 1836, 99; Ruete, *op. cit.*; Hering, *opp. citt.*; P. L. Panum, *Physiologische Untersuchungen über das Sehen mit zwei Augen*, 1858, 38 ff.; G. T. Fechner, Über einige Verhältnisse des binocularen Sehens, *Abhandl. d. sachs. Gesell. d. Wiss.*, 7, 1861, 391, 397; H. Aubert, *Physiologie der Netzhaut*, 1865, 298.

[7]B. B. Breese, On inhibition, *Psychol. Monog.*, 3, 1899-1901, 20 ff.

stone[8] cited Du Tour as the first to get rivalry with colored glasses. Aubert and Panum believed that some mixture was always present in rivalry and Dawson attempted to determine the extent of mixture in rivalry.[9]

Volkmann defined rivalry physiologically as "the sensation arising when differently colored light rays fall on identical retinal points," and Fechner, in terms of stimulus, wrote: "If one has different kinds of contours crossing or adjoining in the visual field of the two eyes, rivalry or reciprocal intrusion usually results in that now the one, now the other, appears with the ground coloring adjacent to it."[10] Hering and Ruete both said that "only when a contour of one retina crosses a contour of the other does a rivalry between the contours occur at the crossing place."[11] We shall see that the statement of Hering and Ruete is not quite true for we have been able to set up rivalry by using for each eye different figures which are not physically designed to cross, and often do not phenomenally cross, in the common field of vision. For Helmholtz rivalry was the fluctuation between parts of the two fields of vision, while Breese and Kuroda defined it in terms of the physical conditions under which rivalry occurs.[12]

Scant bits of description of rivalry are to be found in the literature more or less buried in a mass of explanation. Fechner says that now this, now that, color dislodges the other patch-wise over a large extent or even for a time over the whole extent.[13] Hering says,[14] "If a yellow and blue are brought to binocular correspondence pure yellow may first appear at one place, at the next moment it suffers from the admixture of blue, then becomes grayer and grayer until finally it is seen submerged in pure gray. Out of this neutral mixture the blue now forces itself more clearly to the front, finally completely subdues the yellow and now fills the place in the visual field alone. The victory of the blue does not take place simultaneously over the whole field of battle but the phases of the battle are different at different places." Helmholtz says,[15] "For the most part the restless peculiar color fluctuation is at its liveliest at the beginning; then the sensitivity for the colors is blunted and the aspect becomes quieter, taking on an indistinct more grayish color which fluctuates continually and stepwise between a reddish and more bluish hue." Ruete and Dawson call attention to a difference in localization of the members in rivalry in the third dimension.[16]

Turning to rivalry of figures we find that Wheatstone reported on presenting a figure S to the left eye and a figure A to the right, in coincident circles, that "the common border will remain constant, while the letters within it will change alternately from that which would be perceived by the right eye alone to that which would be perceived by the left eye alone. At the moment of

[8] C. Wheatstone, Contributions to the physiology of vision, *Philos. Trans.*, 2, 1838, 386.

[9] Aubert, *op. cit.*, 298 ff.; Panum, *op. cit.*, 38 ff.; Dawson, *op. cit.*, 510 ff.

[10] Volkmann, *Müller's Archiv*, 1838, 373; Fechner, *op. cit.*, 388.

[11] Hering, *Beiträge zur Physiologie*, 1861, 373; Ruete, *op. cit.*, 129 ff.

[12] Helmholtz, *op. cit.*, 767; Breese, *op. cit.*, 18; Kuroda, *op. cit.*, 282.

[13] Fechner, *op. cit.*, 384.

[14] Hering, *Beiträge zur Physiologie*, 1861, 313 f.

[15] Helmholtz, *op. cit.*, 774.

[16] Ruete, *op. cit.*, 129; Dawson, *op. cit.*, 525.

change the letter which has just been seen breaks into fragments, while fragments of the letter which is about to appear mingle with them, and are immediately after replaced by the entire letter."[17] Panum[18] said that in presenting a vertical line to one eye and a horizontal to the other the vertical line cuts through the horizontal. (The present writer has found, on the contrary, that the horizontal tends to cut through the vertical.) Hering and Helmholtz also used the cross-figure as well as a number of others and Hering described the fluctuations and instability of the central part of the cross-figure fully.[19] Hoppe found an unstable superposition of two sets of lines and Lau, on showing the parallel lines of the Zöllner figure to one eye and the criss-cross lines to the other, found that the two sets of lines were seen in different planes.[20] Nowhere do we find more than a few brief bits of description.

It is the purpose of this paper to deal with the phenomenon of retinal rivalry in its own right and to attempt a phenomenological description of the process or processes customarily called rivalry. We shall concern ourselves only incidentally with conditions which influence the process and shall not seek to make an exhaustive study of such conditions. The investigation was begun in the Psychological Laboratory of Cornell University, and was carried through to completion at Clark University.[21]

Observers. The *O*s were Dr. L. B. Hoisington (*H*), a member of the instructing staff at Cornell with much experience in observing on psychological experiments; Dr. J. P. Nafe (*N*), a member of the instructing staff at Clark with about four years training in observation; Dr. C. A. Dickinson (*D*), Honorary Fellow at Cornell with about two years training in observation; Mr. G. Kreezer (*K*), and Miss H. Bateman (*B*), first year graduate students at Cornell with no previous training in observation; Mr. F. Geldard (*G*), and Mr. R. W. Nafe (*R*), graduate students at Clark each with one year of previous training in observation.

I. Experiments with Colored Glasses and Figured Backgrounds

Apparatus. A wooden frame to hold the colored glasses was constructed to fit over the tubes of a Ludwig tropostereoscope mounted on adjustable standards. A sheet of white cardboard, with a brightness value of 0.845 when

[17] *Loc. cit.*

[18] *Op. cit.*, 17 ff.

[19] Hering, Hermann's *Handbuch*, 380 ff., and *Grundzüge der Lehre vom Lichtsinn*, 1920, 241 f; Helmholtz, *op. cit.*, 767 ff.

[20] J. I. Hoppe, *Psychologisch-physiologische Optik*, 1881, 152 ff.; E. Lau, Versuche über das stereoskopische Sehen, *Psychol. Forsch.*, 2, 1922, 1-4.

[21] Acknowledgment is due to Professors E. B. Titchener and L. B. Hoisington, formerly of Cornell, and to Professor J. P. Nafe of Clark, for their very material assistance.

compared with a magnesium carbonate target, was set up in a frame 50 cm. from the wooden frame of the tropostereoscope. On this white cardboard were drawn two series of oblique parallel lines sloped at an angle of 45°. One set of lines sloped from the upper left down to the lower right of the field and was arranged in such a way as to be seen with the right eye only, and the other set sloped from upper right to lower left and could be seen only with the left eye. The stripes were of black ink, 1 mm. wide and 2 mm. apart and ran obliquely all the way across one or the other of the monocular fields. The *Os* looked at these fields of differently directed oblique parallel lines through colored glasses, different for each eye, set in the frame of the tropostereoscope.

The glasses used were the set of seven monochromats of the Wratten Light Filters prepared by the Eastman Kodak Company, and one ordinary yellow glass of low absorption value. These monochromats are numbers 70 (dark red), 71A (light red), 72 (orange), 73 (yellow green), 74 (blue green), 75 (blue), and 76 (violet).[22] The percentage transmission of the eighth glass used, the ordinary yellow, was:

Wave length:	450	480	500	520	540	560	580	600	640	680
Transmission:	16	23	30	39	48	54	60	63	60	59

The stimuli were presented by the method of paired comparisons, the space error being compensated for by presenting each color an equal number of times to each eye. All experiments were performed in a dark room where the light from two 200-w. Mazda bulbs filtered through Gage 'daylight' glass and fastened to standards 1.5 m. from the floor on each side of the *O*, illuminated the cardboard evenly and was reflected down the tubes of the tropostereoscope and through the colored glasses.

Instructions. The following instructions were read to the *Os:* "You are to give a full description of experience with special reference to moments of change in the field of vision. You may give the report while the experience is going on."

Results.—In the rivalry of colors and differently directed sets of diagonal lines, the lines usually make their appearance before the color belonging to them. While change is actively progressing, one set of diagonals becomes gradually more and more prominent, darker, and clean-cut, while the other set disappears. Sometimes the color comes in along the lines between the diagonals, in which case it progresses fairly uniformly across the field, usually with the diagonals slightly leading, but sometimes across the diagonals, when the lines appear to bound the color which comes in haltingly, 'flopping down' at each diagonal as if the diagonal served to halt the process. Each set of lines was apprehended as belonging with a particular color. The report of the *Os* was usually given in terms of one of the members of the rivalry and the changes were appre-

[22]Transmission and absorption values for these glasses may be found in *Wratten Light Filters*, published by Eastman Kodak Co., 1925, 47 ff., 68 f.

hended as taking place in relation to it; thus there was an active and a passive member of the rivalry. Only occasionally were the two members in rivalry almost equal in dominance.

The active member is apprehended as being nearer the *O*; it expands and contracts covering or blotting out the passive, or it thins out exposing the other, and is more filmy, more distinct and more mobile. When neither member predominates, parts of each color with its set of lines may be seen in different parts of the field, but more frequently one of the members predominates and is seen more clearly and in front of the other. When the two colors appear equally bulky there is no difference in apparent localization; in other cases the more filmy color is nearer the *O*. The active, nearer color fluctuates in filminess, thickening up or thinning out. The passive member is usually seen *through* the thinned out, filmy, active one. The diagonals with the passive color never come out as clearly and as sharply nor do they appear as dark or as black as those with the active color, though they have the same stimulus value.

II. Experiments with Colored Glasses

Apparatus. For the first part of this work, involving *H*, *D*, *K*, and *B*, the experimenter employed the hood of a Brewster-Holmes stereoscope from which the lenses had been removed. It was mounted on a stand adjustable to the sitting height of the *Os*. On the back of this hood a wooden frame was constructed to hold two colored glasses, one for each eye. The same colored glasses were used as in the preceding experiment but in place of the card with the differently directed diagonals there were three different cards, each of uniform grayness and set in a frame 16 cm. from the back of the hood. The cards had the value of 0.042, 0.126 and 0.845 when equated with the magnesium carbonate target, and each card was presented in turn with each pair of colors. The lighting conditions, instructions and procedure were the same as in the preceding experiment. The light was reflected from one of the gray backgrounds through the colors.

For the second part of this work, involving *N*, *R*, and *G*, the experimenter made use of a haploscope consisting of two parallel cardboard tubes 4 ft. in length and 2 in. in diameter. The insides of the tubes were lined with white and were free from contours or irregularities. These tubes were set, interocular distance apart, in a large wooden frame designed to shut off all light from the *O* except that reflected down the interior. The whole apparatus was set in a dark room. A stereoscopic hood was mounted over the end of the tubes nearest the *O* to shut out lateral light, and a wooden frame to hold the colors used as stimuli was attached to the other end of the tubes. This frame held two colors, one fitting over the end of each tube, and thus each seen with one eye only. A sheet of white cardboard serving as a background for the colored glasses, was

set up 17 cm. from the far end of the tubes. Between this cardboard and the frame on the tubes were two 150-w. daylight Mazda lamps. The light from these lamps was reflected from the white background through the colored glasses and down the length of the tubes. This apparatus permitted great freedom in manipulating and controling the stimulating conditions. The glasses used, instructions and procedure were the same as before.

Results. The process of rivalry proved to be essentially the same under these conditions as in the preceding case of colored glasses with differently directed diagonals. It also appeared that the lighter the background the more lively the rivalry. Nevertheless phenomenologically rivalry was in this case also an experience in every way like that of the earlier experiments. There was an active and a passive member, each having the characteristics already described above.

In general, it may be said that rivalry is a tridimensional experience in which the active color spreads over the whole field although it cannot be said to 'advance' because there are no contours. The passive color shines through the active as the active thins out in withdrawal. The more stable, the less active, color is the more persistent. The front color is more 'surfacy,' more filmy, and acts as the invader, while the color behind it is the more bulky and the more constant.

Fractionation. In four series the *Aufgabe* was narrowed and a report on one specific aspect of the experience was required. The stimulating conditions were unchanged; only the instructions were varied. These results follow.

(*1*) *Texture.* The *O*s were instructed to "name the color or colors, and to report on the texture of colors seen and changes in such texture, if any." Nothing new came out of this series.

(*2*) *Localization in the third dimension.* Here the *O*s were asked to report on the "relative localization in the third dimension of the colors seen and changes in the relative localization, if any." *R* had great difficulty in following these instructions and reported tridimensional differences but rarely, though there are a fair number of such reports in his earlier introspections. The other *O*s reported such differences in well over half the cases.

No difference in localization was reported when the colors in rivalry appeared to the *O* to be of the same texture, but when there was a difference in texture the more filmy color was reported on a plane in front of the other, between the other color and the *O*.

These differences in tridimensional localization and texture were independent of ocular differences, for the reports were the same no matter which color was seen by which eye. The reports were consistent for each *O* but since they varied with the *O* no attempt was made to arrange the colors in respect to relative filminess and nearness.

(*3*) *Stability and movement.* The *O*s were instructed to report on "whatever differences in the stability of the members of the rivalry" were observed.

Under this *Aufgabe*, the *O*s ordinarily reported a difference in stability which correlated very highly with the differences in texture and relative localization in the third dimension. One color was usually apprehended as the stable member while the other was taken to be the invader, the active, the one which did the moving, spreading, *etc.* The stable member corresponded in general with the less filmy and the color further away from the *O* in space, while the active or invading member corresponded to the more filmy and nearer color. These results were also independent of ocular difference and were consistent for each *O* although there were small individual differences.

(*4*) *Comparison of colors seen monocularly and in rivalry.* In this series the *O*s were instructed as follows: "At the ready signal you are to bring your head with eyes closed into position in the hood of the haploscope. At the first 'now' signal, open the right eye only and observe the field of the right eye. At the second 'now' signal open the left eye and describe whatever changes occur in the field seen first with the right eye only." In half of the experiments in this series the instructions were modified, 'left' being substituted for 'right' and 'right' for 'left.'

The results show that there is a change in brightness, extent and texture in the color seen first monocularly and then in rivalry with another color. Usually the color becomes darker, although violet changed in the direction of brighter. Often the second experience is reported as different, there being practically nothing in common between the two experiences. These results also appear to be independent of ocular difference.

III. Rivalry of Hueless Figures

Various designs were drawn with black ink on white cardboard and were placed in a frame 16 cm. from the far ends of the tubes

of the haploscope. Each figure was so constructed as to come well within one of the monocular fields. The *Os* were instructed as follows: "At the ready signal you are to bring your head with eyes closed into position in the hood of the haploscope. At the now signal open the two eyes together and look down into the tubes. Give a complete description of experience with special reference to moments of change in the field of vision."

The designs used were as follows: (1) vertical line 3 cm. long and 3 mm. wide; (2) horizontal line 3 cm. × 3 mm.; (3) vertical line 2 cm. × 3 mm.; (4) horizontal line 2 cm. × 3 mm.; (5) vertical line 1 cm. × 3 mm.; (6) horizontal line 1 cm. × 3 mm.; (7) vertical broken line consisting of two parts, each 12 mm. × 3 mm. with a gap of 6 mm. in the center, total length of figure 3 cm.; (8) horizontal figure of same dimensions as (7).

(*a*) *Similar figures differently directed.* In this series every design was presented in turn to each eye but was always paired with an identical figure differently directed, presented to the other eye; *e.g.* left design (1) and right design (2). Rivalry always occurred, never a stable design.

Each *O* reported movement, difference in stability, and changes in quality. In general the horizontal bar was reported as the active member, the blacker, and the more mobile, for it appeared to break the vertical. The *Os* reported a design much less stable than the Helmholtz cross,[22] the arms of which Helmholtz described as being quite black at the ends and almost white where they border on the central black square. The *Os* reported no such symmetrical design with a stable square in the center and fluctuations in brightness of the four arms of the cross near the central square. There was no black central square separated from the arms of the cross; in fact, the *Os* did not describe any kind of union of the two designs.

(*b*) *Different designs similarly directed.* Two vertical (or two horizontal) designs differing in length were presented simultaneously, one to each eye. Rivalry always occurred under these conditions; never was a single fused durative impression reported. The shorter line was usually reported the more active, and the darker, changing less in quality.

(*c*) *Different designs differently directed.* The stimulus-designs in this series were the same as in the two preceding but

[22] *Op. cit.*, 767 ff.

they were always paired in such a way that they differed in size and direction. In each presentation one design was vertical and the other horizontal and the two were of different size.

The horizontal line was the active member of the rivalry; frequently, even when longer than the vertical, it did most if not all the moving, and was qualitatively the more stable and the darker. It disappeared or broke much less frequently.

(*d*) *Miscellaneous designs.* In addition to the eight stimulus-designs used in the three preceding series we introduced the following new ones: (9) two lines 3 cm. × .3 mm. joining near the lower right corner to form a right angle; (10) two similar lines joining at the upper left corner to form a right angle; (11) two vertical parallel lines 3 cm. long × 3 mm. thick and 27 mm. apart; (12) two horizontal parallel lines of the same dimensions and distance apart. Stimulus-designs (9) and (10) were so constructed that when presented together in a common field of vision, one to each eye, and when measured from a common central fixation point, they formed a square. Stimulus-design (11) and (12) were also constructed with the same end in view.

The results show that in the combined visual field the ends of the designs never united to form a coherent figure, the expected square, but that rivalry invariably took place at the free ends. The two angles, as well as the two sets of parallel lines, never formed a square and were reported as not belonging to each other. There were frequent reports in terms of active and passive members with the same characteristics reported above.

IV. Rivalry of Objects

The haploscopic arrangement used in the preceding experiments were used in this series also. Small wooden cylinders were used as stimulus-objects. These objects were: (1) a vertical cylinder 3 cm. long and 3 mm. in diameter; (2) an horizontal cylinder of the same dimensions; (3) a vertical cylinder 1 cm. long and 3 mm. in diameter; and (4) an horizontal cylinder of the same dimensions. These objects were presented in pairs, one to each eye, against a black velvet background, with distracting shadows reduced to a minimum.

In spite of the fact that under the conditions of our experiment these stimuli were not seen as tridimensional objects but appeared to the observer rather like the figures shown in the preceding

series, it seemed well to use a short series of experiments with tridimensional objects as stimuli in order to see if the experiences set up differed from those in the preceding series. However, rivalry under these conditions was essentially the same as in the foregoing series.

Conclusions

(1) Rivalry is the differential behavior of two visual members, integrated into a "unit-like complex." It is a complex of durative visual experience in which one set of experiences, apprehended as a unit, behaves differently from another set of experiences, also apprehended as a unit.

(2) Usually one member predominates, though alternation is possible; hence there are an active member and a passive member.

(3) The active member is nearer the *O*; the passive is further away.

(4) The active member undergoes a patchy expansion and contraction; the passive is covered by it, or blotted out by it.

(5) The active member is filmy; the passive is less so.

(6) The active member is blacker in black on white objects; the passive is grayish.

(7) The active member is distinct; the passive is blurred.

(8) The active member is mobile; the passive is static.

(9) The active member behaves as the 'invader.'

(10) The active member is apt to be determined by a horizontal stimulus.

(11) The active member is even more apt to be determined by a smaller stimulus.

(12) All these results are independent of ocular difference.

(13) The unit-like character of the complex is derived from the reciprocal relation of the attributes noted in (3) and (8) above, and is thus unlike figure and ground, the fluctuations of the negative after-image, or fleeting clouds.

THE POSTURE OF NURSERY SCHOOL CHILDREN DURING SLEEP

By M. Adelia Boynton and Florence L. Goodenough,
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Although the question of bodily posture during sleep has considerable theoretical importance, it appears to have received almost no attention in the psychological literature. A number of studies have been made dealing with the amount of sleep taken at various ages,[1] the effect of deprivation of sleep, the intensity of the stimuli necessary to awaken a sleeper under various conditions, and the frequency of postural changes during sleep,[2] but thus far, no one appears to have attempted to ascertain whether or not the frequency of postural changes during sleep is associated with the character of the postures assumed, or whether certain postures more than others tend to favor the induction of sleep.

A possible exception may be cited in the work of Sidis who makes the following statement.

"Now I have suspected for some time, that if the condition of limitation of voluntary movements is one of the important factors in the induction of sleep we should expect that it would not be a matter of indifference on which side we rest. We should expect that if the right side is the more active that the limitations of the bodily movements would be more marked on that side. In carrying on my experiments on children and adults when in their sleeping states I had occasion to observe that there was a definite course in the motor reactions in the process of falling asleep. There is a method of sleep. Some people go to sleep only on their backs and find it difficult to fall asleep otherwise, while others who go to sleep on their side and who form the greater majority always go to sleep on the same side. There are very few who fall asleep indifferently on either side. Moreover, my observations have shown that by far the majority of right-handed people go to sleep on the right side, while left-handed people go to sleep on their left side. I have further verified this interesting fact by statistical inquiry among my patients as well as among Harvard students. Some of the right-handed people who go to sleep on the right side may after some time turn to their left to change position, while others keep on sleeping on the same side throughout the whole night. The majority change position, the right-handed to the left and the left-handed to the right. More than 75% of right-handed people have given records to the effect that they sleep on the right side, or rather fall asleep on that side. Of the left-handed persons I find only one out of ten who fall asleep on the right side."[3]

This statement is highly interesting, and if true might have considerable bearing on theories of sleep. Unfortunately, however, Sidis presents no figures,

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[1] J. C. Foster, F. L. Goodenough, and J. E. Anderson, The sleep of young children. *Ped. Sem.* 35, 1928, 201-218.

[2] H. M. Johnson, Sleep, *Psychol. Bull.*, 23, 1926, 482-503.

[3] Boris Sidis, An experimental study of sleep, *J. Abn. Psychol.*, 3, 1908, 1 ff.

and the method which he appears to have used in collecting his data is of questionable validity. In the absence of verification by more objective methods, the amount of confidence which can be placed in the findings is small. As will be shown later, the results of the present study are not in accordance with those reported by Sidis.

Some theoretical as well as practical significance attaches itself to the popular observation that the induction of sleep is facilitated by the presence of sensory cues which, through habit, have become associated with the act of falling asleep—the kind of clothing customarily worn at night, the familiar furnishings of the bedroom, etc. People sometimes state that they are "unable to sleep well in a strange bed" even though the bed may be quite as comfortable as their own. Among these sensory cues, kinaesthetic sensations arising from bodily postures presumably play a part. The question then arises whether those individuals who show marked uniformity of bodily posture on falling asleep go to sleep more quickly on the average than do those whose postural habits are more variable, and in whom, therefore, the kinaesthetic sensations arising from a particular posture would be less strongly suggestive of sleep.

Object. In the hope of throwing light on these and other problems relative to posture during sleep, a number of nursery school children were observed during the school nap-hour.

Method. One of the authors (Boynton) made all the records of this study. She used a form sheet, a copy of which follows,[4] that was prepared especially for this study. For illustrative purposes, the record is partially filled in.

It was found that after practice had been gained in making the records, six children could be observed at once with reasonable accuracy. The six record sheets were arranged in order upon a large blotting pad in such a way that all were visible at once. The observer then seated herself before the cots,[5] holding the pad in her lap, and choosing a position from which all children could be seen clearly. During the observational periods no responsibility for the care of the children rested upon the observer.

Records were made as follows. The spaces above the heavy line were filled in at the beginning of the hour. As each child went to his cot, the time of lying down was recorded. Activities preceding sleep were noted by underlining as

[4]A slight rearrangement of the form has been made here for the sake of economizing space. In the record sheets actually used all the descriptions of posture were arranged in a single column running down the left-hand side of the page, thus allowing space for 18 columns of records.

[5]The children sleep in low canvas cots 24 in. wide without sides. Shoes are removed before lying down, but other garments are retained. The covering consists of a single blanket through which posture can readily be observed. The cots are placed with heads against the wall and are separated from each other by a space of 12-18 in.

they occurred. The moment of going to sleep was judged as accurately as possible, using the ordinary criteria of closed eyes, regular breathing, and cessation of bodily movements. Waking time was judged in a similar fashion. If, as sometimes happened, a child did not waken spontaneously before the time of preparation for going home, it became necessary to waken him, in which case the time of awakening was recorded in the space labelled 'awakened at.'

SLEEP STUDY NO. 2, INSTITUTE OF CHILD WELFARE, UNIVERSITY OF MINNESOTA

Name F—— B—— Date Jan. 13, 1929 Room No. 301 Room temp. 60°F Humidity ——

Weather: bright, cloudy, stormy (rain, snow, thunder, wind) outside temp. +16°F.

On cot at 12:54 asleep at 1:10 time before sleep 16 awake at 2:48

Awakened at —— time asleep 1-38. If no sleep, up at —— time on cot ——

ACTIVITIES PRECEDING SLEEP: Went to bed (willingly, with protest, forced).
Motor: (quiet, somewhat restless, very restless on cot, left cot —— times).
Vocal: (none, murmuring to self, conversation, soft crying, loud crying).
Respiratory: (sneezing, coughing, difficult breathing).
Soporific habits: (sucking hand, sucking clothing, handling genitals).
Eliminative: off cot for (urination, bowel movement).

ACTIVITIES DURING SLEEP:

	1	2	3	——18	
Trunk					
On back	1:10				
On stomach					
On right side		1:38	✓		
On left side					
Head					
Straight					
Turned right	1:10	✓	✓		
Turned left					
Right Arm					
Above head					
Under head					
At side	1:10				
Extended					
Flexed		1:38	✓		
Left Arm					
Above head					
Under head					
At side					
Extended	1:10		1:51		
Flexed		1:38			

ACTIVITIES DURING SLEEP:

	1	2	3	——18	
Right Leg					
Straight	1:10				
Hip flexed		1:38	✓		
Knee flexed		1:38	✓		
Left Leg					
Straight	1:10				
Hip flexed					
Knee flexed		1:38	✓		
Vocal and Respiratory					
Murmuring					
Whispering					
Talking					
Crying out					
Diff. breathing					
Snoring					
Sneezing					
Coughing					
Seemed Awake					
Reason unknown or					

The posture of each part of the body at the moment of going to sleep was recorded by writing the time of going to sleep in column 1 opposite each appropriate descriptive phrase. As movements occurred the time of each was recorded in the successive columns opposite the phrases describing the posture at the cessation of movement, thus giving a continuous account of both the time of the movements and their general character. Thus, in the example given, the child is recorded as having fallen asleep at 1:10 after having gone to his cot at 12:54. His bodily posture on falling asleep was as follows: on his back with his head turned to right, right arm at side, left arm extended, both legs straight. The first movement occurred at 1:38, after which his posture was as follows: on right side, head still turned to right (as indicated by check mark), both arms flexed, right leg flexed at both hip and knee, left knee flexed. At 1:51 the left arm was extended, other parts of the body remaining the same. It was not found feasible to attempt a description of the character of the movements through which change in posture was effected. Thus, the first movement may have involved only the minimum activity necessary to produce

the indicated change in posture, or there may have been a momentary tossing about of the whole body with subsequent coming to rest in the posture described. When there was no change in posture as a result of movement, the time of movement was recorded in the first appropriate space and the remaining items checked as before. Since only six children were observed on any one day, care was taken to vary the order of observation in such a way as to avoid bunching the records of the individual children on the same day of the week.

A total of 278 nap-records were obtained from 56 nursery-school children ranging in age from 2 yr. 0 mo., to 4 yr., 11 mo. Instances when a child lay down but failed to go to sleep have not been included. The number of records for each child varies from 1 to 16. Twenty-five children had at least 5 records each. The total sleeping time included was 367 hr. 4 min. The mean length of the nap-period was 1 hr. 19 min.

Results

(*a*) *General findings.* With the limited amount of data available, it did not seem worth while to consider minor variations of posture separately. Results have therefore been treated chiefly with reference to main bodily posture as indicated by the trunk. All movements, whether of trunk, head, limbs, or entire body have been counted as movements, but the resultant changes in posture have been disregarded except as they affected the trunk. A brief summary of the posture of head and limbs is, however, presented in a later section.

The data have been examined for effects of age and sex, but as no reliable differences were apparent, age- and sex-groups have been combined in the final presentation. The mean time required to go to sleep was 34.8 min., S.D. 20.7 min. The mean length of nap was 1 hr. 19 min., S.D. 24.5 min. The total number of movements recorded was 875; the mean length of rest-period (interval between movements), 25.3 min. As will be shown later, the length of the rest-period varies with the duration of sleep.[6]

Table I shows the percentage of the total sleeping-time which was spent in each of the four main bodily postures, and the percentage of the total number of occasions when the initial bodily posture (at the onset of sleep), was as indicated.

[6]This might be thought to be the result of age-differences, but at least two studies (cf. E. A. Bott, W. E. Blatz, N. Chant, and H. Bott, Observation and training of fundamental habits in young children, *Genet. Psychol. Monog.*, 4, 1928, 25; and Foster, Goodenough, and Anderson, *op. cit.*, 2) confirm our findings that such differences are practically non-existent. As age increases the frequency at which the nap is omitted increases, but the length of the nap when taken shows but little tendency to vary with age.

Slightly over 60% of the total sleeping time is spent on the side, with the right side being given a small preference over the left. The rank-order of the four postures is the same, whether initial bodily postures or total sleeping time is considered. This is in part a result of the length of the first rest-period, which, as will be shown later, is considerably in excess of that of later rest-periods.

TABLE I

BODILY POSTURES DURING AND AT THE ONSET OF SLEEP

Posture	Left side	Right side	Back	Abdomen
During sleep	29.1%	31.1%	12.9%	26.9%
At onset	27.7%	30.6%	16.9%	24.8%

Since all these children tended to be right-handed, the slight preference for the right side might seem to offer some confirmation of Sidis' theory. Inasmuch as a number of the cases had recently served as subjects in an investigation on the development of handedness,[7] it was possible to make a more intensive study of this factor. For 23 of the 25 children who had 5 or more sleep records, scores on Miss Light's very comprehensive series of tests for handedness were also available. For these children, the rank-order correlation between strength of preference for the right-hand and percentage of times the child went to sleep on the right side was $+0.02 \pm 0.140$; between right-hand preference and percentage of total sleep on the right side the correlation was -0.11 ± 0.13. Apparently the most right-handed children are no more likely to sleep on the right side than those who show little preference for the right hand. The range of handedness scores for these children was from very marked right-hand preference to almost complete ambidexterity but still with slight preference for the right hand. There were no left-handed children in the group.

The corresponding figures for the left side, however, tell a somewhat different story. The correlation between right-hand preference and percentage of times the child went to sleep on the left side was $+0.338 \pm 0.124$; between right-handedness and percent of total sleep on the left side $+0.530 \pm 0.101$. While both the number of cases and the number of records for each case are too small to warrant a final conclusion, these results strongly suggest that the slight general preference for the right-side posture is the result of factors other than handedness, and that the general tendency is to leave the preferred hand free during sleep rather than to assume a posture which would inhibit its movements.

The effect of strength of postural habit-formation upon length of time required to go to sleep has been studied from several points of view. As a measure of strength of postural habit, we have used the highest percentage of the sleeping time spent in any one

[7]Margaret Light, Unpublished doctoral dissertation, University of Minnesota.

posture, disregarding the nature of the posture. With four postures, the possible range of percentages for the most favored posture would then be from 25%, if the sleeping time were divided equally among the four, to 100% if the entire sleeping time were spent in a single posture. The correlation between the mean time required to go to sleep and the strength of postural habit according to this criterion was, for the 25 children with 5 or more nap-records, $+0.44 \pm 0.109$. This suggests that uniformity of kinaesthetic cues does tend to facilitate the induction of sleep to some extent. We have also computed the correlation between the mean time required to go to sleep and the strength of initial postural habit, using as a criterion the highest percentage of times that any one posture was assumed at the onset of sleep. In this case the correlation was $+0.40 \pm 0.113$. These correlations are based only upon the 25 cases with 5 or more nap-records.

Apart from the factor of postural habit, it is conceivable that certain positions, rather than others, tend to favor the induction of sleep. We have therefore computed for the entire group of *S*s the mean time required to go to sleep according to the posture assumed at the onset of sleep. The results are shown in Table II.

TABLE II

MEAN NUMBER OF MINUTES REQUIRED TO GO TO SLEEP IN VARIOUS POSTURES

	Left side	Right side	Back	Abdomen
Mean	35.2	35.8	33.9	34.0
σ	15.9	16.1	16.8	16.2
No. cases	77	84	48	69

The data presented in Table II do not show any reliable superiority of one posture over another in regard to facilitating the onset of sleep. It appears, therefore, that while the formation of some kind of postural habit is a factor of some importance in the induction of sleep, the particular nature of the habit is of little or no significance.

(*b*) *Posture as related to frequency of movement.* Table III shows the mean length of rest-period (interval between movements) for each of the four main postures.

TABLE III

MEAN LENGTH OF REST PERIOD IN EACH POSTURE

	Left Side	Right Side	Back	Abdomen
Mean	27.4	30.7	22.9	22.8
σ	21.3	23.4	19.5	18.8
No. cases	244	232	132	267

Movements are least frequent when on the right side, slightly more frequent when on the left side, and most frequent when on the back or the abdomen.

(c) *Character of postural changes.* Changes in the posture of the entire body are more frequent than any one kind of partial change in posture. They make up 43.2% of the recorded changes. Changes in the posture of one or both legs, other parts of the body remaining as before are next in frequency; accounting for 22.8% of the total. Arm movements alone make up 11.8% of the total and head movements alone make up 6.5%. Various combinations of leg, arm, and head movements not involving the trunk make up 15.8% of the total. Complete postural changes involving the entire body are most frequent when on the back, and make up 63.8% of all the changes from this position. Leg movements alone are less frequent in this than in other postures, accounting for only 9.6% of the changes. When on the abdomen, leg movements alone are more frequent (27.2% of all changes) and complete postural changes less frequent (27.2% of all changes) than in other postures. Head movements without other changes in posture make up 12.8% of the movements when on the back, and 12% of those occurring on the abdomen, but only 1% of those occurring on either side. Arm movements without other changes in posture are, however, most frequent when on the left side (20.1%), only slightly less frequent when on the right side (17.3%), but considerably less frequent when on the back or abdomen (5.3% and 3.8% respectively).

(d) *Serial changes in the length of rest-period.* The mean length of rest-period for all cases combined was 25.3 min. In order to see whether variations from this mean were distributed evenly over the entire sleeping period, or whether there was some constant trend in the frequency of movement as sleep continued, the mean length of each successive rest-period was computed. The results are shown in Table IV.

TABLE IV

MEAN LENGTH OF SUCCESSIVE REST-PERIODS

Rest Period	Mean Length	σ	Number of cases	Rest Period	Mean Length	σ	Number of cases
First	42.0	24.3	278	Sixth	19.8	13.4	27
Second	21.7	16.8	231	Seventh	9.1	5.3	7
Third	17.4	14.7	168	Eighth	10.8	6.5	4
Fourth	15.7	12.2	100	Ninth	37.0	15.0	2
Fifth	14.1	11.1	53	Tenth	8.0	—	1

It is evident from Table IV that movements tend to become more frequent as sleep continues. The falling-off in length of rest-period continues steadily up to the time of the fifth movement, after which the number of cases has become so small that the data are unreliable. Miles has recently shown that the mean hourly loss in weight through the insensible perspiration increases with the duration of sleep.[8] Our data suggest that this may be a function of increased activity during the later hours of sleep.

[8] W. R. Miles, Duration of sleep and the insensible perspiration, *Proceedings Soc. Exper. Biology & Med.*, 26, 1929, 577-580.

(e) *Diurnal and nocturnal sleep.* Mr. C. R. Garvey of the Institute staff is making a study of the frequency of bodily movements during night sleep, using the recording apparatus developed by Professor H. M. Johnson of the Mellon Institute. Ten of the children who had 5 or more nap-records also served as *Ss* in his experiment. For these *Ss* the rank-order correlation between mean length of rest-period during the nap and mean length of rest-period during the night was +0.43. This finding needs, however, to be confirmed upon a larger number of cases and with more adequate methods of determining movements during the nap. It is to be regretted that lack of space in the nursery school sleeping room has thus far prevented the installment of the Johnson beds.

The absolute findings from the two studies cannot be compared directly because of differences in the manner of recording. Taken at their face value, Mr. Garvey's mechanical records show somewhat more frequent movements during the night than were found in this study. Apart from the probability that in the observational study occasional minor movements were overlooked, the following factors may be expected to have contributed to the discrepancy: (1) the shorter length of the day nap, with the longer initial rest-periods making up a greater percentage of the total; (2) differences in the method of deciding upon time of falling asleep and time of awakening; (3) the fact that children are excluded from the nursery school for all conditions of poor health, including colds, thus eliminating occasions of unusual restlessness due to the child's physical condition, and (4) the fact that respiratory activities, such as coughing, sneezing, or heavy breathing, have been recorded separately in the present study and have not been classified as 'movements' unless accompanied by a change in posture. Most of these would be registered as movements on the mechanical apparatus.

(f) *Additional factors.* Tabulation of the reports on activities preceding sleep and on vocal and respiratory activities during sleep showed no clear relationship between these factors and either frequency of postural change or length of sleep. It must be remembered that these children were all in good health and free from colds when the records were taken. In 94% of all instances the children went to bed willingly. Some degree of motor restlessness preceding sleep is reported for 65% of the cases, vocal activity (chiefly murmuring to self) for 63%. Soporific habits (sucking fingers or clothing, or handling genitals) are confined largely to two or three individual children. Outdoor weather conditions show no clear relationship to sleep, but there was a small curvilinear relationship between room temperature and length of nap, the correlation ratios being 0.34 and 0.51. Extremely low and extremely high temperatures were alike unfavorable for duration of sleep. Under our sleeping conditions, the optimum room-temperature was from 54° to 58°F.

Summary

(1) The changes of posture occurring among a group of 56 children during a total of 278 nap-periods in a nursery school were observed and recorded on a prepared form. The number of naps observed for each child varied from 1 to 16.

(2) The total number of postural changes observed was 875 which is an average of one change in 25 min. during an average nap-period of 79 min.

(3) These children spent the greatest part of their total sleeping time on the right side. The left side was slightly less favored, the abdomen ranked next, the back lowest. The rank-order of the four postures is the same when initial posture rather than total sleeping time is considered.

(4) A comparison of the sleeping postures most favored by the individual children with scores on a comprehensive test of handedness does not bear out Sidis' assertion that the most right-handed individuals tend to sleep on the right side. No correlation was found between degree of dextrality and either percentage of total sleeping time spent on the right side or percentage of times the child went to sleep on the right side, but a positive correlation of 0.530 between dextrality and percentage of total sleep on the left side and of 0.338 between dextrality and percentage of times that the initial sleeping posture was on the left side was found. These correlations suggest that the slight general preference for the right side is the result of factors other than handedness, and that individuals in whom hand-preference is more strongly developed are slightly more likely than others to sleep on the side opposite to the preferred hand.

(5) Children whose postural habits during sleep are most uniform tend to fall asleep somewhat more quickly than those whose habits are variable. This suggests that kinaesthetic cues are operative to some extent in the induction of sleep.

(6) Changes in the posture of the entire body are more frequent than any single kind of partial changes in posture.

(7) Movements are least frequent during the early stages of sleep.

COMPARATIVE SUGGESTIBILITY IN THE TRANCE AND WAKING STATES[1]

By Clark L. Hull, Yale University and
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The hypnotic trance is generally regarded as a condition of heightened suggestibility as compared with the normal waking life. The difference between the two states is thus presented as essentially a quantitative one. Indeed, it may very well turn out that there is no very signficant difference between them except in the degree of suggestibility. It accordingly becomes a matter of some importance to compare in as exact and objective a manner as possible, the relative degree of suggestibility in the two conditions.

Subjects. The *Ss* employed in the investigations here reported were 8 students at the University of Wisconsin. A number of others were experimented on for longer or shorter periods but their results could not be used because, for one reason or another, they were unable to complete the experimental series. The *Ss* were put into the trance by the method of optical fixation, the fixation-object being the experimenter's eyes. The *Ss* had been hypnotized repeatedly previous to being used in the present experiment so that all went into the trance almost immediately at the command of the experimenter.

Procedure. The essentials of the method and apparatus by means of which the degree of suggestibility was measured in the present investigation, have been described in considerable detail elsewhere.[2] The *S* stands quietly erect, blindfolded. The *E* stands before him and repeats in a quiet, confident tone, "You are falling forward, You can't help yourself, You are falling forward, forward, forward," etc. continuously during the suggestion period. Under the conditions of the experiment, practically all the hypnotically susceptible *Ss* so experimented on will sway so far forward that they lose their balance both in the waking and trance states. The *E* ceases his suggestions the instant this loss of balance takes place, at the same time arresting the impending fall by pressing his hands against the *S*'s shoulders.

Quite without his knowledge, a pin with a tiny hook at its end is lightly caught into the fabric of the garment over the *S*'s right shoulder. To this pin

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[1]From the Psychological Laboratory, University of Wisconsin. This study was made possible by a grant from the Social Science Research Council. The writers are indebted to Mr. Wilbert S. Ray for assistance in tabulating the data.

[2]C. L. Hull, Quantitative methods of investigating waking suggestion, *J. Abn. Psychol. & Soc. Psychol.*, 24, 1929, 153-169. This article contains a diagramatic drawing of the apparatus employed.

is attached a thread which runs back about 4 ft. and over a simple, easy-running system of flanged aluminum pulleys. From this pulley system there is suspended by means of a second thread a small steel rod which moves in a vertical sleeve and to the lower end of which is attached a light stylus. By this system the horizontal postural movements of the *S* are traced by the stylus on the smoked paper of a kymograph as a vertically oscillating line, the oscillations in the tracing being reduced to one-third of their actual amplitude. One of these tracings, considerably reduced, is reproduced in Fig. 1.

The basic experimental procedure of the present investigation was to induce by suggestion two falls fairly close together in one of the two states and then two in the other states, with about 15 min. separating the second fall of the first two from the first fall of the second two. In any pair of falls, the second suggestion was given approximately 1½ min. after the fall resulting from the first suggestion. Every *S* served on four different experimental days. On two of these days the waking suggestion was given first and on two the trance suggestion preceded. We shall arbitrarily call the days on which the waking suggestion came first *A* and the days on which the trance suggestion preceded, *B*. The record reproduced in Fig. 1 shows a complete tracing made on an *A* day.

In order to minimize any constant errors which might result from the time factor, half of the *Ss* were experimented on in the *A,B,B,A* order and the other half in the *B,A,A,B* order. The experimental programs for the two groups of *Ss* is shown in detail in Table I.

TABLE I

THE DETAILED PROGRAM OF ADMINISTERING SUGGESTION FOR EACH OF THE TWO GROUPS OF *Ss*

Group	Type of Experimental Day	Ordinal Number of Experimental Day	Nature of State: First Suggestion	Second Suggestion	Third Suggestion	Fourth Suggestion
I	*A*	1	Waking	Waking	Trance	Trance
	B	2	Trance	Trance	Waking	Waking
	B	3	Trance	Trance	Waking	Waking
	A	4	Waking	Waking	Trance	Trance
II	*B*	1	Trance	Trance	Waking	Waking
	A	2	Waking	Waking	Trance	Trance
	A	3	Waking	Waking	Trance	Trance
	B	4	Trance	Trance	Waking	Waking

Criteria of degree of suggestibility. Two characteristics of the behavior of *Ss* in most suggestion experiments of this kind may be used as criteria of the degree of suggestibility. They are (1) the magnitude of the response and (2)

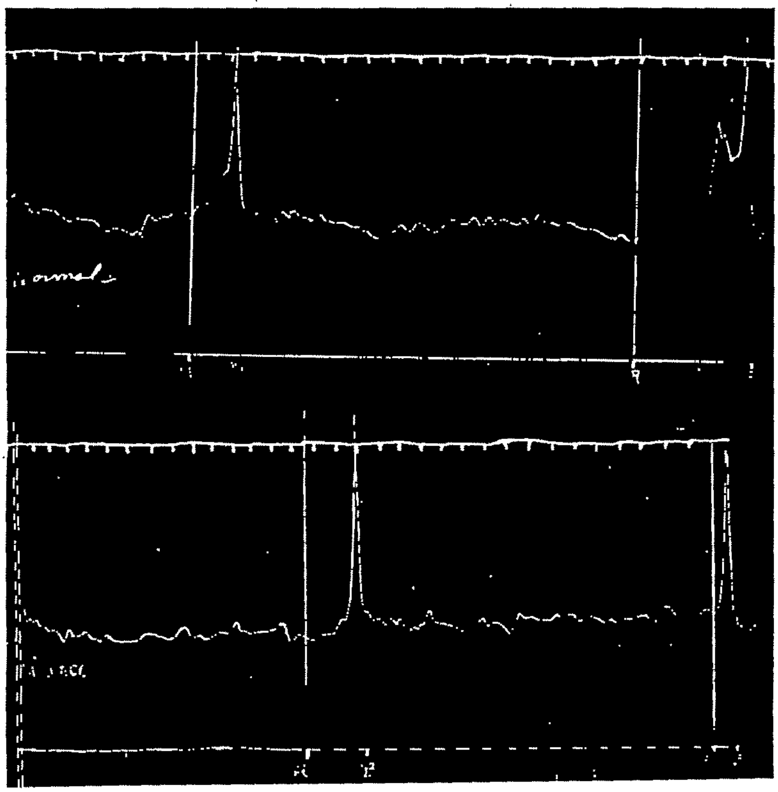

FIG. 1. TYPICAL RECORD FROM THE EXPERIMENT TO DETERMINE THE RELATIVE SUGGESTIBILITY OF NORMAL ADULTS IN THE TRANCE AND THE WAKING STATES

The upper line gives the time in 5-sec. intervals. The middle line shows the forward postural movements of the *S* in response to suggestion. These were traced automatically by a special device at one third their actual size. The suggestion was continued until the *S* actually fell forward and needed to be steadied by the *E*'s hands. The falls are shown in the tracing by the abrupt peaks. The lower line is for signals, showing the point at which suggestion begins (A) and approximately where it terminated (B). Vertical marks have been drawn across the time line at the beginning of the suggestion and at the point where the fall took place to facilitate the determination of the length of this period, which is taken as the measure of suggestibility.

the speed of the response. In general, the S is regarded the more suggestible the greater the amplitude of the response and the shorter the time required to evoke it. The presence of two distinct and more or less independently varying criteria of suggestibility often presents practical difficulties in scoring experimental results. Fortunately in the present investigation (with insignificant exceptions) the amplitude of the response of all Ss on all occasions was maximal, *i.e.* they swayed on all occasions so far forward that they lost their balance. With all Ss yielding a practically identical score as to magnitude, this amplitude criterion of suggestibility ceases to have any differentiating significance as between the two states being compared. This leaves the single criterion of time, which greatly simplifies the scoring situation. To this natural advantage is added the fact that the sharpness of the falling reaction as recorded by the apparatus used, removes practically all ambiguity in the determination of the time actually required to evoke the response.

The method by which the suggestion time is secured from the records may be best explained by referring to Fig. 1. The upper line is the time record, with a notch marking the lapse of every 5-sec. period. The second line is a record of the forward-backward postural movements, the sharp peaks representing the 'fall' or culmination of the response, after which the suggestion is discontinued. The lowest line records by means of notches the exact point at which the suggestive stimulation begins and approximately where it ends. The former is marked by 'A' and the latter by 'B.' After the record had been shellacked a perpendicular white line was drawn from 'A' across the time line. A second perpendicular white line was drawn across the time line from the point of maximum reaction. The distance between these two lines represents the suggestion time. Thus the first suggestion time of the record shown in Fig. 1 is 9 sec., the second is 26 sec., the third 12 sec., and so on. The suggestion times for all Ss are given in detail in Table II. Those of the record shown in Fig. 1 appear in the June 4th entry for S No. 6.

Results. We may now seek in Table II the answer to our primary question as to whether Ss in general are more suggestible in the hypnotic trance and if so, how much more. The procedure of the present experiment makes possible two fairly independent comparisons of the suggestion time in the two states. One is based on the time of the first waking suggestion as compared with that of the first trance suggestion. The other is based on the time of the second waking suggestion as compared with the second trance suggestion. It is evident, even to casual inspection of the concrete results of Table II that, despite occasional exception, the trance state shows a decidedly more rapid response to suggestion than does the waking state. This is shown with great distinctness by the arithmetical means of the contrasted columns of data. The average suggestion time of the first waking fall is 29.75 sec., whereas that of the first trance fall is only 11.97 sec.

TABLE II

Showing for every S of each Experimental Group the Suggestion-Time for every Trance and Waking State

Group	S No.	S Sex	Date	State during Sug. 1, 2*	Waking 1st Sug.	Waking 2nd Sug.	Trance 1st Sug.	Trance 2nd Sug.
					Suggestion-time during			
I	1	M	4-3	W	57	33	11	3
			5-14	T	9	11	3	3
			5-16	T	8	8	3	3
			5-25	W	8	7	5	2
	2	M	5-28	W	75	8	16	3
			5-29	T	6	6	9	5
			6-4	T	4	2	3	1
			6-5	W	2	1	3	3
	3	F	3-8	W	5	1	1	1
			3-15	T	8	8	8	7
			4-5	T	21	29	6	8
			4-8	W	13	13	7	5
	4	M	6-16 (12 M)	W	150†	150†	46	96
			6-16 (3 P.M.)	T	150†	74	86	17
			6-17 (10 A.M.)	T	61	30	39	16
			6-17 (11 A.M.)	W	100	64	23	17
II	5	F	5-13	T	20	16	6	9
			5-22	W	17	13	9	11
			5-24	W	18	22	13	16
			5-29	T	24	22	14	8
	6	M	6-3	T	4	4	7	3
			6-4	W	9	26	12	3
			6-5	W	8	4	2	3
			6-6	T	9	14	3	2
	7	F	3-13	T	7	5	9	4
			3-20	W	12	7	6	5
			3-27	W	7	6	7	3
			4-5	T	7	6	4	5
	8	F	3-13	T	6	4	4	2
			3-20	W	31	40	5	11
			3-27	W	79	41	9	13
			5-13	T	16	24	4	4
				Sums	951	699	383	292
				Means‡	29.75	21.84	11.97	9.12

*W = Waking; T = Trance. The state during suggestion 3 and 4 was of course the opposite to that indicated for 1 and 2.

†Values interpolated; S did not fall on these occasions.

‡[Editorial Note] The m.v. of these means are respectively as follows: 29.12, 18.30, 9.58, and 7.75.

or 40.2% as great. Again, the average suggestion time for the second waking fall is 21.84 sec. whereas that for the second trance fall is only 9.125 sec., or 41.3% as great. The close agreement of these two determinations from entirely distinct sets of measurements (40.2% and 41.3%) lends color to the view that if the experiment should be repeated, substantially similar results would be obtained. The ratios definitely confirm the prevailing view that the trance is a state of heightened susceptibility to suggestion and give a quantitative indication of its degree.

Corollary results. (a) *The cumulative effect.* Leaving the problem of our primary interest, we pass to the consideration of several others for which the experimental results furnish incidental solution. It was early observed that in the waking state the second application of suggestion during a given experimental period, is likely to evoke a distinctly more prompt response than the first.[3] The question at once arises as to how great this cumulative effect may be and whether it differs materially for the trance and waking conditions. The data in Table II offer a fairly direct answer to each of the two questions. The average suggestion time of 29.75 sec., for the first waking suggestion falls to 21.84 sec. for the second, the latter being only 73.7% of the former. In the case of the trance, the first suggestion time averages 11.97 sec. whereas the second one average only 9.125 sec., the latter being 76.2% of the first. The close agreement in the ratios obtained from the distinct sets of measurements is noteworthy. The indication is that under the conditions of the present experiment the suggestion time of the second of two suggested responses is reduced by approximately a fourth. There is no evidence that the trance differs at all in this respect from the waking condition.

(b) *The perseverative influence.* The evidence which has just been presented that there is a perseveration of the influence of responding positively to one suggestion upon the response to another suggestion in the same state, raises the further question of whether there may not also be a preseveration of the influence of a preceding trance state and the suggested activity which took place during it, upon the waking suggestion administered following it. It is a well-known fast that an S goes into a trance much more readily at once after waking from a previous one than when the two trances are separated by a longer period. The experimental results of the present investigation offer two separate sets of measurements bearing on this question. It will be recalled that half of every S's waking suggestions preceded the trance suggestions and half followed. It is only necessary to tabulate in separate columns the two sets of waking suggestions shown in Table II and find their means. This procedure shows that the average time for the first waking suggestion when preceding the trance is 36.94 sec. whereas when following the trance the time averages only 22.5 sec. which is only 60.9% as great. When the second waking suggestion precedes the trance it averages 27.25 sec. whereas when it follows the trance it falls to 16.425 sec., which is 60.3 as great. The ratios from the two independent sets of

[3]Hull, *op. cit.*

measurements are in excellent agreement (60.9% and 60.3%). The indication is that the influence of the trance, possibly augmented by the activity which took place during it, persists very strongly into a following (supposedly) waking state even after a period of approximately a quarter hour.

(c) *The practice effect.* Still another question demands our attention. If the influence of responding to one suggestion shows such a profound effect upon the response to a closely succeeding suggestion, may we not expect a certain amount of persistence over into the next day? May there not, indeed, be a kind of practice effect analogous to the ordinary curves of learning? It is an extremely easy matter to re-tabulate the data of Table II so as to secure averages from which a composite practice curve may be plotted. The mean

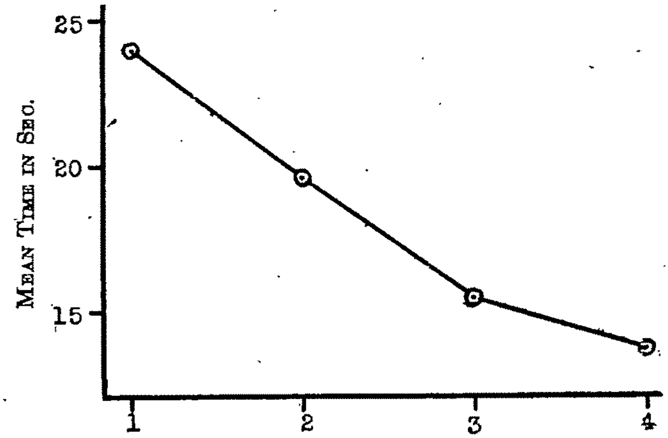

Fig. 2. Composite Graph Showing Habituation Effects in Responding to Suggestion

was found of the 32 suggestion times made up by pooling all four suggestion times of the first experiment period of every one of the 8 *S*s. This mean was found to be 24.03 sec. The mean for the second period was 19.6 sec., that for the third period was 15.4 sec. and that for the fourth was 13.7 sec. These results are shown graphically in Fig. 2. It is evident that we have here a very marked practice effect, the average time for the fourth period being but little over half that required for the first period.

Summary and Conclusions

The results of the present investigation may be summarized as follows.

(1) The type of suggestion employed in the present study shows a trance response time approximately 40% that of the waking response time. Stating the matter the other way round, waking suggestion requires something like 2½ times as much

time as trance suggestion. This confirms in an objective and quantitative manner the somewhat vague and uncertain impression obtained from casual observation that the hypnotic trance is a state of heightened suggestibility. Whether the above ratio will hold for other forms of suggestion can only be determined by specific experiment but presumably it will vary more or less with the form of behavior suggested as well as with other circumstances.

(2) If a second suggestion is begun within two minutes or so after the termination of the reaction to a previous suggestion, and in the same state, the suggestion time of the second is reduced about one fourth. The waking and the trance states appear not to differ at all in this respect.

Quite apart from the significance of this fact as an established relationship, is its possible bearing on the nature of the hypnotic trance. Presumably the principle is very general. Having responded to one suggestion, the organism is at once in a state of heightened susceptibility to further suggestion. According to this line of reasoning, the hypnotic trance becomes only a special case of the general law. According to the usual procedure for inducing the trance, the closure and ultimate catalepsy of the eyelids, along with certain related phenomena, are first evoked on the basis of waking suggestion. In harmony with the above independently established principle, these responses to waking suggestion would naturally produce a state of heightened susceptibility to further suggestion which is, in fact, such a conspicuous characteristic of the trance state. In the above experiment the response to the waking swaying suggestion reduced the suggestion time about 25% and the response to the waking suggestion to close the eyes etc., in the process of trance induction reduced it about 60%.

(3) The influence of the trance, possibly combined with the additional suggestions given the *S* while in it, appears to persist rather strongly after waking. Waking suggestions given a quarter of an hour after waking from a trance require only about 60% as much time to evoke maximal reactions as when not preceded by a trance.

(4) Response to suggestion shows a marked practice effect from period to period, the mean suggestion time for the eight *S*s on the fourth period being only a little over half that for the

first period. Unfortunately the present experiment was not set up primarily to solve this problem and the time intervals between the experimental periods vary considerably. As a consequence the exact quantitative values must be accepted with some caution but of the general tendency to practice effects there can be no doubt. It has long been observed as one of the most conspicuous characteristics of successive trance inductions that, as a rule, every time an *S* goes into the trance the process becomes more rapid. Here again our experimental results furnish objective and quantitative confirmation of casual observations.

ANOMALIES OF VISUAL ACUITY IN RELATION TO INTENSITY OF ILLUMINATION

By Ellis Freeman, University of Louisville

In papers recently published I gave an account of several inexplicable phenomena connected with the determination of visual acuity.[1] The aim was to present these from the point of view of psychology. Only a descriptive treatment was advisable, however, because the phenomena were so stubbornly anomalous that they resisted all efforts at explanation and reconciliation with the classical point of view. In this article I propose to examine these phenomena, together with some newer experimental data, in relation to the alleged physiological processes involved. Specifically my purpose is to determine the bearing of these data upon the conclusions of Hecht regarding the relationship between visual acuity and intensity of illumination.[2]

In Hecht's account, the intensity of illumination determines acuity by setting in operation a different number of retinal elements for each intensity within a unit of retinal area. He assumes sensitivity to vary among the individual receptors in accordance with the statistical distribution curve, just as any individual trait varies within an unselected homogeneous population. This means, then, that within any unit of retinal area (consisting of 540 elements) a small number will have a low threshold, a greater number a medium, and a small number again a high. With a retinal image of low intensity only the receptors with a low threshold will be capable of reacting. As the intensity rises, and the higher thresholds are reached, progressively more receptors will become operative. The increase in the number of functioning elements corresponds to the increase in visual acuity which is experienced with a rise in the intensity of illumination of the stimulus. When the light intensity has been so far increased that the thresholds of all receptors have been passed, and all the elements are therefore in operation, maximum acuity has been attained. Further increases in the intensity of the stimulus will not produce a heightening of acuity. Acuity varies, then, only with the number of receptors in operation, which is to say, with the intensity of illumination so long as the maximum has not been exceeded. This statement is, therefore, unequivocal with regard to two propositions: (1) that *acuity varies only with the number of receptors functioning within unit retinal area*; and (2) that *this number varies only with the intensity of the retinal image*. The two propositions are not independent; rather each is a necessary consequence of the other.

My experimental approach to Hecht's propositions is based upon the following considerations. If intensity and area of retinal image be kept con-

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[1]Ellis Freeman, What does a test of visual acuity measure? *Arch. Ophth.*, 2, 1929, 48-46. Anomalies of peripheral visual acuity, *J. Exper. Psychol.*, 12, 1929, 324-340.

[2]Selig Hecht, The relation between visual acuity and illumination, *J. Gen. Physiol.*, 11, 1928, 255-281.

stant, no matter by what means, for a series of observations, then for each observation the number of functioning receptors ought to be identical, and, as a result, the acuity ought in all cases to remain constant. The data which follows here are derived from experiments in which precisely constant intensity and area of retinal image have been secured from variable external conditions of stimulation. The resulting acuities, if Hecht's propositions are correct, should all have been equal. They were, however, found to be widely divergent. But before the significance of these facts can appear, the experiments themselves must be examined.[3]

Visual acuity was tested by the translation method. If, monocularly and with appropriate eye-fixation, the observer be shown a row of letters, each of size *a*, by instantaneous exposition, on any retinal meridian, with the eye at distance *b* from the point of fixation, the letters may be recognized (resolved) as far out into the periphery as the distance *c*. If, now, under the same conditions, except that the letters have been increased to 10 *a*, and the distance of the eye from the fixation-point to 10 *b*, a similar instantaneous exposition be made, the observer will be unable to recognize the letters as far out into the periphery as 10 *c*. Contrary to the requirements of geometrical optics, he will be able to read only as far out as 5 *c* or 6 *c*. Despite the fact that the physical conditions were designed to give, and as far as is known did give, identical retinal stimulation for both expositions (*e.g.* identical time of exposition and intensity of illumination, and the same visual angle for the individual letters), a contraction of the field of vision occurred in the case of the larger constellation. Only when the letters of the large constellation were further enlarged, disproportionately to the distance from the eye, could the peripheral recognition be extended to 10 *c*. But with such an enlargement of the letters there arises an inconsistency with the principles of geometrical optics, for then a greater retinal area is being stimulated in order to elicit the same degree of clearness or acuity as before in the case of the small constellation. In terms of acuity, this means that a reduction has occurred without a concomitant reduction of intensity of illumination.[4]

Peripheral acuity was again tested under continuous illumination. At right angles to the line of regard a white cardboard, travelling in a vertical slide and bearing two black squares separated by a white interval equal in width to the side of a square, was shifted up and away from the fixation-point until the observer just ceased to resolve the squares as two. The dimensions of the stimulus and the distance of the eye from the fixation-point were then increased ten-fold. Again in this experiment, resolution of the large constellation occurred at a lesser point of eccentricity than that of the small constellation. The peripheral limit of the large was, for example, 6 *c* from the fixation-point instead of 10 *c*. If the large squares were increased sufficiently, disproportionately to the distance, or the distance were decreased sufficiently, disproportionately to the size of the squares, the resolution could be extended eccentrically to a point 10 *c*.

[3]A more detailed description of the experiments themselves and of their background is to be found in the articles mentioned under the first footnote.

[4]Hermann Aubert, *Physiologie der Netzhaut*, 1865, 235-253; E. R. Jaensch, Zur Analyse der Gesichtswahrnehmungen, *Zsch. f. Psychol., Erg.-Bd.*, 4, 1909, 1-26.

But here, as in the observations with the letters, a greater retinal area would be stimulated than in the case of the small constellation. This would mean again a reduction in acuity without a concomitant reduction in the intensity of illumination. Curve 1 (Fig. 1) represents in principle this *decrease* in acuity which unaccountably accompanies each increase in the distance of fixation. Clearly this is contrary to the inferences of geometrical optics. This anomalous decrease of acuity with the larger constellations has been given the name of its discoverers, and is called the Aubert-Foerster phenomenon.

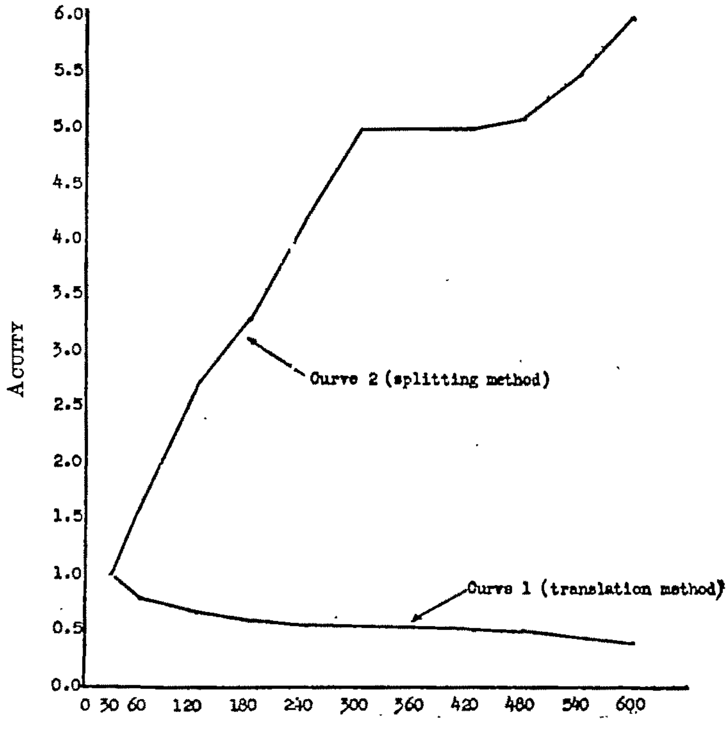

FIG. 1. RELATION BETWEEN ACUITY AND FIXATION DISTANCE BY THE METHODS OF TRANSLATION AND OF SPLITTING

EXPERIMENTAL PROCEDURE

(*1*) *Acuity by the translation method of testing.* To determine the data for Curve 1 (Fig. 1), I repeated the preceding experiment by employing black squares, similar to those of Aubert and Jaensch, in substantially the same apparatus under constant intensity of illumination with continuous exposition. The distance of the eye from the fixation-point was 30, 60, 120, 180, 240, 300, 360, 420, 480, 540, and 600 cm. in turn. Sometimes the observations began at one end, sometimes at the other, in order to eliminate the effects of practice

and of fatigue. Corresponding to these distances, the dimensions of the black squares were 1, 2, 4, 6, 8, 10, 12, 14, 16, 18, and 20 cm., respectively. Obviously each square taken with its appropriate fixation-distance must have produced a retinal image of the same area as that of any other square with its appropriate fixation-distance. As the illumination of the campimetric exposition surface was constant, all the retinal conditions (except those conceivably due to differences in accommodation and pupillary width, which will later in this paper be demonstrated as indifferent) were in every case the same. Observations were made on the upper half of the vertical meridian of the field of vision (lower half of the vertical meridian of the retina) of the left eye. Table I, column 2 shows the vertical distance of the limit of resolution for each fixation distance.

TABLE I

SHOWING IN CM. THE DISTANCE OF THE EYE FROM THE FIXATION-POINT, THE ACTUAL VERTICAL DISTANCES OF THE STIMULI AT THE LIMIT OF RESOLUTION, THE THEORETICAL VERTICAL DISTANCES OF THE STIMULI AT THE LIMIT OF RESOLUTION, AND THE EMPIRICALLY DETERMINED DIMENSIONS OF THE STIMULI

(Actual dimensions are the averages of 4 determinations, the mean variations being negligible)

Distance from fixation	Actual vertical distance	Theoretical vertical distance	Empirical dimensions of stimuli
30	10	10	1.0 × 1
60	15	20	1.3 × 2
120	24	40	1.6 × 4
180	36	60	1.7 × 6
240	44	80	1.8 × 8
300	55	100	1.8 × 10
360	68	120	1.8 × 12
420	74	140	1.9 × 14
480	78	160	2.0 × 16
540	80	180	2.2 × 18
600	81	200	2.5 × 20

The data of columns 2 and 3 merely confirm the results of Aubert and of Jaensch. For our purpose a further step was necessary, for which the values of column 4 were computed. This further step was the introduction of a significant variation, which constitutes a critical test of Hecht's propositions. Applying the formula T/A (in which T equals the 'theoretical vertical distance,' and A the 'actual vertical distance'), I calculated the factor representing the number of times each of the squares would have to be increased linearly in order to be resolvable at the corresponding theoretical vertical distance. Obviously the reciprocal of this factor represents the acuity for each constellation. If each of the squares be now enlarged, as they were enlarged, in accordance with this empirical factor, and resolution should occur at the theoretical vertical distances given in Table I, column 3, then this calculated acuity

will also represent the actual acuity. In accordance with these calculations, stimuli squares were prepared with the dimensions given in Table I, column 4, and presented at the corresponding fixation-distances. Other conditions remained exactly as before.

With these stimuli of a size empirically determined, the resolutions now occurred at the theoretical points of eccentricity (based upon geometrical optics) given in column 3 of Table I. These facts mean, then, that for an apparently unaccountable reason, the ultimate retinal image on a given spot of the periphery must be progressively increased in order to elicit the same degree of acuity, when supposedly irrelevant changes are made in the external conditions. This supposedly irrelevant external condition is an increase in the size of the constellation, *e.g.* increase of fixation-distance and proportional increase of stimulus. Such a reduction in acuity is contrary to geometrical optics. It leads also, since intensity of illumination had remained constant, to the conclusion that the variations in acuity are here independent of the intensity of illumination. This is contrary to Hecht's propositions. This progressive decrease in acuity, representing a progressive increase in the retinal image, as the fixation-distance increases, is represented by Curve 1 of Fig. 1. A comparison of the readings on any two points selected on the curve will make its significance clear. Since acuity at 30 cm. fixation-distance is the standard, it may be represented as 1/1 (in ophthalmologic terms, 6/6). But, by contrast, at 600 cm. fixation-distance, acuity has dwindled to 4/10 (6/15). Obviously a 250% reduction in acuity cannot be properly attributed to faults in experimental technique.

A similar but less marked condition was discovered by both Jaensch[5] and Jacobsson[6] in testing foveal acuity with figures and letters of different sizes at proportional distances from the eye. The large, compared to the small constellations, always elicited a lower degree of acuity. They had to be brought closer to the eye than their size warranted, and so produced a larger retinal image, before resolution could occur. The conditions so far described are tantamount to a declaration that visual acuity is not a constant factor for a given individual, nor is it necessarily constant when all the retinal conditions remain unchanged. If the acuity of the observer for the preceding tables be taken as standard at 30 cm. fixation-distance, and called 6/6, then at 480, for example, it will be 6/12, and at 600, 6/15, etc. The essential fact is that this marked and progressive *decrease* in acuity will have occurred without a concomitant reduction in the intensity of illumination.

(*2*) *Acuity by the splitting method of testing.* It is possible to test the visual acuity of the periphery by still another method than the preceding one. The observer fixates a point at eye-level on a campimeter, where, contrary to the arrangement in the translation method, the double stimulus objects (black squares or lines) which are to be resolved are not moved together, but are presented at a fixed angle of eccentricity. The data which follow here refer to a point at 5° of eccentricity, in the upper half of the vertical meridian of the field

[5] Jaensch, *op. cit.*

[6] Malte Jacobsson, Über die Erkennbarkeit optischer Figuren bei gleichem Netzhautbild und verscheidener scheinbarer Grösse, *Zsch. f. Psychol.*, 77, 1916, 1-91.

of vision (lower half of the vertical meridian of the retina) of the left eye. Data of a similar character may be secured at different angles and on different meridians. One of the two stimulus objects remains stationary on the campimeter surface, while the other, at first contiguous with it, is slowly and regularly shifted away on the campimeter surface until the white interval has increased sufficiently to permit the two objects to be seen as separate (to be resolved). By this method the acuity is inversely proportional to the width of the required white intervals.[7]

The same fixation-distances as in the preceding experiment were employed here, 30–600 cm., with stimuli proportionately enlarged and running from 1–20 cm. The retinal images of the black stimulus objects were therefore identical in area throughout the series; and as the illumination of the campimeter remained unchanged, the intensity of the retinal images also remained identical throughout the series.

TABLE II

SHOWING THE DISTANCE OF THE EYE FROM THE FIXATION-POINT, THE ACTUAL WIDTH OF THE WHITE INTERVAL NECESSARY FOR RESOLUTION, AND THE THEORETICAL WIDTH OF THE SAME

(Values in column 2 are each the average of 4 observations, the mean variation being negligible)

Fixation distance (in cm.)	*Actual* width (in mm.)	*Theoretical* width (in mm.)
30	1.8	1.8
60	2.2	3.6
120	2.7	7.2
180	3.2	10.8
240	3.4	14.4
300	3.6	18.0
360	4.4	21.6
420	5.0	25.2
480	5.6	28.8
540	5.9	32.4
600	6.0	36.0

From this data Curve 2 of Fig. 1 can be plotted to represent the progressive *increase* in acuity for the same retinal area, with illumination constant, as the fixation distance increases. It is merely necessary to apply the formula T_w/A_w (in which T_w equals the theoretical width of the white interval, and A_w equals the actual width) in order to plot the acuity values.

Curve 2 of Fig. 1 shows that by this different method of testing, a new phenomenon, opposite to that of Aubert-Foerster, is introduced. As fixation-distance now increases, the retinal image necessary for resolution correspondingly decreases. Contrary to the results which represent the Aubert-Foerster phenomenon, acuity by the splitting method increases with distance of fixation (although not in direct proportion, as Curve 2 shows). This is tantamount to a

[7]Full details concerning the method are given in the second article mentioned in the first footnote.

declaration that again acuity is not constant for a given observer, nor is it necessarily constant when all retinal conditions remain unchanged. The curve shows, for instance, that at 30 cm. fixation-distance, acuity is 6/6; at 480, 6/1.2; and at 600, 6/1. The essential fact is that this enormously marked and progressive *increase* in acuity will have occurred without a concomitant increase in the intensity of illumination.

Sources of Experimental Error

(*a*) *Influence of accommodation changes in crystalline lens on acuity.* Jaensch[8] has satisfactorily shown by means of adequate experimental devices that Heinrich's[9] explanation of the Aubert-Foerster phenomenon as an artifact and a consequence of the physiological limitations of the crystalline lens is incorrect. But even if Jaensch had not demonstrated this fact, the internal evidence of the two sets of acuities in the two curves of Fig. 1 would show it immediately. A comparison of the acuity values secured by the two methods would prove that changes in accommodation cannot be the cause of these anomalies. A simple consideration is significant. At 600 cm. Curve 1 shows acuity to be 6/15, by the translation method. At the same distance, Curve 2 shows acuity to be 6/1, by the splitting method. Between the two there is a 1500% difference in acuity. But since for both the fixation-distance was the same, 600 cm., the accommodation must necessarily have been the same and so could not have caused that which demands a change in accommodation. Furthermore, for Curve 1, acuity *increases* with accommodation (that is, with a decrease in fixation-distance); for Curve 2, acuity *decreases* with accommodation (that is, with a decrease in fixation-distance). To argue that accommodation was the determinant would be a contradiction, for how could it increase as well as decrease acuity?

(*b*) *Influence of width of pupil on acuity.* Pupillary width varies with accommodation and with intensity of illumination. Since intensity of illumination was constant within each series and for the two methods as well, the width of the pupil must of necessity have remained constant, at least as far as the factor of illumination was concerned. What remains is the possibility that the variations in pupillary width that accompany changes in accommodation might have caused the anomalies. But here again the same simple consideration as that upon which the preceding paragraph is based will show that this cannot have been the case. For in Curve 1 a *decrease* in acuity accompanies an increase in pupillary width, while in Curve 2 an *increase* in acuity accompanies the same increase in pupillary width. This presents an irreconcilable contradiction to those who might hold the pupil responsible, for how could a pupil of given size once cause high acuity and another time low?

(*c*) *Influence of air layer on acuity.* The influence of an air layer 600 cm. thick is probably negligible. But the same simple consideration of the discrepancy between the two methods demonstrates that the air layer can have had no bearing on the results here presented. In one case a thick air layer accompanied a low acuity, and in another a high.

[8] Jaensch, *op. cit.*, 19-26.

[9] Wilhelm Heinrich, Die Aufmerksamkeit und die Funktion der Sinnesorgane, *Zsch. f. Psychol.*, 9, 1896, 342-388; 11, 1896, 410-431.

Conclusions

Hecht has made two fundamental propositions, first that acuity varies only with the number of receptors functioning within unit retinal area; and secondly, that this number of functional receptors varies only with the intensity of illumination on the unit area. It therefore follows that acuity depends directly upon intensity of illumination, so long as this remains less than maximum. An experimental verification should therefore show that if intensity of illumination and area of retinal image be kept constant, by no matter what external means, acuity will remain constant also. Experiment contradicts this not only with one but with two sets of data, each of which is independently sufficient for the denial of Hecht's propositions. The Aubert-Foerster phenomenon and that of opposite character give variable acuity values for identical conditions of retinal image and illumination. Thus, by varying a single factor, the size of the constellation, I have produced differences in acuity, which, if Hecht's propositions were valid, could have been produced only by variations in the intensity of illumination.

The experimental evidence is coercive, and the two propositions of Hecht must therefore be denied. Acuity does indeed vary with intensity of illumination, but only when the fixation-distance is constant. But as acuity varies with fixation-distance in such a radical manner, intensity of illumination and the resulting number of receptors rendered operative cannot be the fundamental determinants. Certainly they cannot be the sole determinants as Hecht suggests. The fundamental determinant of acuity must be an at present obscure complex factor, in which intensity can be no more than one of several components.

THE RELATIONSHIP BETWEEN MUSCLE TENSION AND MUSCLE THICKENING[1]

By Raymond Dodge and Roland C. Travis, Yale University

There are two main reasons for recording the muscular action of intact humans from muscle thickening rather than from movements of limbs. Such records avoid the delay incident to the inertia of the limb and the distortion incident to its momentum. For example, records of the knee jerk taken from the moving leg utilize a recording lever whose weight distorts the curve of muscle contraction, delays its onset, smooths out its finer variations and slows down its main deflections.[2] Such a recording lever is an obvious absurdity in any technique that aims at exactitude. A second reason for recording indirectly instead of by limb movements is that the latter give imperfect information of the interaction of antagonistic muscle groups. Since it is impracticable to extirpate human muscles for experimental purposes, the systematic study of voluntary human behavior requires some such indicator as muscle thickening to record the action patterns of the several muscles involved.

There are, however, several difficulties in this procedure. Notwithstanding the probability of high correlation between muscle thickening and muscle pull the exact relationship and its limitations and sources of error have not been empirically determined. Movement of the limb has a mechanical effect on muscle thickening, so that the technique of recording from muscle thickening commonly requires a rigid stabilization of the supporting limb and the mobile member against which pull is exerted. Moreover, all highly magnified records of muscle thickening are superimposed on a variety of muscle changes including pulse, respiration, and tonus. Finally, the muscle thickening corresponding with any given pull probably varies from individual to individual and from muscle to muscle.

The present report represents experiments on a single muscle group of a single subject. Thickening of the quadriceps group was photographically recorded in the following manner. The leg and thigh were stabilized as far as practicable with the exceptions to be noted. A horizontal light wooden lever resting on the muscle carried on its axis a small convex mirror of suitable curvature. At a distance of 15 cm. from the axis an off-set approximately 5 mm. in diam. was pressed into the mid-quadriceps by a constant force. The recording camera stood 60 cm from the axis giving an optical magnification of muscle thickening of $(60 \div 15) \times 2$. This was approximately the limit of magnification imposed by the width of our sensitive paper. All measurements were taken from the right thigh and right leg of a sitting subject, with the leg flexed

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[1]From the Psychological Laboratory, Institute of Human Relations.

[2]Raymond Dodge, A systematic exploration of normal knee jerk, its technique, the form of the muscle contraction, its amplitude, its latent time and its theory, *Zsch. f. allg. Physiol.*, 12, 1910, 1-58.

at approximately 90° to the thigh. The shoe on the right foot rested against a stop on a platform which was rigidly attached to the bottom of a pendulum whose axis was in line with the axis of the leg at the knee. An electric circuit including a Becker marker was broken at the least movement of the pendulum by means of a contact at the back of the platform. The pendulum was held against the contact stop by the pull of various known weights. As the muscular tension was increased from relaxation, the thickness gradually increased until at some point the muscle tension just overbalanced the weight. The quantitative relationship was given by the thickness of the muscle at this point which was indicated by the movement of the Becker marker. In reading the records, movement of the marker was projected across the record of thickening and the latter was read at that point. The part of the excursion antecedent to movement of the leg was regarded as the magnified muscle thickness corresponding to the muscle tension which was just sufficient to lift the weights.

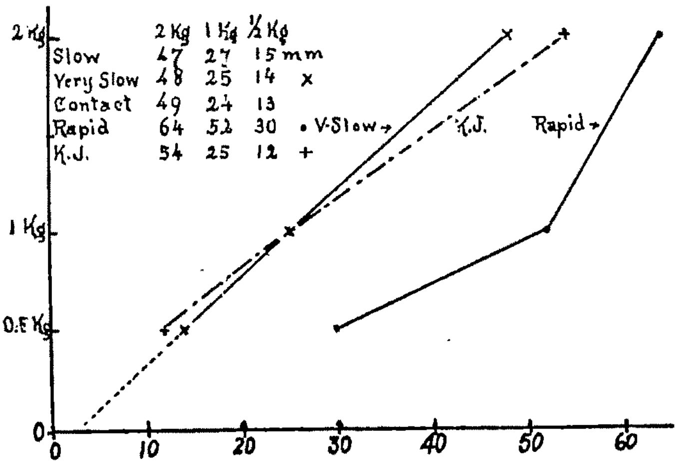


FIG. 1. SHOWING RECTILINEAR RELATIONSHIP BETWEEN QUADRICEPS THICKENING AND THE FORCE OF QUADRICEPS CONTRACTION

Our exploratory measurement included three kinds of behavior as follows: (1) slow contraction and relaxation just sufficient to break and make the pendulum contact; (2) the knee jerk evoked by a pendulum of known weight and velocity striking the patellar tendon; and (3) quick voluntary contraction.

Thickening data corresponding to all three of these situations are plotted in Fig. 1. The abscissa represents the average significant excursion of the recording line in mm. The ordinate represents the several weights and in our experiments the force of muscle tension just sufficient to lift them. The data from which the graphs of Fig. 1 were drawn show that within the limits of our exploration there was a rectilinear relationship between quadriceps thickening and the force of quadriceps contraction as measured by the capacity of the leg to lift weights against gravity when the cocontraction of antagonistic muscle was minimal. While extension of the line does not pass exactly through zero the error is not large. This error may and probably does include the friction

and inertia of the moving system and slight cocontraction of antagonistic muscles. The difference between the curves of slow and those of rapid contraction is quite marked. Some difference was expected on the basis of the records of Dodge and Bott,[3] which showed regular cocontraction of antagonistic muscles at the beginning of all rapid voluntary extensions of the leg. The discrepancy between slow and rapid contraction probably approximated the effect of antagonistic contraction in rapid extension. The slight but consistent difference between the slope of the curves of slow contraction and the knee jerk may possibly also be understood, in part at least, on the same basis. All available records show that there is a delay in the contraction of antagonistic muscle in the knee jerk which practically frees the beginning of the knee jerk from antagonistic cocontraction.

On the basis of this exploration of the relationship between muscle thickening and the muscle strain we suggest the following generalization. In a stabilized limb isometric quadriceps thickening increases in direct proportion to muscle work from zero to strains near the upper limit of the strength of the muscle.

The possibility of using muscle thickening as a quantitative indicator of muscle strain, irrespective of the muscle studied, depends on the approximately rectilinear relationship between the two under most favorable conditions from almost maximum contraction to zero. For purposes of quantitative estimate of strain with a small probable error, it should suffice to determine two points on the line as accurately as practicable for each muscle and experimental setting and read the approximate muscle tension from interpolated thickening values.

[3]Raymond Dodge and E. A. Bott, Antagonistic muscle action in voluntary flexion and extension, *Psychol. Rev.*, 34, 1927, 241-272.

APPARATUS

An Improvement in the Construction of Electrodes

By Alvhh R. Lauer, Ohio State University

The author recently described an electrode for the galvanic skin reflex.[1] A demand from other laboratories brought up the question of reproducing the original device. Instrument makers were disturbed over the size of glass funnels available and the inconvenience of cutting and grinding the stock. It was also found that the quality and size of bakelite required is not always available, and delays from special orders occur. Further, it is often desirable to have several pairs of electrodes on hand for different experimental purposes and to have them of different sizes for varying the area covered, region of application, etc. The size especially was difficult to change with the type previously used. Another inconvenience of the first model was the necessity of inserting the electrolytic compound before attaching the apparatus to the *S*.

For the above reasons a study was made of possible alternative materials which would facilitate construction of an improved model.[2] Glass requires an expensive steel die for moulding, and small quantity production would render the fabrication impractical. Pure bakelite is hard to procure in the proper sizes. Consequently porcelain was investigated and found to possess none of the difficulties met in other materials. It is, moreover, cheap, and thoroughly reliable; and it can be molded into any form or size, and produced in any ceramics laboratory.

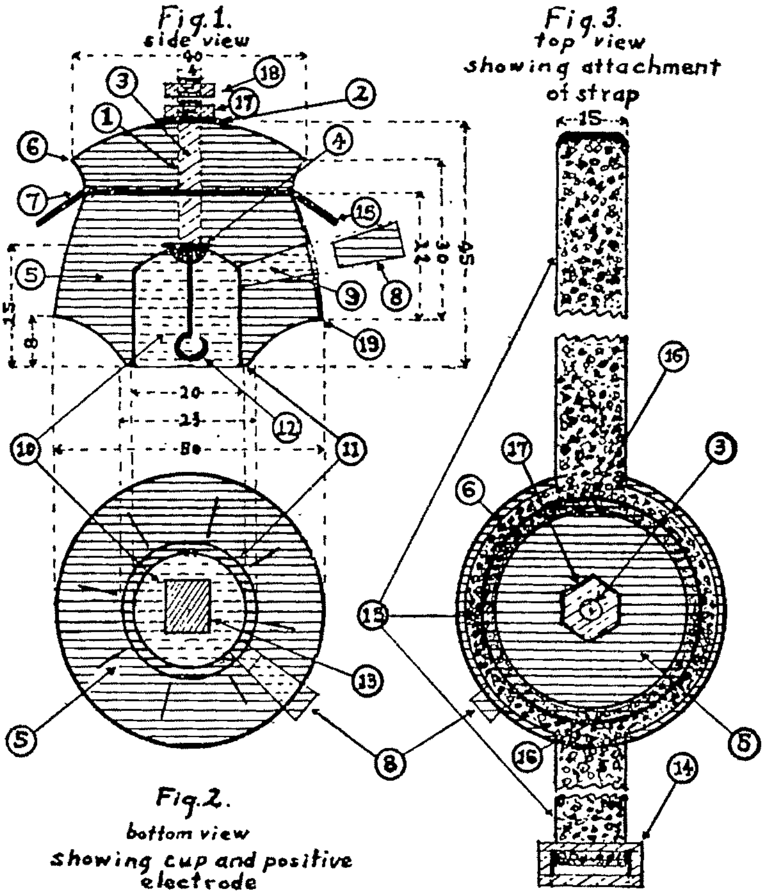
Accordingly the following electrode was designed by the author and constructed by ceramic engineers.[3] The cup (5) is made of an electrical porcelain body such as used for high-tension insulation. A blank composed of 32% felspar, 30% ball clay, 22% China clay, and 16% flint was turned to the size and shape desired. The necessary holes (1) and (9) were also made in the wet clay. A coating of a typical electrical porcelain glaze was then applied. The material is absolutely vitreous, and when fired to Cone 12, 1370°, it comes out with a brown glaze. When heavily constructed as shown in Fig. 1, there is no danger of warping during the firing process. The lip (11) projects down, to fit into the hand or other area of application. A 4-mm. hole (1) through the top allows the insertion of a machine screw (3) to which are attached the

[1] A. R. Lauer, A new type of electrode for the galvanic skin reflex, *J. Exper. Psychol.*, 11, 1928, 248-251.

[2] The opportunity for developing the apparatus described in this paper was made possible by a National Research Council Fellowship which the writer holds under the direction of Dr. A. P. Weiss at The Ohio State University. It represents a phase of the analysis of integrated responses in which refined techniques for determining the relationship of implicit and explicit reactions are being studied.

[3] Through the courtesy of Professor Arthur S. Watts, head of the Department of Ceramic Engineering at the Ohio State University.

electrodes proper (12) and (13). Instead of attaching lugs to the side for straps, a groove (7) is turned in the wet clay. The strap (15) is split sufficiently to allow the smaller part of the electrode (6) to slip through and into the groove (7). A leather strap will stretch enough to allow its being attached more or less permanently. When webbing is used, it may need to be reënforced at the cut ends by sewing slightly at (16) to avoid tearing and becoming de-



FIGS. 1-3. SHOWING CONSTRUCTION OF THE PORCELAIN ELECTRODE

tached. In order to insert the contact medium after fastening to the S the hole (9) is made to fit a No. 0 cork or rubber stopper (8). The aperture is 8 mm. at the external end and tapered to 5 mm. at the internal end. The other dimensions given above in Fig. 1 are almost identical with those of the previously described electrode except that the lip (11) is extended 8 mm. rather than 5 mm, the distance which the glass cup projected out of the bakelite.

(Throughout the units are millimeters.) This prevents leakage from restricted contact. By using a large pipette the amount of liquid inserted can easily be standardized. The size is variable, and the cross-section of the cup can be made smaller or larger as desired. The projection of the lip (11) should not be much less than 8 mm. and the main body of the electrode (5) very little smaller than shown in Figs. 1, 2, and 3 for best results. It may be made as large as the location of application will permit. A large amount of material was used in the main body of the electrode to make it especially durable.

Other parts not named in the description are: nuts (17) and (18) for holding screw (3) and connection in place. Rubber washers (2) and (4) are placed at each end to seal the screw (3) in the hole (1). The shoulder at (19) stabilizes the position of the electrode. Fig. 3 shows how the strap is split and slipped over the enlargement (6). In this way it is thoroughly insulated from the brass connection at (3), and no possible leak across a damp strap can occur. The function of the slip buckle (14) is to make sensitive adjustment. A rubber sponge under the strap makes the apparatus much more comfortable.

It is alleged that polarization is a factor in the galvanic reflex.[4] Whether the magnitude is sufficient to be of consequence is an open question. Spinney maintains that increasing the size of the positive electrode will delay polarization.[5] To compensate for this effect one of the electrodes (12) is made with a platinum No. 30 wire 2 cm. long, while the other (13) is made of a thin platinum plate 1 cm. square attached to a similar wire 1.5 cm. in length. By connecting the source of current (when using the Féré method with a galvanic current) such that the plate is the positive electrode the effect should be considerably delayed. The time and amount of delay has not been determined.

This electrode has all the advantages of the earlier model, and many of the disadvantages have been eliminated.[6]

The Measurement of the Latent Time of Electric Light Bulbs*

By W. N. Kellogg, Indiana University and
Roland C. Davis, University of Virginia

The chief drawback in the use of the ordinary electric lamp as the stimulus in reaction-time experiments, or as the source of light in tachistoscopic and Aussage tests is its latent time. The obvious advantages of the electric lamp over elaborate shutter or electric-spark arrangements make its use highly de-

[4]H. E. O. James and R. H. Thouless, A note on the effect of polarization in psychogalvanic experiments, *Brit. J. Psychol.*, 17, 1926, 49-53.

[5]L. B. Spinney, *Textbook of Physics*, 1927, 359.

[6]Since this paper was submitted a need has arisen for changing the electrolyte without disturbing the subject, as in continuous studies during sleep. By inserting another hole opposite (9) and making the necessary connections with a reservoir and disposal vessel the change can easily be accomplished. The flow can be made either continuous or periodic by the proper kind of release such as a hose-clamp for constricting the supply tube.

*The method described in this note was worked out in the psychological laboratory of Columbia University.

sirable, and yet unless the interval required for the filament to come to full incandescence can be conveniently measured and allowed for, an unknown time error is introduced which may seriously affect results.

The method of measuring the latent time of incandescence, outlined in this note, was employed successfully to determine the latent time of a small 6-v. lamp.[1] It is extremely simple and takes little time.

The lamp (Fig. 4) is connected with the source of current (B) through switch (S) and a rheostat of 2 or 3 ohms maximum resistance (R′). An ammeter (A) and voltmeter (V) are also in the circuit so that latent time can be determined if desired for different intensities of illumination—which may be controlled through the rheostat (R′). The oscillograph (O) which acts as an instantaneous voltmeter is connected in parallel with the lamp through a second rheostat (R) of relatively high resistance (500 ohms or more).

When the switch (S) is closed a small fraction of the current (the amount depending upon the relative resistances in the oscillograph and lamp circuits) flows through the oscillograph. The maximum possible current for any given setting of the rheostat (R′) does not flow through the lamp, however, until the filament reaches full incandescence. The change in the conductivity of the filament produces corresponding changes in the oscillograph which may easily be photographed. By photographing time-units simultaneously the latent time of the lamp is directly measurable from the film.

The only source of error in this method lies in the relative resistances of (R) and the lamp. If (R) is kept large with reference to the lamp the presence of the oscillograph circuit in parallel with the lamp circuit will not appreciably affect the desired current through the lamp. If (R) is not sufficiently large, however, the error introduced will be proportional to

$$\frac{\text{resistance of the lamp}}{\text{resistance of (R)}}.$$

Double Time-Lines as a Device for Precise Reading of Time Relations in a Photographic Record

By Stanley B. Lindley, Institute of Human Relations, Yale University

Fig. 5 illustrates the disposition of two sets of time-lines upon a photographic record in a manner that permits precise measurement of temporal intervals when reading the completed record. The two time-lines, a and b, which are located at the top and bottom of the record were produced by two beams of light reflected from a pair of small concave mirrors fixed to one prong of an electrically maintained tuning fork of 50 d.v. per sec. The two broken lines, x and y, beside the tuning fork lines resulted from the action of two beams of light reflected from two concave mirrors that were attached to a Becker marker. Once each second the marker was actuated by an electrical impulse from the laboratory master-clock and as a consequence the time-lines were broken. Two sets of such breaks are shown in the figure. Lines *l* and *m* are stimulus-line and reaction-line respectively and were also produced by the

[1] The latent time of the bulb in question was found to be 0.18 ± .01 sec.

action of beams of light from concave mirrors. All these lines were recorded on a strip of moving bromide photographic paper. Each of the six beams of light passed through a narrow slit in the camera before reaching the sensitive paper. Owing to the fact that the photographic paper lies a short distance behind the camera slit a slight parallax exists when all of the beams of light are not reflected from a common plane lying parallel to the slit of the camera. In the figure accompanying this note the parallax is negligible. On reading the records a line drawn perpendicularly between any two homologous points on the time-lines and across the entire paper passes through the points on all of the other lines which indicate the position or condition of the mechanical recorders at any particular instant.

A vertical ordinate is conveniently constructed by placing a straight edge ruler on the points 1 and 1′ which mark the homologous breaks in lines x and y and drawing a line along this edge across the record paper. Then, if the record is short and is to be submitted to close study, a set of parallels to this vertical ordinate, connecting identical points on lines a and b (the tuning fork records), can be drawn. The ordinate connecting the breaks in lines x and y serves for orientation and as a base line in constructing the ordinates which connect identical points on the duplicate tuning fork records, a and b.

If the record is long and is only to be scanned, a set of carpenters' parallels or a rolling parallel rule may be used. The reader should adjust these devices to the record by first applying one of the straight edges to the points which mark breaks in the second-lines, x and y.

Fig. 5 reproduces a section from a record of manual pursuit of an oscillating visual object. Insurance Bromide paper, 4 in. in width, was used.

A Tambour Stylus which Eliminates Arc Distortion

By Robert G. Krueger, Institute of Human Relations, Yale University

The stylus of the ordinary tambour is frequently unsatisfactory because of the sloping arc in the tracing (see Fig. 7,A) which is difficult to measure with exactness, especially where the excursion is large. To eliminate the arc the following design is proposed.

Instead of placing the tambour with its stylus tangent to the kymographic drum, in the usual manner, the stylus is mounted at a right angle to the tangent (Fig. 6). It consists of two sections, A and B, joined together by means of a hinge. At the end of section A, a small U-shaped piece of stiff paper (D) is attached with cement. The floating glass section (B) is 8 cm. long and ½ mm. in diameter. One end of section B terminates in an arc of 7 mm. radius; the other is bent to form a right angle. The short leg of the angle is inserted in the vertical sides of the paper forming a hinge. A drop of cement on the free end at (C) holds section B in place. The holes in the paper should be just large enough to permit the glass tracer to swing freely when hanging down; if the holes are too large there will be a lateral vibratory movement. The glass section is allowed to ride against the drum, its own weight being sufficient to make the tracing. A typical tracing secured in this way is shown in Fig. 7,B.

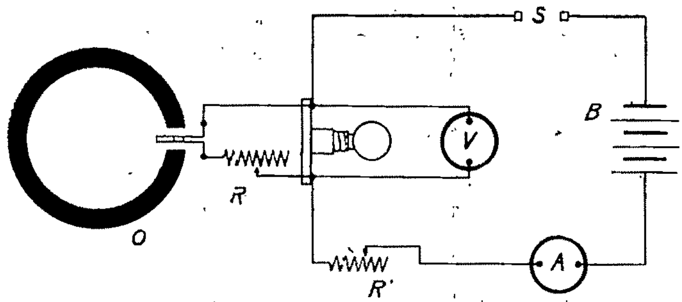


Fig. 4. Wiring Diagram for the Measurement of the Latent Time of Electric Light Bulbs

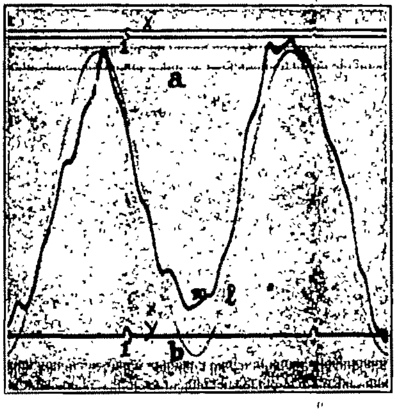


Fig. 5. Illustrating Double Time-Lines as a Device for Precise Reading of Time Relations in Photographic Records

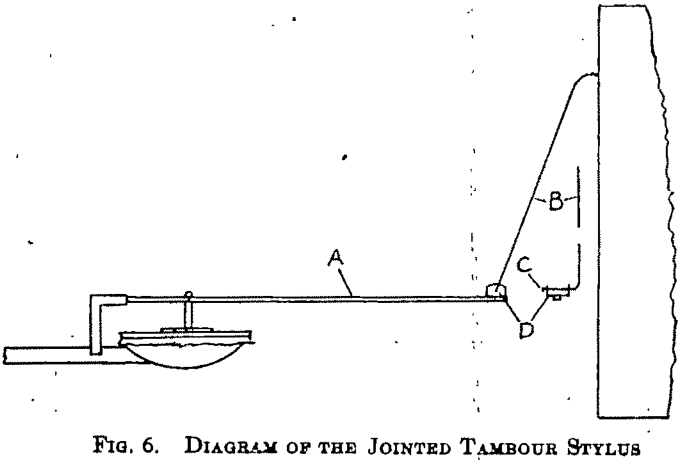


Fig. 6. Diagram of the Jointed Tambour Stylus

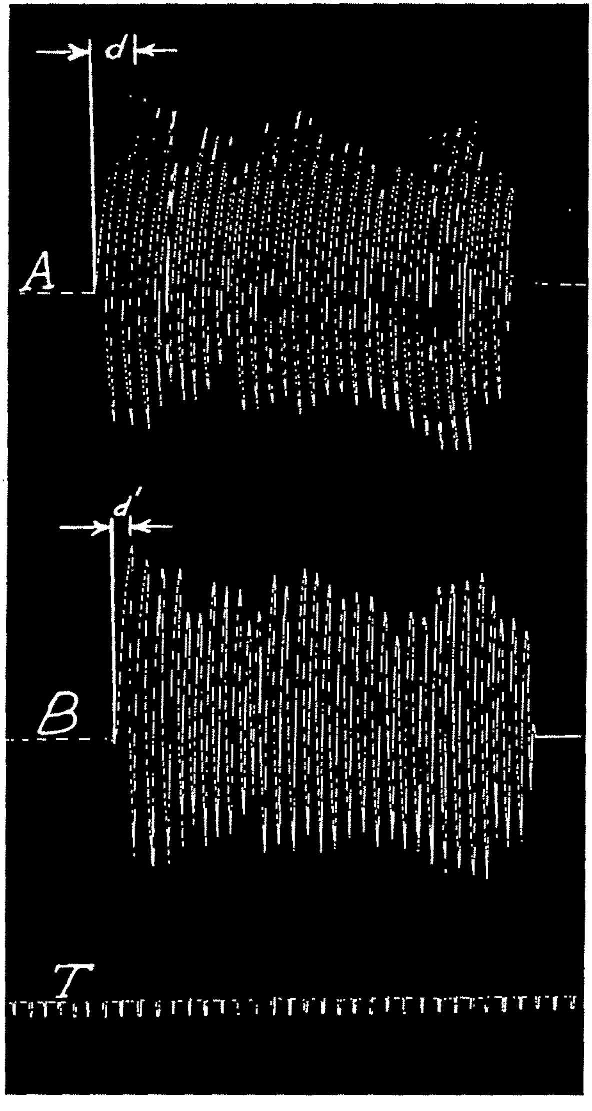


FIG. 7. TRACINGS MADE WITH THE TWO TYPES OF STYLUS
Tracing A was made with the ordinary type; B with the new type; T is the time line showing 5-sec. intervals.

Tracings A and B are breathing records obtained by connecting two tambours to a single pneumograph by means of a glass Y-tube. Tracing A is made with the ordinary stylus, tracing B with the instrument described above. For the purpose of comparison the two styluses were adjusted so that when at rest one was directly above the other. Vertical lines were drawn through the starting points of both tracings. The time-line, marking 5-sec. intervals, is at the bottom.

In tracing B, d′ represents the actual distance which the drum has moved between the center and the upper end of the excursion. In tracing A, distance d represents the actual distance the drum has moved plus the distance due to the distortion produced by the arc of the stylus. Translated into units, d′ represents 2 mm. and d represents 6 mm., a difference of 4 mm. This 4-mm. distortion produced by the ordinary stylus is corrected with our jointed type.

APPARATUS NOTES

'TACHISTOSCOPE' *VS.* 'BRADYSCOPE'*

The limitations of the tachistoscopic technique in reading-research need hardly be emphasized. The earliest perceptual and legibility studies in reading, since Valentine,[1] through Cattell,[2] and up to Erdmann and Dodge,[3] Goldscheider and Müller,[4] Zeitler,[5] and Messmer,[6] were done by the tachistoscopic technique. While important conclusions were drawn from these purely atomistic studies in reading, not to say in perception and attention, Dodge decried, as early as 1907,[7] the indiscriminate use of the tachistoscopic technique in general. He pointed out that the tendency to reduce the physical exposure time to a minimum is a methodological mistake, that visual perception from a threshold exposure is quite a different matter from normal visual perception, and that the adequate tachistoscopic exposure should permit a full and uniformly 'cleared-up impression.' This exposure may still be considered 'tachistoscopic' not in the sense of 'the most rapid excitation' but in the sense of 'the most rapid vision.'

To put 'the most rapid vision' in place of 'the most rapid excitation' means an intentional departure from the fundamental concept of rigid, constant,

*I am indebted to Professors W. R. Miles and K. M. Dallenbach for criticisms. The substance of this paper was read in the Symposium on Laboratory and Teaching Devices at the Ninth International Congress of Psychology, Yale University, September 4, 1929.

[1] G. Valentine, *Lehrbuch d. Physiologie*, 1844.

[2] J. M. Cattell, Über die Zeit der Erkennung und Benennung von Schriftzeichen, Bildern, und Farben, *Philos. Stud.*, 2, 1885, 634-650.

[3] B. Erdmann und R. Dodge, *Psychologische Untersuchungen über das Lesen auf experimenteller Grundlage*, 1898.

[4] A. Goldscheider and R. F. Müller, Zur Physiologie und Pathologie des Lesens, *Zsch. f. klin. Med.*, 23, 1893, 131-167.

[5] J. Zeitler, Tachistoscopische Versuche über das Lesen, *Philos. Stud.*, 16, 1900, 380-463.

[6] O. Messmer, Zur Psychologie des Lesens bei Kindern und Erwachsenen, *Arch. f. d. ges. Psychol.*, 2, 1903, 190-298.

[7] R. Dodge, An experimental study of visual fixation, *Psychol. Mon.*, 8, 1907, (no. 35), 32-37.

automatic, and involuntary control of the duration of exposure to adopt the method of flexible, variable, simultaneous, and voluntary maintenance or control by the *S* himself of the duration of exposure in a tachistoscope.[8] The duration of exposure as reaction-time is taken as a measure of the experimental factor, instead of keeping the time factor constant and using the number of exposures or the so-called perception-span as a measure. This technique of simultaneous voluntary maintenance of exposure adopted in the Quadrant Tachistoscope[9] is perhaps a significant departure from all reaction experiments in general and from all tachistoscopic perceptual and memorial studies in reading and learning in particular. Indeed, the fundamental characteristic of the ordinary tachistoscope, *i.e.* controlled exposure, is radically changed, the exposure being *maintained* by the *S* as long as he needs it. Of course, a tachistoscope adapted from the photographic shutter, as Whipple's portable, has the possibility of obtaining this feature. But nobody, to my knowledge, has made it the aim of a special design and technique.

A related technique is involved in the Dodge Drop-Exposure Apparatus[10] where the interval between the beginning of the exposure and the beginning of the response through a *voice* key is taken as a measure. Here the departure from the usual tachistoscopic controlled exposure is complete, since the extension of the exposure is infinite, *i.e.* the exposed material remains exposed even after the response. In other words, it is only the fixation that is initially maintained *without* the corresponding control of the exposure. The reaction-time is mainly visual-verbal. Although the termination of the vocalization may be taken as the point of measure, Dodge took only the initial utterance of single words as the limiting point. The technique of simultaneous voluntary maintenance of exposure, however, includes more events in the total sensorimotor act than his technique does, and above all allows the *S* to maintain the exposure as long as he needs but no longer.

Dodge used his apparatus to expose single words, to which the *S* responds through a voice-key. So far his technique would closely resemble in fundamentals what I have used. For the purpose, however, of exposing and reading whole sentences in the two axes, the Quadrant Tachistoscope would seem to have the advantage. Whereas in the Dodge Drop-Exposure Apparatus the continuance of exposure after the response may perhaps be a source of error, the Quadrant Tachistoscope permits voluntary exposure, maintenance, and closure of the shutters, allowing the *S* to respond as long as he needs but no longer. It will be at his own disadvantage if he hesitates to release his finger from the key. The response is complex, involving in the single act a succession of muscular action on the finger, perceptual reading, vocal utterance, and finally muscular relaxation of the finger again. There is the eye-voice span and there is also the voice-finger span. This total pattern of response is char-

[8]S. K. Chou, Reaction-keys and a new technique for reading-reactions, this JOURNAL, 41, 1929, 469-473.

[9]S. K. Chou, A quadrant tachistoscope for studying the legibility of Chinese characters, *J. Exper. Psychol.*, 12, 1929, 178-186.

[10]R. Dodge and F. G. Benedict, Psychological effects of alcohol, *Publ. Carnegie Inst. Wash.*, 232, 1915, 90-108. See also M. A. Tinker, Reading reactions for mathematical formulae, *J. Exper. Psychol.*, 9, 1926, 450-454.

acteristic of the technique and is measured from the beginning to the end. The total reading reaction-time is at the same time perceptual, central, and motor, including the complete performance on the reading material as a whole. It is the influence of the introduction of any experimental factor into the reading material as a whole on this complete performance that is sought. In fact, many normal everyday performances such as opening a big dictionary closed on a spring stand to look for a word, or unfolding an atlas tightly rolled up to locate a city or town, resemble this type of reaction. Indeed, ordinary reading of a book is nothing but a perceptual process *with simultaneous voluntary maintenance of exposure!*

In my original description of the Quadrant Tachistoscope, I suggested the term *bradyscope*, which was independently coined by me but was first used by Esper,[11] for all 'memory apparatus' in contrast to *tachistoscope*. I pointed out the fact that both types of apparatus belong to the same class since they are all used for exposing perceptual material for experimentation under temporal control. In fact, some of the instruments such as Wirth's[12] and Ranschburg's[13] employ the same principle of disk construction; Kuhlmann's[14] and Dearborn-Langfeld's[15] can not be strictly classified with the one or with the other. Although perhaps the duration of exposure is the main criterion for differentiating the one from the other, not infrequently the tachistoscope is maintained for over a second, thus overlapping the customary duration of the other apparatus. It might be better to extend the connotation of the already well-known term 'tachistoscope' to include the so-called 'memory apparatus.' But from the insistence of both designers and users on retaining the term 'memory apparatus' as consistently distinct from 'tachistoscope,' it seems obvious that there must be some inherent, characteristic differences that justify this separate, independent designation.

In the first place, the fundamental difference between *tachistoscope* and *bradyscope* lies chiefly not in the principle of construction or in the relative duration of exposure, but mostly in their respective functions. This is clear from the history of the two types of apparatus. 'Bradyscope' is used mainly if not solely for memory, learning, and conceptual experiments, in which the mental processes involved are comparatively more complex in general and longer in duration and distinctly more integrative in nature. The compound term 'memory apparatus' (German, *Gedächtnisapparate*) itself is a functional designation. Just as the telescope in astronomy reveals to us the big, the far, and the remote, so the bradyscope in psychology helps us to study the slow, the gradual, and the integrative. On the other hand, the tachistoscope is devised particularly for various studies of the short, the liminal, and the differential, just as the microscope in biological sciences explores the vast region of

[11]E. A. Esper, The bradyscope: an apparatus for automatic presentation of visual stimuli at a constant slow rate, *J. Exper. Psychol.*, 9, 1926, 56-59.

[12]W. Wirth, Zur Theorie des Bewusstseinsumfanges und seiner Messung, *Philos. Stud.*, 20, 1902, 487-569 (Spiegeltachistoskop, 659-669).

[13]R. Schulze, *Aus der Werkstatt der experimentellen Psychologie und Pädagogik*, 1909, 176-188.

[14]F. Kuhlmann, A new memory apparatus, *Psychol. Rev.*, 19, 1912, 74-78.

[15]W. Dearborn and H. S. Langfeld, Portable tachistoscope and memory apparatus, *Psychol. Rev.*, 23, 1916, 383-387.

the small, the infinitesimal, and the invisible to the naked eye. The tachistoscope is chiefly employed in studies of attention, perception, and apprehension. The term itself is descriptive.

In the second place, the control of the duration of exposure in the tachistoscope sometimes necessitates artificial illumination through an accessory attachment, as in the Dodge Mirror Tachistoscope.[16] While the integrative nature of the mental processes involved in experiments on the bradyscope always requires the experimental material to be serially or successively exposed by the aid of some feeder which is necessarily different from that of the tachistoscope. Tachistoscopic experiment may be carried out with hand feeding of the material, but bradyscopic experiment will preferably, if not absolutely, require automatic feeding.

The analogy between the antitheses of telescope-microscope and of bradyscope-tachistoscope is perhaps more significant than exact. Psychology no longer depends upon physiology for instrumentation. It must acquire its own technology and have appropriate names for its set -ups. If 'memory apparatus,' a term adopted entirely without deliberation, is adequate for its purpose, we might just as well be contented with 'perception apparatus' for tachistoscope or 'time apparatus' for chronoscope. Bradyscopes are used for a variety of purposes. They are no longer limited to memory experiments. Therefore, the compound term 'memory apparatus,' even though it may have some intrinsic merit, should give place to the descriptive term 'bradyscope' in contradistinction to 'tachistoscope.' It is my suggestion that hereafter the term 'bradyscope' should be extensively and exclusively used in place of the so-called 'memory apparatus,' the expression 'exposure apparatus' being reserved for covering both tachistoscope and bradyscope. As I noted in the original description, the Quadrant Tachistoscope is both a tachistoscope and a bradyscope since it can be used for both purposes. Although I have already designated it as a tachistoscope, I am now more inclined to call it a bradyscope or a bradyscope-tachistoscope.

In conclusion, I wish to point out that the Quadrant Tachistoscope or Bradyscope is designed for releasing the most rapid vision of perception instead of the most rapid excitation. It is different from the related Dodge Drop-Exposure Apparatus in that it involves what I call the *simultaneous voluntary maintenance of exposure.* I suggest that hereafter we shall use the term 'bradyscope' in place of the so-called 'memory apparatus' in contradistinction to 'tachistoscope.' The justifications for this suggestion are that it is profitable to distinguish these two classes of apparatus and that the compound term 'memory apparatus' is absolutely inadequate since its functional designation has lost its real significance.

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THE TERM 'BRADYSCOPE'

In spite of Dr. Chou's plea for his term 'bradyscope,' I, for one, am not convinced that there is any need for it, or that it can serve any useful purpose.

If Dr. Chou really means what he says, that "hereafter the term 'bradyscope' should be extensively and exclusively used in place of the so-called 'memory-

[16]R. Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10-13.

apparatus,'" then surely he has not made out a case for his term. If one wishes to give a new name to an old apparatus, one must justify the substitution by showing that the old name is inadequate and that the gain resulting from the change outweighs its inconvenience. Dr. Chou does neither.

The term 'memory-apparatus' is, in my opinion, entirely adequate. If, for example, an experimenter reports that he used a Spindler-Hoyer memory-apparatus, the reference is as adequate as it is specific. Nothing is gained by saying that he used a Spindler-Hoyer 'bradyscope.' The term 'memory apparatus' is, moreover, firmly entrenched in the psychological literature, not only in English, but as Dr. Chou himself points out, in other languages as well. Even though Dr. Chou's term were to be adopted by psychologists of other countries—a contingency not to be expected—students of psychology would not be freed from the necessity of learning the old term. If they are to understand the literature of the past, they would have to know that a 'memory-apparatus' is a 'bradyscope.' Not a difficult association, to be sure; but the question is, is it a profitable one?

It is clear, however, to all who read his note, that Dr. Chou does not mean, in spite of the sentence quoted above, to limit the application of the new term to memory apparatus. He proposes a much broader use; he sets 'bradyscope' in opposition to the term 'tachistoscope.' What now is the difference between the two pieces of apparatus? When do we have one and when the other? Dr. Chou is unable to find anything but a functional difference between them. The telescope-microscope analogy, which, as Dr. Chou admits, is "more significant than exact," does not hold. The functional difference between the telescope and microscope is paralleled by a structural difference, and the two pieces of apparatus cannot be used interchangeably. Between the tachistoscope and the 'bradyscope,' there are no significant structural differences, and the two may be used interchangeably. Dr. Chou's Quadrant Tachistoscope, for example, serves the dual function.

It may be laudable to make two blades of grass grow where one grew before, but the same can hardly be said regarding names of scientific apparatus.

K. M. D.

NOTES AND DISCUSSIONS

The *Gestalt* Psychology and the *Gestalt* Movement

Whatever may be the case in Germany, there is no doubt that *Gestalt* psychology has received a tremendous amount of attention in America. There is no need to document this point by reference to discussion in the literature, to the programs of meetings of psychologists, to the welcome to Köhler's book, or to Koffka's distinguished appointment at Smith College. As is always the case when a new view makes a claim for recognition, this attention has been both favorable and unfavorable; however, I cannot resist a very positive impression that applause and disparagement have been mingled in such an intimate way as to make of the reception of *Gestalt* psychology almost a paradox.

Perhaps I can make my point most clearly by stating at the outset my own ambiguous attitude toward *Gestalt* psychology. When its leading exponents have positive statements to make, I am nearly always in accord with them. I applaud the thought, and ordinarily I applaud the positive exposition of the thought. The movement as a whole seems to me to be the most valuable movement in psychology during the present century. Yet I do not like movements, although I recognize their importance. Most especially I do not like *Gestalt* psychology as a movement. Let us take Köhler's new book as an example. Some of the chapters in it seem to me as near an approach to 'truth' as we have yet had. Some of the exposition is masterly. I have read most of the positive content with assent, an assent that is not lessened when I recognize a view as a venerable opinion of James or Wundt or even Aristotle. For all this approval, the negative exposition, directed against an obscure "behaviorism" or an indeterminate "introspectionism" or a mythical majority of psychologists, is such that the argument, in seeking to avoid personal hostility, appears either uninformed or disingenuous, and, in any case, misleading.

Thus it may well be that this strange commingling of approbation and dislike, produced by *Gestalt* psychology, has its roots in certain incompatibilities within *Gestalt* psychology itself. If it mixes fact with propaganda, if it combines the modesty of presenting new hypotheses representing tentative programs of work in a young science with the egotism of contempt for 'barbarians' and esteem only for 'Rome,' if there is this schizophrenia within *Gestalt* psychology, then it is hardly surprising that its reception runs the emotional gamut, not only for psychologists in the mass, but for even a single individual. It is quite possible that scientific humility and the egotism of movements are essentially incompatible, and that *Gestalt* psychology as psychology and as a movement are in conflict.

The first step in resolving a conflict is to recognize its existence and its nature, and it is for such a psychotherapeutic purpose that I write this note.

Let us begin by laying down two fundamental principles. Each is so obvious and so well-recognized that one hardly ventures to make the formulation without an apology. Each is, however, almost universally overlooked in controversy over movements and their evaluation.

(*1*) *The progress of thought is gradual, and the enunciation of a 'new' crucial principle in science is never more than an event that follows naturally upon its antecedents and leads presently to unforeseen consequents.* It is the thankless task of the historian to undramatize science by showing that the 'great' events are seldom more than the maturation of a genetic course, already determinate, in which some name, by a false atomism of historical description, comes to stand for a change which seems discrete, but which is actually continuous. This atomistic view is the "machine-theory" of the history of science and it is as false as any machine-theory against which *Gestalt* psychology ever inveighed. Science is a dynamic whole; it is not made up of schools where some are right and some are wrong; it is made up of the thoughts and activities of persons, all of whom affect each other, and no one of whom remains very long unchanged.[1]

(*2*) *A movement is always negative; it is characterized in part by its opposition to some other older view.* This older view constitutes for it a frame of reference in respect of which it 'moves.' Of course, a movement may have its positive aspects too, but they alone do not make of it a 'movement.' By themselves they constitute only a 'discovery,' an 'advance,' an attitude or a situation that may come for the time being to be described as 'the more modern view.' This second principle conflicts with the first, for, in its negativism, a movement seeks to give itself a separate individuality by setting itself off discretely from the immediate past. Thus movements support the 'machine-theory' of the history of science, and picture, falsely, a continuous upward trend as a series of abrupt steps.[2]

That the paradox of *Gestalt* psychology arises from the conflict of these two principles will be, I think, fairly obvious to everyone who has sought to evaluate the new movement for himself. There can not be the least doubt that *Gestalt* psychology represents an important positive advance within psychology. It seems to me that there is no less doubt that it exaggerates both the magnitude of the advance and its dependence upon a certain small group of persons. It is not clear that psychology might not have come the same way, only a little more slowly, had there been no *Gestalt* psychologists. The complaint of *Gestalt* psychologists that its critics object to it either as being false, or contrariwise,

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[1]This first principle I have sought to develop, with illustrations, in The problem of originality in science, this JOURNAL, 39, 1927, 70-90.

[2] Upon this second principle I have touched in The psychology of controversy, *Psychol. Rev.*, 36, 1929, 97-121, esp. 114-119. Both of these principles also find frequent illustration and discussion throughout the pages of my *History of Experimental Psychology*, 1929.

In all fairness I ought to add here that movements may have their value, and that the distortion which they present is not always to be condemned. I have made this point in *Psychol. Rev.*, *loc. cit.* The actual history of science as a continuous development in thought and knowledge plays in this situation the rôle of undifferentiated data. The movements, that transform continuity into step-phenomena, are like the perception of data, organized into figure and ground in the fashion that perception is. Institutional science is a sort of social perception of the fundamental data of science, and the principle of its organization always makes discreteness necessary. In the present case, however, we need to get behind institutional science, organized for the economy of thought, in order to resolve the puzzling paradox of *Gestalt* psychology; and for this reason I have treated movements in this note as if the distortion that they involve were always wholly undesirable.

as being 'not new,' is exactly what we should expect if some persons concerned themselves with its positive aspects and others with it negativism. Presently we can attempt to make these points specific, but first we need to consider some other general aspects of the situation.

There is no doubt that *Gestalt* psychology is a movement; and it is a movement in a more extreme sense than behaviorism. The behaviorists do not agree among themselves, and the critics of behaviorism are generally forced to attack Watson, who admits being the founder of behaviorism, if they wish to particularize an argument. In *Gestalt* psychology there is, however, no tendency for Wertheimer, Koffka, and Köhler to differentiate themselves by mutual criticism. If we think that this apparent harmony, which is the greatest strength of a scientific movement, is somewhat suspicious, we shall not be criticizing the integrity of these three men but relying on our faith in individual differences that make complete agreement between intelligent persons impossible in respect of new thought.

Gestalt psychology's character as a movement becomes even more apparent when we realize that it has, almost, a personality. It has, let us note in the first place, a name—although even discordant behaviorism has got a name. Its various members, which include the opinions of the men just named, are integrated into a single dynamic whole. Moreover, *Gestalt* psychology has a mind, and sometimes even a voice, of its own. Köhler speaks of it as something separate from himself, something to be described, something more objective than a personal view.[3] If we suspect telepathy when *Gestalt* psychology has an idea and ventriloquism when its speaks, we are simply objecting to the individuation of schools in psychology, to the 'machine-theory' in science.

Since movements depend upon opposition for their existence, not only do they tend to exaggerate the opposition, but they may even create an artificial enemy in order to have something to fight. Naturally such straw men go down readily to defeat at the hands of their doughty creators; nevertheless, a series of such illusory conquests serves admirably to preserve the individuality of the movement, as well as to increase its prestige. The law here is like the law of visual perception; the figure must have its ground, from which it is set off sharply in perception, although the field which it represents shows no such dichotomy. In the case of movements, however, the law is not always reversible. Attention to the ground may produce no clear figure, or at least none that adequately represents an objective basis for the experience.

In this last respect, it seems to me, *Gestalt* psychology has found it necessary grossly to distort the perception of psychology at large. Köhler has criticized both behaviorism and introspection in great detail, but without specification of culprits.[4] What he gives us are merely John Doe warrants for the arrest of somebody. My detective skill does not reveal a behaviorist who can be indicted

[3]Cf., *e.g.* W. Köhler, *Gestalt Psychology*, 1929, 182-184. I have not tried to collect from this book instances where Köhler lets the god speak for him, because in general Köhler courageously adheres to the first person; cf. p. 34. Nevertheless his occasional tendency to shift responsibility to his idol is too clear a symptom of a movement to be ignored.

[4]Köhler, *op.cit.*, on behaviorism, 3-69; on introspectionism, 70-102.

for just this complication of crimes, and I am convinced that in the case of the introspectionist Köhler must be haunted by the ghost of a psychologist long since dead.[5]

We shall return to this matter in a moment, but it is necessary to say here that what is true of Köhler's method in the big issues is also true in the small. I take one instance. Köhler has had a great deal to say about the fact that phenomenal size, when the distance of the perceived object is varied, is directly proportional neither to the size of the object nor to the size of its image upon the retina.[6] Every psychologists who knows anything at all about this matter knows that he is right. The sophisticated reader is irritated by Köhler's insistence. The naïve reader is led to believe that psychology, benighted before the sunrise of *Gestalt*, believed that perceived size corresponded to the size of the retinal image. Köhler says not a word, for example, about the famous alley experiment and the work of Hillebrand, Poppelreuter, Schubotz, and Blumenfeld from 1902 to 1913, all before the *Gestalt* movement had come into existence.[7] He might say, of course, that the results simply reënforce his point. That is true; but they also reënforce my point that movements suffer from a psychic blindness which is rooted, like other hysterical phenomena, in the ego that a movement protects.

At this point I have said my say about *Gestalt* psychology in general, but, in saying it, I have complained—for it was a complaint—that *Gestalt* psychology cultivates a fictitious opposition in the interests of its self-preservation. It is fair for the reader to ask me to show that I have not been discussing a fictitious *Gestalt* psychology. I ought to be concrete and for this purpose I propose to discuss Köhler's *Gestalt Psychology* as illustrating these general matters of the conflict between the positive and negative sides of the movement. I welcome this opportunity especially because it allows me to mingle applause with disapprobation.

In the first chapter of this book Köhler presents his epistemology, a positivism undaunted by the circular relationship between experience and the objective world, a psychical monism that exposes immediately the absurdity of the chief tenet of behaviorism. It is a masterly exposition of a very difficult matter, and I for one am glad to have at hand so attractive and lucid a reference to

[5]This comment is not to be limited to Köhler. The remarks of Max Wertheimer in *Psychol. Forsch.*, 4, 1923, 301, attack a 'bundle hypothesis' which, I think, no important psychologist since James Mill has held seriously. I have mentioned this point elsewhere, Psychology for eclectics, *Psychologies of* 1930.

[6]Cf. Köhler, *op. cit.*, 74-76, 86 f., 93 f., 105 f., 133.

[7]Cf. F. Hillebrand, Theorie der scheinbaren Grösse bei binocularen Sehen, *Denkschr. d. math.-naturwiss. Kl. d. kais. Akad. d. Wiss. zu Wien*, 72, 1902; W. Poppelreuter, Über die Bedeutung der scheinbaren Grösse und Gestalt für die Gesichtsraumwahrnehmung, *Zsch. f. Psychol.*, 54, 1910, 311-361; Beiträge zur Raumpsychologie, *ibid.*, 58, 1911, 200-262; F. Schubotz, Beiträge zur Kenntnis des Sehraumes auf Grund der Erfahrung, *Arch. f. d. ges. Psychol.*, 20, 1911, 101-149; W. Blumenfeld, Untersuchungen über die scheinbare Grösse im Sehraume, *Zsch. f. Psychol.*, 65, 1911, 241-404. See Schubotz and Blumenfeld for the many other earlier papers that belong in this literature. Poppelreuter and Köhler were at Berlin at the same time and both published papers in this same *Heft* of the *Zeitschrift* (Bd. 45), although they did their experimenting in different laboratories.

which I can send students. Just at present I think of this account as being as important for the psychology of 1930 as Mach's treatment of the same problem was for the psychology of 1885. Here then is a major positive contribution. The view in its general outlines, however, is not new, and I do not believe that it is *Gestalt* psychology, even though any dualism seems at first thought to interfere with the working out of dynamical *Gestalten*. We can not do more than to hypothesize complete continuities in psychology at present, even when no dualistic problem is involved; and even Köhler has had to accept a working dualism in laying down his four laws of psychophysiological correlation. There could be a dualistic *Gestalt* psychology, unless, of course, *Gestalt* psychology is merely whatever Köhler happens to think.

These laws of psychophysiological correlation are also an important positive contribution.[8] They have been implicit in physiological psychology ever since there was such a field, but they have seldom been clearly defined and the assumptions can vary through a wide range of exactitude. Köhler takes a middle ground and fixes his own opinions with delightful clarity.

On the negative side the first two chapters are concerned with a criticism of behaviorism. It is here at the outset that one feels the vagueness of Köhler's enemy. He speaks often of "behaviorism" and sometimes of "the behaviorist," but he mentions no one, except that at the end of a chapter he cites the books of Hunter, Watson, and Weiss. Would he also include Lashley and Tolman and the Holt of 1915? Is there any sense to a picture of "the behaviorist" as a composite portrait of the thought of these six men? Some of them, I should think, would be astonished at this general attack on behaviorism, an attack which ignores their differences.

Köhler's third chapter takes introspectionism to task for rejecting meaning in psychology. This chapter disturbs me because I am some kind of an introspectionist. I am disturbed, not because I disagree with Köhler, but because I agree with him in general, and because most 'good' introspectionists would agree with him today. Whom is Köhler criticizing? Is there any such person as his introspectionist? It is not really Wundt, who never 'came down to brass tacks' in the way that a rejection of the 'meaning-theory' requires. It could be the early Külpe, or even the Külpe of *Beschreibung* and *Kundgabe*, but not the latest Külpe. It could be Titchener of about 1910, but not the Titchener after 1915 who rejected sensation as an observational datum, took up with dimensions of consciousness, and turned his laboratory upon experimental phenomenology. Certainly it is not Müller, for experimental phenomenology was well intrenched at Göttingen three years before Wertheimer's famous paper of 1912. Köhler admits that he is being vague,[9] but that is hardly an excuse for writing a chapter about the current views of nobody of importance.[10]

Experimental phenomenology is, of course, the introspective method employed without the attempt to limit description to a few formal sensory and attributive categories. As I see the development of this view, it was something like this. Nobody, whatever he might say about the problem of psychology,

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[8] Pp. 64-67.

[9] P. 70.

[10] Köhler's definition of introspection reminds me of a grotesque; cf. his pp. 89 f.

ever limited himself strictly to the formal categories. It is almost impossible to find instances of the narrower view in the printed introspections in the literature.[11] About the turn of the century experimental phenomenology was beginning to appear. There were the *Gestaltqualitäten* (ca. 1890-1900)[12] of the Austrian school and the *Bewusstheiten* of the Würzburg school (ca. 1901-1908). Both these movements made the mistake of thinking of the new data as new elements, but Schumann's account of visual form (1900-1904) is even freer from the old tradition. Then, somehow or other, the Göttingen laboratory got into the freer descriptive method with the work of Jaensch, Katz, and Rubin (1909-1915). The *Gestalt* movement proper dates from Wertheimer in 1912. The shift of interest in the Cornell laboratory was later, but the initial stimulus seems to have been derived more from Müller's laboratory than from Wertheimer and Koffka. No wonder then that we are puzzled when we try to determine whom Köhler is refuting.[13]

Köhler gives the core of his *Gestalt* psychology in Chapters IV to VI, on dynamics, sensory organization, and the properties of wholes. The exposition rivals his positive contribution of Chapter I. It is just what we have wanted, a definite, convincing exposition of the central doctrine. The argument shows also how *Gestalt* psychology has matured from the days—eight years ago—when it was occupied with complaining about analysis and association and with asking only for freedom. We continue to get here, however, the same undocumented, and presumably unfounded, complaints about behaviorism and introspectionism, but there are less of them.

Köhler is arguing here for the acceptance of the organization of sensory phenomena in wholes as an obvious fact given to introspection. He draws most of his examples from the field of visual space perception. He is not making of organization a formal synthetic principle. He is saying: let us keep to observation; whatever experience insists upon is scientifically true. Well, that faith is nothing new. It was the faith of Wundt. If Köhler thinks that Wundt failed, it was not because he had the wrong faith. Schumann had the same faith, and he described *sensory organization* in terms of *attention*. The difference here is one of words only. *Gestalt* psychology does not like *attention* as a concept be-

[11] I know of only one case, almost pathological in its extremity: E. Jacobson, On meaning and understanding, this JOURNAL, 22, 1911, 553-577.

[12] Köhler, oblivious of most antecedents, nevertheless gives Von Ehrenfels his historical due: cf. pp. 187-192.

[13] This chapter of Köhler's presents so many minor occasions for dissent that I can only touch dogmatically upon a few of them, with a sentence apiece. The problem of meaning has not been quite neglected by introspectionism (pp.72 f.), for it was the reason for all associationism, Wundt needed *apperception* for it, and the context theory was a theory about it. The interest of Fechner and Hering in memory color (cf. pp. 77 f.) shows that the constancy hypothesis never held complete sway. As a matter of fact the 'constancy hypothesis' (cf. pp. 91, 96 f.) could not have been important or there would not have been any problems for psychology to work upon; practically every psychophysical research is a study of how this hypothesis does not work. I doubt if many psychologists have been taught that illusions which evaporate under an analytic attitude are not valid psychology (p. 94); I was not. Of course, the apparent constancy of perception under some conditions is a matter of learning (p. 80 f.); in narrow introspectionism one use of the 'meaning-theory' was to avoid the 'constancy hypothesis.'

cause it is vague. *Sensory organization* is equally vague. We shall get nowhere until we have a successful hypothesis about the organization. Titchener had the same faith. In order to get rid of the vagueness in *attention* and to insist on its givenness in observation he called it *sensory clearness* and much later *attensity*. If Titchener implied that sensory experience is organized in respect of attention, he was doing nothing more than to attempt to get this phenomenon of organization into verbal description. I do not pretend to have at my command the history of this struggle within psychology, but I think it is fair to say that Wundt, Schumann, Titchener, and Köhler have all faced the same problem, have appealed to the same faith for its solution, have achieved different descriptive terms for the phenomenon, and have gotten almost nowhere with the answer.

Chapter VII deals with behavior, not the behavior of behaviorism, but a mentalistic behavior that implies immediately an experiential correlate or aspect. The exposition wins sympathy. The evidence is rather meager, hardly more than the vague empiricism of the philosopher. The discussion is directed especially against the argument by analogy from behavior to consciousness. I do not know who else may have defended this thesis; Titchener, at least, would never accept the argument by analogy and held that consciousness is not inferred from behavior but is immediately intuited by empathy.

The chapters on association and reproduction strike me as a new general systematic account of learning and recall. Köhler shifts the stress from the law of contiguity to the law of organization. Reproduction is by wholes, within wholes, or for wholes; and frequency of contiguity is rather the principle of the structurization of wholes. All this sounds novel, and, as far as I know, it is novel as a systematic exposition. Nevertheless, we must again look to our history. The law of contiguity was the outcome of the long unexperimental discussion of the associationists, and they had hardly settled down to this single law, when Ebbinghaus brought the problem under the experimental method. Since then there has been much lip service to the law, but I think it is not an exaggeration to say that every experimental research on association has been undertaken to show how the law does *not* work in some particular respect. Take Ebbinghaus' first law. An observer can learn 12 syllables in 17 repetitions, but 24 syllables require 44 repetitions. There has been lots of stupidity. Even Meumann seemed to think that the law of contiguity would hold if twice as many syllables took just twice as many repetitions, whereas the law really means that if 12 syllables will 'stick together' after 17 concurrences, then 24 should 'stick together' after 17 concurrences. Obviously the organization of a larger whole is more difficult. I am not going to argue that Ebbinghaus founded *Gestalt* psychology. I am simply saying that psychologists have understood this principle of the relation of organization to learning well enough to experiment intelligently and profitably ever since the experimental method was brought to bear upon the problem, and that Köhler now at last gives us a good systematic account of what has always been going on. The progress of science is continuous.

The last two chapters of this book, the one on reproduction and the one on insight, are notable. After introducing concepts of the self and of purpose, Köhler proceeds to show that *insight* is the organizing principle by which

organization is dynamically constituted in time. Köhler is modest and tentative, realizing no doubt the vagueness of this principle that lacks even a physiological hypothesis to inflate its frail fabric. With his difficulties one can be sympathetic. Every psychologist who has attempted systematic completeness has come up against this problem. And what have we? Community in recognition of the problem of the gross phenomenal fact, and a variety of words: *unconscious inference, apperception, attention, determining tendency, libido, instinct, purpose, drive, conation*, and now *insight*.[14] I can not see that we are one bit further along.

When one considers the book as a whole, one sees that it progresses steadily from assurance at the beginning to incertitude at the end. The later chapters are but little polemical, and I have nothing but appreciation for the author's candor in thinking aloud and in exposing hypotheses to the ruthless reader. This is *Gestalt* psychology as psychology and at its best. I do not, however, find this spirit in the earlier chapters where the author triumphs easily over dummy antagonists. That, I think, is *Gestalt* psychology as a movement. If there are really two such different *Gestalt* psychologies, it is no wonder that the double personality has been received with such mixed emotions!

Harvard University EDWIN G. BORING

SOME DETERMINING FACTORS IN MAZE-PERFORMANCE

As a result of experiments on animal learning a great deal of attention has been paid to the establishment of the conditions which underlie efficient maze-performance. The conditions are complex and writers have tended to emphasize one condition or another without—so it seems to the present writer—gaining a satisfactory picture of the various interrelationships involved.[1]

Among the factors which determine efficient maze-performance it is possible to distinguish at least three primary variables: 'knowledge,' 'reward,' and 'drive.' There is also a secondary variable, 'reward-value,'[2] which is dependent upon all three of these. Let us then examine these variables and see how they are related to maze-performance and to each other.

Knowledge is a requisite for efficient maze-performance. The rat must 'know' the maze if he is to traverse it rapidly and with a minimum of errors. And here we should note that the usual 'learning' experiment deals only with this relationship, measuring knowledge in terms of maze-performance. Blod-

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[14]"There they stood ranged along the hill-sides! . . . And yet dauntless the slug-horn to my lips I set and blew." Poetical quotation is out of place here, but the picture that I have in mind is given by the last verse of Browning's *Childe Roland to the Dark Tower Came.*

[1]Professor Tolman has referred to the present analysis in a paper read at The Ninth International Congress of Psychology at New Haven, September 6, 1929. See E. C. Tolman, Maze-performance as a function of motivation and of reward as well as of knowledge of maze-paths, *Proc. IX Internat. Cong. Psychol.*, 1929, 439f.

[2]The term 'reward-value' has been used with a slightly more limited meaning by Katherine A. Williams.

gett,[3] the writer,[4] and, indirectly, Simmons,[5] have shown, however, that knowledge is not, in itself, sufficient to produce quick and accurate maze-performance. The animal may know the maze but he will not perform efficiently unless he is rewarded.

Reward, then, is also necessary for efficient maze-performance. Szymanski[6] and others have found that removal of the reward causes a decided decrease in the quality of performance. Blodgett's experiment indicates, moreover, that these first two variables are relatively independent. In other words knowledge may be acquired independently of the reward-conditions.

Drive is also a necessary condition—and by drive is meant the internal state of the animal, *i.e.* the complex of physiological concomitants which we refer to as thirst, or hunger, or sex-drive. Szymanski,[7] for example, found that when hungry, a rat would give a perfect performance; when satiated, it would make many errors.

Reward-value is, as stated above, a secondary variable and is dependent upon all three of the others, *i.e.* reward, drive, and knowledge. The dependence has been, in each case, established experimentally. It is well known that different rewards vary in their effectiveness (or reward-value). Simmons,[8] for example, has shown that "bread-and-milk and sunflower-seed both appeal to the hunger drive . . . . . and yet we get a difference in records with the two rewards." Rewards, then, tend to have a natural or 'real' reward-value which may be due to biochemical factors.

It is obvious, however, that changes in the drive will affect the reward-value of a given reward; that is to say, a reward of food will bring about more efficient maze-performance in a hungry rat than in a satiated rat. Likewise food will have little reward-value for a rat which is thirsty but not hungry. The writer has given a case where different rewards have approximately equal reward-values because of appropriate variations in the drive.[9] Also a case is given where the reward (food during one period and water during another period) has different reward-values for various groups of rats because of differences in the drives.[10]

Furthermore, it has been pointed out that maze-performance is not determined solely by the 'real' reward-value even when the drive is constant.[11] Knowledge or 'expectation' may alter the reward-value. The writer has given a case where the same reward (sunflower-seed) has different reward-value for two groups of rats under the same drive-conditions.[12] This was apparently

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[3]H. C. Blodgett, The effect of introduction of reward upon the maze performance of rats, *Univ. Calif. Publ. Psychol.*, 4, 1929, no. 8.

[4]M. H. Elliott, The effect of change of reward on the maze performance of rats, *Univ. Calif. Publ. Psychol.*, 4, 1928, no. 2.

[5]R. Simmons, The relative effectiveness of certain incentives in animal learning, *Comp. Psychol. Monog.*, 2, 1924, no. 7.

[6]J. S. Szymanski, Abhandlungen zum Aufbau der Lehre von den Handlungen der Tiere, *Arch. f. d. ges. Physiol.*, 179, 1918, 1-244.

[7]*Loc. cit.* [8]*Loc. cit.*

[9]M. H. Elliott, The effect of change of drive on maze performance, *Univ. Calif. Publ. Psychol.*, 4, 1929, no. 11.

[10]M. H. Elliott, The effect of appropriateness of reward and of complex incentives on maze performance, *ibid.*, 4, 1929, no. 6.

[11]Elliott, *ibid.* [12]Elliott, *op. cit.*, 1928.

due to the fact that one of the groups had previously been trained to 'expect' another reward. Williams gives an extreme instance of the effect of previous training on reward-value: a box in which rats had been repeatedly fed came to have reward-value, in itself, even though when used as the goal-box of a maze it contained no food.[13]

$$\begin{tikzcd}
\text{KNOWLEDGE} \arrow[rr] \arrow[dr] & & \text{EFFICIENT MAZE-PERFORMANCE} \\
\text{DRIVE} \arrow[r] & \text{REWARD-VALUE} \arrow[ur] & \\
\text{REWARD} \arrow[ur] & &
\end{tikzcd}$$

The accompanying diagram, then, summarizes the present analysis of the factors underlying efficient maze-performance. The maze-performance is directly determined by knowledge and reward-value. The latter, in turn, is dependent upon reward, drive, and knowledge. Reward and drive function only through the intermediary of reward-value.

The reader may well object that this schema is mere speculative logic; that the variables are arbitrarily selected; and that the schema is incomplete. The answer to the objections is that a moral is pointed directly to the experimenter. The maze *is* used as a measuring device and an understanding of the factors determining efficient maze-performance seems highly to be desired. The three 'primary' variables may be rather arbitrary but they are capable of experimental manipulation and control. There are undoubtedly other factors underlying these 'primary' variables but we must wait upon experimental investigation for further extension or modification of the analysis.

Harvard University M. H. Elliott

Constancy of Attitude Maintained over a Long Period of Time

Five years ago, Professor H. Sherman Oberly acted as observer in a lifted-weight experiment reported by Rudisill.[1] After a period of 5 years during which he has not made any judgments of this sort, he has again been acting as an *O* in another lifted-weight experiment. The objective conditions of the two experiments were the same. In Rudisill's experiment, three forms of instruction were employed—one of which was framed to induce the stimulus attitude. It is possible to compare those results with the ones from the present experiment. These results are given in the accompanying table, in which will be found the values for the indices of precision, the limens and the points of

[13] K. A. Williams, The reward value of a conditioned stimulus, *Univ. Calif. Publ. Psychol.*, 4, 1929, no. 3.

[1] E. S. Rudisill, Constancy of attitude in weight perception, this Journal, 36, 1925, 562-587.

subjective equality and intervals of uncertainty, for both the present and the former sets of results. It is evident from a comparison of the averages of each set of results, that the precision of judgment has not changed to any great degree, inasmuch as the values of h_1 and h_2 are very similar—that for h_2 being almost identical.

The average values for both limens (S_1 and S_2) are lowered and this becomes evident when one considers the values for the points of subjective

TABLE SHOWING THE INDICES OF PRECISION (h), THE LIMENS (S), THE INTERVALS OF UNCERTAINTY, AND THE POINTS OF SUBJECTIVE EQUALITY FOR THE TWO SETS OF DATA

YEAR	SERIES	h_1	h_2	S_1	S_2	INTERVAL OF UNCERTAINTY	POINT OF SUBJECTIVE EQUALITY
1924	1	0.095	0.100	93.05	98.88	5.83	95.99
	2	0.094	0.081	94.79	98.34	3.55	96.44
	3	0.094	0.093	92.46	98.77	6.31	95.60
	4	0.135	0.099	92.04	96.43	4.39	94.32
	5	0.135	0.086	90.33	97.08	6.76	92.96
	6	0.119	0.082	90.89	96.41	5.52	93.14
	AVE.	0.112	0.090	92.22	97.75	5.39	94.74
1929	1	0.112	0.108	90.99	96.60	5.61	93.61
	2	0.090	0.072	90.05	96.09	6.04	92.73
	3	0.080	0.084	88.18	96.91	8.73	92.66
	4	0.111	0.099	89.82	95.40	5.58	92.45
	AVE.	0.098	0.091	89.76	96.25	6.49	92.86

equality. The value for the present series is almost two grams lower than that for the former set. Indeed all four of the values for the fractionated series are lower than all but two of the same values obtained before. But it will be observed that the present values are of a similar magnitude to the *last* values of the former experiment after progressive practice was complete.

The same relations are observable for the values of the interval of uncertainty. Although the average for the present set is larger by almost 20% than the average for the former experiment, the present average is much more similar to the values after former progressive practice.

So far as the author knows, there have never been reported results of an observer taken under similar conditions after a long interval. The results of a single observer carry little conviction but these results, which show such a striking constancy of attitude after a 5-yr. period without practice, are given in the hope of stimulating further research as opportunity may offer.

University of Pennsylvania S. W. FERNBERGER

The Psychological Register

Professor Dallenbach's critical examination of the *Psychological Register* in the January number of the Journal seems to me to dwell too lightly upon the matter of the omission of so many of the names of important foreign psychologists. In editing the *Proceedings* of the Ninth International Congress of Psychology I have gained the impression that, when in doubt about the exact name or the city of residence of a foreign psychologist, it is quite useless to go to the *Register*. The membership of this Congress included, however, some relatively unimportant persons and it is probably not a fair check upon the *Register*.

One gets a little more insight if he examines the membership of the International Committee that stands back of the Congresses. Each of the persons in this Committee is supposed to have been among the leading psychologists of his country at the time of his election, although many who were elected long ago have now ceased to be active. There are at present 122 persons on the Committee, and the following 46 are not listed in the *Register*.

d'Arsonval	Forel	Patini	Sergi
Asher	Gheorgow	Patrizi	Seris
Baldwin, J. M.	Gielecky	*Pavlov	*Spielrein
Bernfeld	Head	Pikler	Stout
Bohn	Heinrich	Roels	Stuart
*Borovski	Höffding	*Roxo	Thiéry
*Bouman	Leuckfeld	Rubin	Toulouse
Brown, W.	Ley	*Salkind	Tschelpanov
Cajal	Magalães	Schuyten	Villa
Demoor	Mendelssohn	Séglas	Winkler
Ehrenfels	Mingazzini	Seracky	Youriévitch
Ferrari			Zwaardemaker

Of these 46 there are 25 that I, from my own knowledge, believe should have been included in the *Register*, and of course the *Register* ought to transcend the casual knowledge of any one individual. The asterisks in the list indicate six omitted names among the 21 foreign psychologists elected to the Committee in 1929, men whose psychological activity is therefore definitely of the present.

Plainly a revised and more comprehensive edition of the *Register* is needed whenever it can be prepared. Let us hope that, if the editor continues with the plan of mustering the dead in a second volume, he can find some better method for making up the roll. Unfortunately for him the dead will not reply to questionaires, nor will their contemporaries ordinarily be willing to compile lists.

E. G. B.

The Origin of the Word *Cretin*

Professor S. I. Franz in a recent article says: "To those children in which general mental-cerebral relation is retarded because of failure of development of the thyroid gland, the name of cretin is given. This is due to its early recognition as a frequent occurrence in the island of Crete."[1] This theory of the origin of the word *cretin* rests upon a similarity which is entirely accidental. The Oxford English Dictionary and the larger dictionaries of the French language indicate that the English word 'cretin' is taken from the French *crétin*, which goes back to a Swiss patois word *crestin* or *creitin*, which came from the

[1] *The Foundations of Experimental Psychology*, 1929, 811.

Latin *christianum*, Christian. Why these idiots were called 'Christians' is not entirely clear. The word 'Christian' in the Romanic languages used to mean human being as distinguished from brutes. Apparently its application in this case is a euphemism. The same sort of euphemism is seen in the words applied to lepers in France in early times—*les malades* or *les chrétiens*.

Rice Institute FRANK A. PATTIE, JR.

ANOTHER NOTE ON THE OBSERVER IN PSYCHOLOGY

The note to which Professor Dashiell refers (*Psychol. Rev.*, 37, 1930, 183) I was prepared to publish provided an editorial comment were to follow it. The note itself seemed to me neither sufficiently competent nor informed to appear by itself. It was then withdrawn by the author, who failed, of course, —until my brief article came out (this JOURNAL, 41, 1929, 682-683)— 'to learn what distinctions were meant." Has Dashiell exaggerated the dependence of my article upon his note? His note, which was typical of much recent behavioristic writing, merely presented an occasion for my remarks upon "the history and significance" of the terms 'observer' and 'subject.' Dashiell contended that since the word 'observer' had been misused (he cites one doubtful example), it should be dropped entirely. On the basis of that slender proposal, I could scarcely have held him responsible for the matters which I specifically discussed.

In professing to "reply" to me, in his second note, Dashiell seems to have been too deeply annoyed to observe my distinction between the terms in question. He proceeds gravely "to recommend the universal use" of *subject*, for, as he argues, "an interest in clarity would demand the use of one common word in all cases." He therefore proposes to compromise upon his own preferred term. That would "be accurate enough" for him.

Since I made no declaration about the general "scientific rôle" of the observer, that part of Dashiell's criticism might have found a more appropriate context. Had he seriously wanted to know about the observer "in Titchener's sense," he might have turned to that writer's *Systematic psychology; prolegomena* (1929) instead of loosely observing that "there is abroad the opinion, etc." To label those who use this "particular terminology" as 'disciples' of Titchener seems to remove the discussion for partisan purposes from the discourse of the sciences; but Dashiell may elect to bring it back by naming those whom he thinks of as maintaining the discipular relation. MADISON BENTLEY

A CORRECTION: ALLPORT'S "SOCIAL FACILITATION"

In his article upon "Allport's experiments in 'social facilitation' " E. G. Williamson[1] examined the evidence presented by that author for the alleged effect of facilitation in small working groups. In so doing Williamson misread a table of Allport's[2] and contended that the author of the article had not taken into account an unlike number of trials made in isolation and in group-work. He overlooked a statement in Allport's text about averages, and he seems to have been confused by the "total number of associations" (Table II), which appears to be the sum of three minute-periods; although, if it is, then 23 of Allport's additions are arithmetically wrong. At all events, this point, which Williamson

[1] *Psychol. Monog.*, 35, 1926, (no. 163), 138–143.
[2] *J. Exper. Psychol.*, 3, 1920, 164.

also makes against other tests upon association and upon "thought process," obviously rests upon a misunderstanding of Allport's figures. Since the present writer was general editor for the *Studies* in which Williamson's criticism appeared, he makes this tardy correction. MADISON BENTLEY

KWARTALNIK PSYCHOLOGICZNY

Volume 1, number 1, of the *Kwartalnik Psychologiczny*, the Polish *Quarterly Journal of Psychology* has recently been published. Stefan Blachowski of Poznan is the editor. The first number has 144 pages. It is well printed and edited. American psychologists will welcome it and wish it success.

Psychological journals have hitherto failed in Poland for want of financial support; but the new *Quarterly* is to be sustained from a governmental "Fund for National Culture." While it is established for Polish psychologists, the *Quarterly* will receive contributions (in German, French or Polish) from other European countries. In the first number are five original articles, two experimental and three theoretical. Mario Ponso (Turin) writes upon "Phénomènes d'annulation perceptive avec des 'stimulus' surliminaires, Georges Dwelshauvers (Paris) upon "Cerveau et pensée" and Ramiro Bujas (Zagreb) upon "Die psychischen Bedingungen des psychogalvanischen Phänomens." The two Polish articles are from Poznan University. One is on "Erroneous responses in pedological researches" (by Ludwik Jaxa-Bykowski), and the other is a "Note upon the new definition of intelligence proposed by W. Stern" (by Adam Wiegner). The foreign articles are abstracted in Polish, and the Polish in French.

The *Quarterly* contains discussions, notes, and book reviews. It also includes a section of Polish abstracts of articles from foreign periodicals. As the abstracts are seriously done for most of the important European and American journals, they should be of great service in Poland where the foreign periodicals are difficult to obtain.

Sarah Lawrence College THEODORA MEAD ABEL

JOURNAL OF SOCIAL PSYCHOLOGY

The first number of the *Journal of Social Psychology*, dated February, 1930, has been received. It is edited by John Dewey and Carl Murchison. Luberta M. Harden is assistant editor. An "international coöperating board," composed of fourteen American and six foreign members, is listed on the cover page. The scope of the new journal, as announced in a subtitle, extends to "political, racial, and differential psychology."

The February number contains the following articles: A neurotic inventory, by L. L. and T. G. Thurstone; The development of men of science, by A. T. Poffenberger; The physiological basis of neurosis and dream, by T. Burrow; Recent improvements in devices for rating character, by M. A. May and H. Hartshorne; The contribution of ten chronicles-of-American photoplays to seventh grade history teaching, by J. W. Tilton and D. C. Knowlton; An objectivity-subjectivity ration for scales of measurement, by H. F. Adams; and Why do we weep? by F. H. Lund. A section is devoted to short articles and notes, and one to reviews of books. The articles are followed by abstracts in French and German. K. M. D.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Bodily Changes in Pain, Hunger, Fear, and Rage. By WALTER B. CANNON. Second Ed., Revised and Enlarged. New York, D. Appleton & Co., 1929. Pp. xvi, 404.

The revisions in this valuable and well known book are particularly interesting to psychologists because four of the five new chapters which have been added in the present edition deal with topics which are largely psychological. These four new chapters are: Emotional Derangement of Bodily Functions, The Physiological Basis of Thirst, A Critical Examination of the James-Lange Theory of Emotions, and Emotion as a Function of the Optic Thalamus. These additions are, in the order named, chapters 14, 16, 18, and 19. (In the Table of Contents 18 is wrongly numbered 19.) These chapters, together with Chapter 11, on Emotional Increase of Red Corpucles, the fifth new chapter, cover 89 pages. A good deal of recent material has been added to the other chapters in support of the author's general thesis "that the bodily changes which attend great excitement are directed toward efficiency in physical struggle," and the references at the end of the chapters have been brought up to date. Much of the data even in the chapter on Emotional Derangement of Bodily Functions is also of the 'supporting' kind, although the author does admit (and does support by evidence) that pathological states may manifest themselves in extreme emotions and harmful results may follow. In this chapter, after replying briefly to Piéron's contention that emotional reactions contain large elements of useless facial contortions and are accompanied by pathogenetic processes, Cannon argues that the outbursts of these major defensive emotions have the characteristics of a simple reflex—"the inborn, prompt, constant, uniform, permanent and useful nature of the response to a definite kind of stimulus"—and differ from the reflex "not in quality but in complexity" (p. 244). One cannot but think that these utility aspects are still somewhat overdone, though he may agree with the general thesis of the author.

Experiments by Cannon and Britton and by Bard are considered, which indicate rather conclusively in cerebrally decorticated cats the existence of certain complex responses some of which are under cerebro-spinal and some under sympathetic control, and which show that "typical sham rage, accompanied by vigorous discharge of sympathetic impulses, occurs when both hemispheres, the corpora striata and the anterior half of the diencephalon have been completely isolated. The additional extirpation of the posterior half of the diencephalon," however, "promptly abolishes the spontaneous activity. Further tests proved that the center lies in a small brain mass in the ventral part of this region" (p. 247). This ancient portion of the brain contains the nervous organization for the display of rage including both bodily attitudes and visceral responses. This region is not so located, as are the cortical regions, that the activities under its control can be constantly adjusted to external

changes in the environment of the animal, but it is so disconnected from such outer influences that the essential features of the acts controlled through this center are unlearned, being "prompt, constant, uniform and permanently established patterns of reaction to appropriate stimuli" (p. 248).

Thus wholly on experimental evidence it has been shown that these related defensive emotions have their centers in the thalamic region. Neural schemes are presented to show how, through subcortical interruption of the motor tract, patients with certain forms of unilateral paralysis (hemiplegia) may be incapable of moving the face on the paralyzed side, yet in emotional excitement the muscles which were unresponsive to voluntary (*i.e.* cerebral) control flash into action, giving both sides of the face the appropriate expression whether cheerful or sad. In unilateral injury of the thalamic neurones the patient may move both sides of the face at will but laughs or weeps only with one side of the face.

So, it is pointed out, the skeletal muscles are governed at both levels, cortical and thalamic, but the viscera are directly only under the thalamic control. When there is conflict between these two systems, the cortical excitation dominates over and checks only those bodily functions which are normally under voluntary control. Now, on the assumption that as a rule only the cortically controlled activities are conscious, Cannon holds that those under subcortical control, which are ordinarily not conscious, may send their impulses into the cortical area causing the well-known "emotional seizures," such as uncontrollable laughing, weeping, manifestations of rage, of 'possession,' etc. In this respect there is certainly much similarity between his view and that expressed by James.

Cannon notes the danger to patients suffering from hypertension and senile impairment of getting into situations which are likely to cause extreme excitement. Under such conditions the sympathetic impulses both speed up the heart rate and constrict the arterioles, thus raising the blood pressure. He also gives evidence that violent emotional disturbances may produce harmful effects through hyperthyroidism and the consequent marked increase in metabolism, as well as by other means. In cases cited, certain murderous acts witnessed by the unfortunate individuals were final. They could not be undone, and the neurones oversensitized by their effects were afterwards constantly aroused by stimuli which were in some respects similar to the original ones. Thus, Cannon maintains, "the persistent derangement of bodily functions in strong emotional reactions can be interpreted as due to persistence of the stimuli which evoke the reactions" (261). Other cases of persistent derangements may be due, he suggests, to circumstances which prevent the completion of the emotional impulse and thus the elimination of the derangements.

This latter is interesting to the reviewer as implying in its negative aspects, of *in*completeness of response, a principle that he urged in 1916 as important in learning, and which found considerable support in later physiological and psychological work, a principle that does not invoke the 'stamping in effects of pleasure.' When Cannon refers to habit formation, however, even of emotions, he falls back on the old frequency doctrine of association, which for over ten years has been put in question by psychological experiments, and asserts that

"habits are established in the nervous system by frequently repeating an act. Every time the nerve impulses traverse a given course they make easier the passage of later impulses" (p. 265).

In the long chapter on the Physiological Basis of Thirst, Cannon discusses both the "local origin" and the "general origin" theories, and presents interesting observational and experimental evidence for the latter view. Cannon's theory is that thirst does not require specialized nerves or any peculiar sensitiveness of the early portion of the digestive tract. In aquatic forms of life the entire organism was bathed in water, and even the mouth and throat were continuously flooded, since food was taken in a wet medium. In land animals only the mouth and throat continue to be moist. The respiratory tract, constantly exposed to air, is protected by a lining membrane of columnar epithelium and is richly provided with mucous glands, particularly in the nose. The mouth is not so provided, and although there are some mucous glands they are not capable of keeping the surfaces wet, expecially when air passes through the mouth as in prolonged speaking or singing. These surfaces must therefore be constantly flooded by salivary secretions, as in swallowing. In the pharynx the respiratory passage crosses the digestive tract, and if the water content of the body be for any reason reduced so that decreased salivation occurs dryness easily sets in, stimulating tireless swallowing activities, with the well-known resultant dryness sensation.

Cannon found that checking salivary secretion by means of atropine resulted in the appearance of all the sensations and feelings ordinarily associated with thirst. By a method of measuring the saliva output in the mouth (chewing a tasteless gum at a uniform rate for five minutes and then collecting all the saliva that had been secreted during this time), he shows that the secretion is markedly reduced both as a consequence of going without fluid for a considerable period and as a result of profuse sweating. These results, as well as those of other investigators also presented in the chapter on thirst, support Cannon's conclusion, that the salivary glands, along with their other functions, keep moist the "ancient watercourse," and that a lack of "free water" in the general organism makes it impossible for these glands to keep the mouth and throat moist, the consequence being the local discomfort and unpleasantness, especially where the air passage crosses the food tract. There is no more experimental warrant, however, for the author's view that food and water are taken "to avoid or abolish . . . . the disagreeable sensations which arise and torment us" (p. 331), than for believing that the decerebrate pigeon whose cells have become depleted is seeking food to abolish its discomfort, or that we sneeze 'for the pleasure of it.'

Cannon's criticism of the James-Lange theory of emotions is already, in its main features, well known. It partly suffers from the error we have pointed out at the end of the previous paragraph. He charges James and Lange with a too great emphasis on the effects of visceral reactions. His own recent work, carried out in coöperation with Lewis and Britton on cats, after "removal of the entire sympathetic division of the automatic system," showed (*by the behavior* of the animals) that "these extensively disturbing operations had little if any effect on the emotional responses of the animals" (p. 348). But he could not, of course, *see* what James meant by the *emotions*, the 'felt aspects' of these

expressions. Cannon saw only the habitual, external expressions of certain stimuli. James could today, if he were here, accept all Cannon's results in their entirety. When Cannon says, with Sherrington, that the same visceral changes occur in very different emotional and non-emotional states, James could well reply that the "different emotional states" can of necessity be ascertained in the cats only by the differences in their reactions. Though Angell's criticism in 1916, as well as that by Perry in 1926, is considered, its force seems hardly to be appreciated. Indeed, it seems to the reviewer a useless thing in these days to spend much time on what differences a decerebrate cat *might* "feel" or "sense" under different stimuli and postures, even if these latter be now called "emotional expressions." Experiments on humans, offered by Cannon, show that on injection of adrenin directly into the blood stream or hypodermically and on obtaining thereby "dilation of the bronchioles, constriction of the blood vessels, liberation of sugar from the liver, stoppage of gastro-intestinal functions, and other changes such as are characteristic of intense emotions" (355 f.), the subjects reported no specific emotions, but only feelings of being "on edge," "keyed up." Feelings "as if I had a great fright yet am calm," etc., were also reported. These experiments, too, give no evidence against the theory in question in its general aspect. That theory is that emotions are not some sort of mind stuff, *sui generis*, which produces the bodily changes, but that they are only our sensing of these changes as they occur. The specific contributions of Cannon's excellent work, even to ascribing emotional reaction-patterns in considerable degree to a portion of the optic thalamus, are all modifications which James could well accept, and, indeed, would undoubtedly accept if he had survived to the present.

Cannon's main weakness seems to be a sort of dualistic assumption that sensations, and probably feelings, as subjective experiences, are themselves causes of, or stimuli to, various instinctive and other activities. This is an error that James was working away from. But to what, if not to the physiological changes induced by stimuli (outer or inner), does Cannon ascribe emotions as *felt* experiences? Cannon's reply would probably be that "the peculiar quality of the emotion is added to simple sensation when the thalamic processes are aroused" (p. 369). But is he willing to assert that even *sensations* are wholly conditioned by afferent impulses?

It must, of course, be admitted that James did not know the importance in emotions of certain physiological changes in the thalamic region, though he did admit the existence of purely cerebral emotions. "We have then," he said, "or some of us seem to have, genuinely *cerebral* forms of pleasure and displeasure, apparently not agreeing in their mode of production with the 'coarser' emotions we have been analyzing" (*Principles*, II, p. 468). He probably erred in making these exceptions mainly "aesthetic emotions;" he was certainly wrong, if Cannon's results hold, when he wrote the caption "No Special Brain-Centers for Emotions."

J. P.

Speech and Hearing. By HARVEY FLETCHER. New York, D. Van Nostrand Co., 1929. Pp. xv + 331.

This volume summarizes and discusses experimental results obtained in the Bell Telephone Laboratories. The problems dealt with are chiefly those encountered in engineering technique, and they are treated mainly as problems in physical science, rather than as problems of physiology or psychology. For instance, after a brief statement of the conflicting theories of speech-production —the harmonic theory advocated by Wheatstone and Helmholtz, and the inharmonic theory of Willis and Hermann—one reads that "the difference in the two theories is not, as some suppose, a difference in the conception of what is going on while the vowel sounds are being produced, but in the method of representing or describing the motions in definite physical terms. The second point of view enables one to visualize in a more direct way what is taking place and consequently is of greater value to the phonetician interested in the mechanism of speech production. It probably enables one better to grasp the fundamental characteristic differences between the vowels. . . . The first point of view is probably more useful to the engineer who is interested in designing telephone systems to properly transmit speech" (p. 49).

In discussing the mechanism of the ear in hearing the Helmholtz theory is accepted with slight modifications, such as the all-or-none nerve impulse, the dependence of loudness of sound upon the number of fibers stimulated, and the limitation of nerve impulses to 1000 per second. "Consequently when a pure tone having a frequency of 2000 or 3000 cycles excites the ear it is probable that the number of nervous impulses being sent to the brain per second by each nerve fiber is considerably less than the exciting frequency. This is a very important fact, but up to the present no direct experimental evidence is available to determine the rapidity of the impulses being sent to the brain, when the auditory nerve is excited by tones of various pitch" (p. 129). In other words, the mechanism of sound production above 1000 cycles per second is unknown.

The limits of audition are graphed with reference to low and high pitches of approximately 20 and 20000 cycles, and the thresholds of hearing for weak intensity, and of 'feeling'—near pain—for strong intensity. Within this egg-shaped area of audibility some 2000 perceptible graduations of pitch, and various levels of loudness up to the 'feeling' threshold are mapped out.

Experiments on the masking effects of tones of different pitch indicate that "a low tone will not obliterate to any degree a high tone far removed in frequency, except when the former is raised to very high intensities. Also a tone of higher frequency can easily obliterate a tone of lower frequency if the frequencies of the two tones are near together" (p. 170).

The recognition of phase difference, which enables us to localize binaural sounds, is somewhat naïvely referred to 'education.' "Our experience with such sounds has given us an education so that we unconsciously know the way these changes take place as the source is moved into different positions. According to this view, it seems reasonable to expect that new complex sounds would be very much more difficult to locate than those with which we are ordinarily familiar" (p. 193).

A method of determining loss of hearing in sensation units of the area of hearing is described and graphically plotted on the normal area. The loudness of sounds is found to be related both to the intensity of the sound and to the pitch and sensation level at which the sound occurs.

Recognition of pitch in musical sounds, when various partials, including the fundamental, have been eliminated, appears to be retained even when the first seven partials are gone. Any three consecutive components of the harmonic series 100, 200, 300 etc. are sufficient to give the pitch 100, while 200, 400, 600, 800, 1000 will give the pitch 200. But 300, 600, 900 give separate tones in harmony. The combination of 100, 300, 500, 700, 900 does not give 200. Although the tone 200 is heard, the sound in general is that of a noise. "It is very probable," writes the author, "that the relative positions of these regions of stimulation, due to the harmonics—either objective or subjective—are the real determiners of the pitch" (p. 252). "When the four tones having the frequencies of 400, 500, 600 and 700 cycles per second act upon the ear, the impulses in the auditory nerve will be timed somewhat as follows. There will be certain fibers excited by the 400 cycle tone which will be firing every 4th vibration. Certain ones excited by the 500-cycle tone firing every 5th vibration, certain ones excited by the 600-cycle tone which will be firing every 6th vibration and certain ones from the 700-cycle tone which will be firing every 7th vibration. These discharges will all unite to form impulses in the auditory nerve having a time interval of 0.01 second. Similarly a number of combinations will unite to give an impulse of 1/2, 1/3, 1/4, 1/5, 1/6 and 1/7 of this interval. There will be discharges at other time intervals, but the number of fibers causing them will be considerably less than the particular ones given above. It may be that the recognition of these time intervals by the brain aids in the recognition of pitch" (p. 254).

Chapters follow on methods of recognizing speech sounds, and the varied effects of intensity, distortion of frequency, noise, and deafness.

While the technical proficiency of the author's apparatus and the expertness of his measurements are both admirable, this obviously onesided approach to the problem of speech and hearing makes one wonder if the sensory data may not have been in part created by the methods employed in their investigation. At least, it can be said that there are fewer psychologists today than there once were who are able to sanction the analysis of speech and hearing into such discrete units of sensation as appear in these results.

Cornell University R. M. OGDEN

The Fundamentals of Human Motivation. By LEONARD T. TROLAND. New York, D. Van Nostrand Co., 1928. Pp. xvi, 521.

This book seeks to supply a deficiency in psychology books which has come about because of the peculiar circumstances under which experimental psychology began. In that day either the higher 'faculties' or external stimulation, which might arouse various associations, were supposed to supply the drives to activity; and not for decades did motivation become a problem for special investigation. The author rightly holds that from one point of view the problem of motivation involves all psychology, and in the present book he attempts

to give "a complete treatment of the problems and facts of motivation by the psycho-physiological method" (p. 3). The book contains no original experimental work on the problem; it is rather a theoretical presentation chiefly of different views that have been held in pre-scientific times, so far as psychology is concerned, mixed with a good deal of modern neuro-physiology, a large proportion of which is itself the author's speculation. The author considers motivation from the standpoints of popular thought, of physiology, of introspection, and of what he calls "psycho-physiology." He seems much concerned about the "non-physical or mental factors," and while he admits that "the common sense belief that the mind actually guides or energizes the body is almost certainly erroneous" (p. 10), nevertheless he cannot rest content with what he calls purely materialistic explanations. "As a matter of fact, the problem of motivation can be stated without any actual reference to physiological factors or events. This is proven [sic!], very clearly, by a study of *dreams*" (p. 11) which, he holds, have little relation to anything going on in the nervous system. "Accordingly, the laws of the waking mind are to a large extent those of the material world, whereas the dream consciousness makes its own laws and molds the entire presentation to suit subjective tendencies" (*ibid.*). Quite the contrary can, of course, be maintained by any other dualist. These "mental tendencies" are called *desires*, *appetites*, *wishes*, or *wants*. One reason for becoming so entangled with old dualistic views is the author's tendency to consider seriously and attempt to harmonize all past and present views. He thus gets a combination that is probably not only not very *palatable* to many present day psychologists, but that fails to satisfy the inquiring mind concerning the cause of action and thinking.

The general mechanisms of response and of reflexes, properties of reflex centers, etc. receive extensive and clear treatment. To match Sherrington's *nociceptive* (prepotent) receptors he invents the term *beneceptive* reflexes, and these two *terms* are then made to do the duty of explaining the "stamping in" (beneceptors) and the "stamping out" (nociceptors), just as 'pleasure' and 'pain' has for centuries done for the hedonists—and with as little real explanation value. The hypotheses based upon the operation of these systems are of such a nature as to be non-testable experimentally and therefore non-scientific. These mysteriously operating nociceptors and beneceptors, are so intermixed with excellent statements of automatic, reflex, and instinctive processes that even the informed reader must constantly be on his guard not to confound established fact with fancy. The result is that the reader who is not constantly asking "How?" finds the cloudy points, which are due to our still very great ignorance on many points of neural function, generally cleared up. This is satisfying only as long as he takes for granted the undemonstrated hypotheses.

The various theories of learning are critically reviewed and likewise "cleared up." As with other theories considered, those of learning "all point to real aspects of the learning process," but they nevertheless "all display a surprising blindness to a very simple explanation which the phenomena of learning by experience should suggest to anyone who is acquainted with the principles of modern neurology, as well as with the opinions of common sense" (p. 187). This simple explanation is that given by the author based upon the *assumed* functions of the 'nociceptors' and 'beneceptors.' If one assumes such me-

chanisms to do all the work of inhibition, facilitation, fixation, elimination, etc., and of other processes in learning that are not understood. He has the 'explanation.' It is strange, indeed, that all neurologists, physiologists, and psychologists have overlooked this simple explanation! This simple process of explanation, of course, stands out brilliantly in contrast to "the majority of extant theories of the trial and error process of learning," which, "suffer from a vagueness which makes it difficult to comprehend them at all clearly" (p. 198).

In attempting to arrive at a clear notion as to how the "unsuccessful" acts in a learning process—those which are finally dropped off—are inhibited, the author regards inhibition as a process of lowering the conductance of nerve paths—presumably at the synapses—or of increasing resistance. This, he holds, "must be engineered by afferent rather than by efferent devices, since there is no 'back-fire' of the efferent system into the cortex" (p. 203). The proprioceptive impulses are here included with the afferent, of course. Now Troland by this assumption, that only afferent mechanisms are involved in inhibition (and in facilitation), finds an opportunity to put the operation of pleasure-pain factors on this afferent side and of giving these factors a real part in learning. The 'law of effect' is thus explained, and on the basis of the nociceptive and beneceptive mechanism. As in other cases, everything that has been proposed by anyone on learning—even by 'common sense'—is saved. But again the critical reader fails to have his *how* answered. The only aid he gets is this "all that we have to assume is that the stimulation of nociceptive afferent channels reduces the conductances of the paths of cortical conduction which are operative at the moment, or have been operative over a limited prior interval; and that, on the other hand, the excitation of beneceptive channels will cause a corresponding increase in conductances. This hypothesis is very simple and involves no reference to the conscious phenomena" (p. 205).

Indeed this *is* simple! A whole chapter then follows on "Nociception, Beneception and Retroflex Action." Here the author suggests that we might define "a nociceptive sense channel as one, the normal excitation of which produces unpleasantness in consciousness," and a beneceptive channel as one whose operation is normally pleasant. But this definition is rejected, first, because this sort of "mental distinction" is not necessary, and, second, "because it would logically prevent us from establishing a signficant correlation between the physiological and the psychological processes at a later point in our argument." He decides, therefore, to "pick out nociceptors and beneceptors by a purely biological criterion and then show that their functions have a constant correlation with the affective life" (p. 206). A nociceptive system is then defined "as one which responds specifically to stimuli that are injurious to the organism or the species," and a beneceptive system "as one which is especially sensitive to beneficial stimuli." These operations can apply only to average biological conditions. Exceptions to the rule under peculiar circumstances are admitted. After a cataloguing of the nociceptive and the beneceptive systems (which on the whole is not a difficult thing to do as these are defined) the author takes up "retroflex action"—another new term. This term is chosen to suggest "a kind of 'back-kick' of organic effects into the cortex." When the cortex "by its principle of trial and error" response initiates certain actual or incipient organic changes, these "are reported back

to the cortex via the beneceptive or nociceptive channels, and the excitations of these channels modify the cortical tendency. If the 'report' is beneceptive or favorable, the tendency in question is enhanced, whereas if it is nociceptive or unfavorable, the tendency is reduced" (p. 216). Surely we have here nothing but a multiplying of terms which are only *assumed* to do the things that we want to have explained. Any old-time faculty would serve as well for this sort of 'explanation.' These actions, we are informed, "can be regarded as being determined quite mechanistically, without reference to any accompanying pleasantness or unpleasantness, or any 'intelligence' on the part of the cortical processes." On this basis several troublesome problems to psychologists—such as the one of how the burned child learns to avoid the candle—are readily 'solved.' Even the 'instincts,' the Shand-McDougall sentiments, and the Freudian complex are readily 'explained' on this general principle. The conditioned reflex principle—which Troland properly says is novel only in the application of the old process of redintegration to purely physiological subject-matter—is then applied to the retroflex mechanism, a center in the thalamus being assumed to serve in a manner entirely analogous to a reflex center. "The path of nerve conduction in the case of a conditioned retroflex would thus be (1) secondary stimulus to cortex, (2) cortex to thalamus, (3) thalamus to cortex." The author assumes that in maze learning the entire maze-trip is represented in the cortex by a coherent associative mechanism, so that as a result of finding and eating the food by the rat the beneceptive retroflex processes will apply to the entire system of neural processes "leading from the last movement retroactively toward the first." This, of course, does not work in case a long route is at first learned and later abandoned for a shorter one. Why, indeed, should the blind alleys not become fixed by increased conductances in the neural tracts, as representative of parts of the true path? Many blind alleys are entered regularly for many trials in the early part of the learning, and thus are part of the course leading to the food.

But these purely behavioristic accounts are nevertheless not satisfying to the author. *His* 'beneceptors' do not relate behaviorism to success! He does not, however, deny the possibility of purely objective or physiological explanations. Purely introspective accounts, on the other hand, he finds unsatisfactory, for "we are apt to flounder in a slough of vague ideas after the fashion of most of the psychoanalysts;" so he elaborates a psychophysical account which again utilizes everything that has been suggested by anyone. Consciousness—'not a subtle, diaphanous entity or process,' but the totality of one's experience at any moment—is correlated with activities going on in restricted areas of the cortex. Introspection, he says later, does not permit us to examine the entire psychical system, even though the conscious experience of an individual is more real than the conceptually developed physical system with which we explain one's 'psychical organism' from the outside. This translation of a physical "into the corresponding, and true, psychical account," gives the essence of the author's *psychical monism*. In bringing out this view the author reveals why he has not been satisfied with the physiological explanation of behavior. He is really a panpsychist and his interests are philosophical rather than scientific, a fact which the reader does not have to go far into the book to find out. He is expounding a system of philosophy. On this view of psychical

monism, we are told, "psychical agencies, such as sensations, perceptions, ideas, purposes, and affections, are causally effective in a full sense. They stand in the same relation to the general 'psychical organism' and universe as do the corresponding factors in the cortical synergy to the physical organism and universe... The real reactions, however, are not physiological or motor, but objective in the psychical sense" (p. 501).

A large part of the book is devoted to hedonism, with the faults, in the reviewer's opinion, that have been indicated in connection with 'explanations' of learning: we seem to fall back into an old-time method of explaining conduct, with some plausible suggestions, to be sure, but with a point of view that is not scientific, and that does not get at the basic causes of action, or of stimuli to action. Troland develops a hedonistic theory based on the assumption that *rate of change* in the conductances of relevent cortical conductors at any given moment of time is associated with the nature and degree of affective tone experienced by the individual. Accelerating changes are assumed by him to be associated with pleasure; decelerating changes in conductance, with displeasure (or unpleasantness, the opposite of pleasantness, a negative value); and no change in conductance, with a neutral affective tone (zero value). This theory he develops mathematically and expounds very clearly. The hypothesis is interesting and *may* be true, but it is not verifiable; nor could it, even if verified, be taken to prove that consciousness is something apart from these conductance changes. The mere mathematics of the situation will be as well satisfied if 'consciousness' is taken to be only *any indication* of these conductance changes; and so the mathematical treatment of the hypothesis proves nothing beyond the present limits of our ignorance; it only clarifies the (in this case) unverifiable hypothesis of the author, as he recognizes. Sensations and feeling states are not entities that take origin in themselves but must be traceable to changes brought about by physical stimuli.

In the author's system of philosophy "happiness would appear to be the dominant desideratum of all structures and changes whatsoever, being the principle whereby all psychical evolution is motivated. This might provide a definite mode of escape from hedonistic individualism, since the individual is a temporary creation of the psychical system at large [whatever that may mean], and is only one stage in the hierarchy of its affectively determined purposes or desires" (p. 502). Thus the scientific reader who started out with high hopes becomes lost in the by-ways of old subjective systems of philosophy, if not indeed in "a slough of vague ideas," and he wonders what was really the value of the long excursion through the hypothetical, physiological speculations.

J. P.

Prinzipien und Methoden der Kunstpsychologie. By PAUL PLAUT. Berlin, Urban & Schwarzberg, 1928. From Abderhalden's *Handbuch d. biol. Arbeitsmethoden, VI. Methoden d. exper. Psychol.*, C, II, 745–966.

In accordance with the general plan of the *Handbuch*, the present monograph gives "a survey of the most important psychological methods in so far as they are of service in illuminating the broad and often almost impenetrable realm of art." The work makes no claim to be either a complete exposition of

such methods, or a psychology of art complete in itself. After pointing out in the foreword the familiar fallacy of those who think by means of a determination of musical and other talent to predict artistic capacity, the author develops his exposition under the following captions.

I. Introduction. Psychology as a limited, special method cannot pretend to grasp analytically the essence of art; it can merely select from a work of art and from the personality of the artist such elements of the problem as are accessible to its merely factual way of regarding matters. For the study of art, psychology is merely auxiliary to aesthetics and art knowledge in general; but the distinction between these disciplines, sociology, and psychology should not be drawn as sharply as is sometimes the case.

II. The Psychology of the Artist. Here belong all methods concerned with the complete personality of the artist, but biography and autobiography are excluded from the methods of art psychology, inasmuch as they evaluate and are lacking in objectivity. Nevertheless the psychologist of art will make an objective use of the material furnished by the biographer for the auxiliary treatment of certain problems. On the other hand methods like those of characterology, psychoanalysis, individual psychology, and graphology deserve special discussion because of their systematic divergence from traditional psychology. The chief psychological method for the study of the artist is psychography which is differentiated from biography by its purely factual non-selective collecting of material. By means of the data brought together by psychography and autopsychography the structure of the artist's personality may be determined. Under this head the methods of Stern, Heymans, Groos, Rutz, Reinhard, Waser, the Freudians, and that of Pannenborg (involving considerations of heredity, school life, biography, etc.) are mentioned, while the method of Margis receives a longer critical discussion. The sample auto-psychographic records furnished here by artists from various fields are of interest, not merely for the light they throw upon the creative processes involved but also for suggestions of a methodological nature involved in their consideration. In the discussion of the questionary of Alrutz concerning the experience of the actor, Plaut emphasizes the fact (of interest also to those using the method in any field) that "it is not advisable to bring up (in such questioning) detailed, involved problems and thus to confront the artist with difficulties which he will face only reluctantly, if indeed he does not utterly refuse to answer." If a certain freedom from limitations in answering is given, more fruitful results than were anticipated are often obtained. With respect to the problem raised by Lombroso, Moebius, and others, Plaut is of the opinion that attempts to trace the creative process to a specific somatic origin overlook the real problem involved, while he agrees with Müller-Freienfels that it is equally incorrect to speak of a "pathological art." In this connection he contrasts the antithetical views of Jaspers and of Riese with respect to Van Gogh.

III. Characterological Methods. In general agreement with the cautious attitude of Utitz, characterology is distinguished by Plaut from psychography by the fact that the former method is concerned, not with the mere assembling of as large a number of facts as possible, but rather with the determination of traits of character, *Anlagen*, etc., "which constitute the essence of the personality and are therefore not purely psychic phenomena." Characterology forms

the preliminary method for a typology, such as is found in the work of Stern and Utitz. Here Plaut calls attention to the fact (noted by Müller-Freienfels) that nearly all recent investigations in characterology have come from thinkers, like Spranger, Klages, Kerschensteiner, etc., who have felt themselves somewhat at variance with traditional technical psychology. With respect to the graphological work of Klages and others (which attempts to determine traits of personality through the study of handwriting) Plaut finds the methodology in an undeveloped state of limited value for the psychology of art, although he admits that it is the merit of Klages to have shown that the determination of personality "can not belong to the realm of traditional technical psychology." The psychoanalytic method, and the individual psychology of Adler, the last discussed in this group, are thought by Plaut to have some value as auxiliary methods, but he has little sympathy with the narrow dogmatism and aberrations of the school.

IV. Methods of Applied Psychology. Here are discussed the methods for investigating such problems as memory, talent, and aesthetic sensibility. Recognizing clearly the problematical nature of talent in general and the doubtful significance of experimental investigations of children for the determination of artistic capacity (which includes in any case more than mere talent), Plaut regards the problem of the musical individual as an unsolved one, and notes also many divergent attempts to formulate the concept. He quotes Pear to the effect that the difference between musical and unmusical persons is only one of degree. Completion tests in melody, and experiments in memorizing music are the next subjects treated in this section. The *Lesemethode*, or musical practice through reading has an advantage over the *Spielmethode* or practice on the instrument. A discussion of the methods used for the determination of aesthetic appreciation in children and adults, followed by an exposition of those used in the study of creative activity in literature and drawing is the last one in this group.

V. Musical Experience as a Psychophysical Problem. Under this caption is included a survey of all methods concerned with the correlation of musical experience and its physiological conditions, with especial reference to the significant results of Roemer, and of Huber (which are discussed in some detail) together with studies of motor phenomena and musical therapy (Baerwald, Singer, Hessge).

VI. The Ethno-Psychological and Sociological View of Art. Under the first part of the caption is given an outline of methods for the study of art with primitive peoples. The sociological method, which the author in agreement with Müller-Freienfels regards, not as an independent method, but merely as the attempt to study art in connection with the psychological tendencies of a given epoch as a cultural phenomenon having a specific epochal character, occupies the closing pages of the work.

The discussions of the book are throughout succinct and clear, the material is well organized, and the work is in general comprehensive and of fundamental importance for students of the psychology of art.

Vanderbilt University HERBERT SANBORN

Aptitude Testing. By Clark L. Hull. Yonkers, World Book Co, 1928. Pp. xiv, 535.

This book seeks to present two of the essentials of training for aptitude testing: an account of the fundamental principles involved, and a clear and intelligible description of the most effective and economical methods of constructing batteries of aptitude tests. It is the product of years of experience by the author in this particular field of psychology.

After making a brief survey of the early developments in testing, defining clearly and illustrating the meaning of such terms as 'individual differences,' 'trait differences,' 'work-limit,' and 'time-limit' methods, and describing different kinds of tests and devices for their application, the author takes up the question of anatomical and other alleged 'signs' of aptitude. An aptitude sign is defined as simply a more or less reliable indicator of aptitude behavior, not a sample of it as is an aptitude test. Physiognomy, an ancient claimant for recognition as an anatomical sign of character and aptitude, is considered. The validity of character judgments based on photographs is challenged, the results of certain unpublished master's theses by the author's students being offered as additional evidence. It is also shown that viewing the subjects in three dimensions, as exhibited on a stage, has no special advantage over the mere showing of photographs. Objective data likewise give little encouragement to beliefs that complexion or physiognomy are revealers of character traits, though certain head-measures do offer slight correlations with university grades. A combination of these by multiple correlation methods have a correlation as high as 0.50. Academic scores showed little relation to height or weight, or to Kretschmer's physical types (asthenic, athletic, and pyknic), and there was scant evidence among normal subjects of an association of his cycloid temperament (manic-depressive) with the pyknic type, or of his schizoid, nonaggressive temperament with the asthenic type. Moreover, for Naccarati's theory—that persons who in the early 20's are relatively slender have greater intellectual capacity than those more stocky in build, particularly in the trunk region—little evidence is found. The claims for chirognamy (hand-reading) and chiromancy (palm-reading) as revealers of character traits are shown to be ill-founded, and handwriting fares very little, if any, better.

Different methods of aptitude prognosis from raw test-data—letter scales, percentiles, I.Q., S.D. units of deviation from the mean, T-scores, etc.—are considered, as well as means of converting such scores into data which reveal aptitude possibilities. Of these means the multiple regression equation is found to be the only really adequate procedure. The technique of this method is briefly and simply explained. The author estimates, after some calculations and discussion, that the respective contributions toward success of (1) capacity or ability (presumable innate), (2) industry or 'willingness,' and (3) chance or accident are in this order approximately 50, 35, and 15%. This is given as a mere estimate and we do not need to stop to consider its probable accuracy, or even the meaning of each of these factors. Incidentally it may be mentioned that by an accident of inversion 1.8426 is given in the note on page 170 instead of 1.4826 as the ratio of σ to P.E.

Unusually clear and short presentations of the extreme general-factor and the extreme individual-factor theory of intelligence, as well as of the inter-

mediate (Thomson) theory of a combination of group and specific factors, are given. These are illustrated in tabular distributions which show by a check-method mere presence or absence of certain factors (or 'determiners'), whether specific or general, in various assumed aptitudes, A, B, C, D, etc. The method is admittedly a rough one, in as much as it must assume each determiner to be specific and either present or absent in a sort of all-or-none fashion, and also to remain constant in the individual in different situations and through successive stages of development. It seemingly must likewise assume a constancy of determiners as we pass from one individual to another. As a result of this the general exposition gives an impression of an individual as a mere sum of factors so far as intelligence and aptitudes are concerned, a sort of quantitative, composite view of intelligence.

This is, of course, a rather general fault of our present-day mathematical methods in psychology and education: our *x*s, *y*s, etc. are assumed to represent certain aspects of mental functioning, and these aspects must therefore be *constants* even in vastly different combinations of organic mechanisms—an obvious absurdity from the biological standpoint, which we are forced more and more by the actual facts of behavior to recognize, though not from the older conception of mind as composed of arbitrary and disparate faculties. The effect upon the lay reader, however, of this tabular representation of factors or determiners is that he gets a very much clearer understanding of the different theories of intelligence and of their general implications than he can probably get from any other book now available to him. In most textbooks this situation is found to some extent: in order to clarify the exposition for the initiates we so simplify the situation that it becomes really untrue as representative of the bit of reality we try to study. All calculations of 'practical judgment,' 'arithmetical ability,' 'facility in the use of language,' or what not, are subject to this same criticism. These are not constant entities under different situations for the same individual or even in similar situations for different individuals despite the fact that they are measured in terms of the units of certain 'objective' tests. These difficulties with over-simplification of text-book expositions are, of course, clear enough to the author, whose broad, practical experiences in the field of aptitude testing are so well reflected in the book as to make it an excellent outline for class-room purposes. The student can hardly miss being influenced by the fine enthusiasm of the author for research, as reflected in such statements as this: "One of the most fascinating problems in the world is that concerning the ultimate causes of human success and failure" (p. 184).

Chiefly because of lack of space, evidently, Spearman's later developments in his theory are not well brought out, but it is indicated that this theory is not now strictly a two-factor theory as in the earlier statements. It is briefly mentioned that both the recent empirical facts and the mathematical developments bearing upon the nature of intelligence are tending toward a position between the extreme theories, which recognizes the existence of a number of overlapping 'group' factors which are found in various related mental functions and are possessed of different degrees of generality. Strictly special factors are finding little support. It is also pointed out that Thorndike's view regarding the existence of a mechanical, a social, and an abstract type of intelligence has

not found much support in research. The author brings out the fact that neither of the extreme theories offers any basis for real aptitude prediction, and he offers his own theory, which agrees with Godfrey Thomson's in rejecting a strictly universal factor running through all human activities, and which, in opposition to either of the three theories mentioned, rejects all specific or really unique factors. This theory also does not limit the more general to strictly "intellectual" factors—whatever they may be.

The so-called normality of distribution of mental traits is probably assumed too uncritically throughout the book. In calculating from the various data the degree of variation in abilities, moreover, no allowance is made for the fact that actually obtained distributions on a single test are too wide and that they must be corrected for chance factors. The ratios of poorest to best scores, as shown, for instance, on p. 33, are therefore misleading, as are also conclusions drawn from them. There is consequently too strong a tendency to take as indicative of aptitude differences the differences found in obtained scores, and to neglect effects on scores of training and of environment generally. Reliability measures of obtained individual aptitude-scores are probably not considered adequately. These omissions are presumably in the interest of clearness and simplicity of treatment, but they tend to develop in uncritical readers certain erroneous conceptions which should be avoided as much as possible. It must be added here, however, that the limitations of accurate prediction of success by means of multiple regression equations are clearly and graphically presented.

The last 250 pages of the book are devoted to descriptions and illustrations of scientific methods by which batteries of aptitude tests are constructed, and to various helpful devices in scoring and handling data. In four appendices are given tables for the converting of ranks into linear S.D. scores for numbers as high as fifty, for finding the values of $1 - r^2$, and $\sqrt{1 - r}$, and tables of squares and square roots of numbers to 1000, and of multiples to 100×100.

J. P.

Henri Bergson. By Jacques Chevalier. Translated by Lillian A. Clare. New York, The Macmillan Co., 1928. Pp. xxi, 351.

Chevalier's work consists of a reworking and translation of his lectures on Bergson at the University of Grenoble. Its aim is to give guidance to the further intelligent study of the philospher, and to record the appraisal of a master by his very ardent disciple. The topics selected for treatment in successive chapters do not pretend to be exhaustive; yet they give an adequate view. They are: The Milieu and the Period; The Man and the Work; The Method; Intuition and the Philosophical Mind; The Immediate Data of Consciousness; Matter and Memory; Evolution and Creation; The Trend of Bergsonian Thought. Of these the third and fourth chapters, and possibly the fifth, are of importance to psychologists.

The defects of the book appear first and consequently it is simpler to treat them first. It would be advisable for the reader who wishes to get the very genuine benefits of the chapters recommended above to skip the first two chapters of the book, because they fail in the first requisite of philosophical, or even quasi-philosophical, exposition. They lack judicial calmness, dispassionate detachment, and cast an unmerited suspicion on the soundness of

mediate (Thomson) theory of a combination of group and specific factors, are given. These are illustrated in tabular distributions which show by a check-method mere presence or absence of certain factors (or 'determiners'), whether specific or general, in various assumed aptitudes, A, B, C, D, etc. The method is admittedly a rough one, in as much as it must assume each determiner to be specific and either present or absent in a sort of all-or-none fashion, and also to remain constant in the individual in different situations and through successive stages of development. It seemingly must likewise assume a constancy of determiners as we pass from one individual to another. As a result of this the general exposition gives an impression of an individual as a mere sum of factors so far as intelligence and aptitudes are concerned, a sort of quantitative, composite view of intelligence.

This is, of course, a rather general fault of our present-day mathematical methods in psychology and education: our *x*s, *y*s, etc. are assumed to represent certain aspects of mental functioning, and these aspects must therefore be *constants* even in vastly different combinations of organic mechanisms—an obvious absurdity from the biological standpoint, which we are forced more and more by the actual facts of behavior to recognize, though not from the older conception of mind as composed of arbitrary and disparate faculties. The effect upon the lay reader, however, of this tabular representation of factors or determiners is that he gets a very much clearer understanding of the different theories of intelligence and of their general implications than he can probably get from any other book now available to him. In most textbooks this situation is found to some extent: in order to clarify the exposition for the initiates we so simplify the situation that it becomes really untrue as representative of the bit of reality we try to study. All calculations of 'practical judgment,' 'arithmetical ability,' 'facility in the use of language,' or what not, are subject to this same criticism. These are not constant entities under different situations for the same individual or even in similar situations for different individuals despite the fact that they are measured in terms of the units of certain 'objective' tests. These difficulties with over-simplification of text-book expositions are, of course, clear enough to the author, whose broad, practical experiences in the field of aptitude testing are so well reflected in the book as to make it an excellent outline for class-room purposes. The student can hardly miss being influenced by the fine enthusiasm of the author for research, as reflected in such statements as this: "One of the most fascinating problems in the world is that concerning the ultimate causes of human success and failure" (p. 184).

Chiefly because of lack of space, evidently, Spearman's later developments in his theory are not well brought out, but it is indicated that this theory is not now strictly a two-factor theory as in the earlier statements. It is briefly mentioned that both the recent empirical facts and the mathematical developments bearing upon the nature of intelligence are tending toward a position between the extreme theories, which recognizes the existence of a number of overlapping 'group' factors which are found in various related mental functions and are possessed of different degrees of generality. Strictly special factors are finding little support. It is also pointed out that Thorndike's view regarding the existence of a mechanical, a social, and an abstract type of intelligence has

not found much support in research. The author brings out the fact that neither of the extreme theories offers any basis for real aptitude prediction, and he offers his own theory, which agrees with Godfrey Thomson's in rejecting a strictly universal factor running through all human activities, and which, in opposition to either of the three theories mentioned, rejects all specific or really unique factors. This theory also does not limit the more general to strictly "intellectual" factors—whatever they may be.

The so-called normality of distribution of mental traits is probably assumed too uncritically throughout the book. In calculating from the various data the degree of variation in abilities, moreover, no allowance is made for the fact that actually obtained distributions on a single test are too wide and that they must be corrected for chance factors. The ratios of poorest to best scores, as shown, for instance, on p. 33, are therefore misleading, as are also conclusions drawn from them. There is consequently too strong a tendency to take as indicative of aptitude differences the differences found in obtained scores, and to neglect effects on scores of training and of environment generally. Reliability measures of obtained individual aptitude-scores are probably not considered adequately. These omissions are presumably in the interest of clearness and simplicity of treatment, but they tend to develop in uncritical readers certain erroneous conceptions which should be avoided as much as possible. It must be added here, however, that the limitations of accurate prediction of success by means of multiple regression equations are clearly and graphically presented.

The last 250 pages of the book are devoted to descriptions and illustrations of scientific methods by which batteries of aptitude tests are constructed, and to various helpful devices in scoring and handling data. In four appendices are given tables for the converting of ranks into linear S.D. scores for numbers as high as fifty, for finding the values of $1 - r^2$, and $\sqrt{1 - r}$, and tables of squares and square roots of numbers to 1000, and of multiples to 100×100.

J. P.

Henri Bergson. By JACQUES CHEVALIER. Translated by Lillian A. Clare. New York, The Macmillan Co., 1928. Pp. xxi, 351.

Chevalier's work consists of a reworking and translation of his lectures on Bergson at the University of Grenoble. Its aim is to give guidance to the further intelligent study of the philospher, and to record the appraisal of a master by his very ardent disciple. The topics selected for treatment in successive chapters do not pretend to be exhaustive; yet they give an adequate view. They are: The Milieu and the Period; The Man and the Work; The Method; Intuition and the Philosophical Mind; The Immediate Data of Consciousness; Matter and Memory; Evolution and Creation; The Trend of Bergsonian Thought. Of these the third and fourth chapters, and possibly the fifth, are of importance to psychologists.

The defects of the book appear first and consequently it is simpler to treat them first. It would be advisable for the reader who wishes to get the very genuine benefits of the chapters recommended above to skip the first two chapters of the book, because they fail in the first requisite of philosophical, or even quasi-philosophical, exposition. They lack judicial calmness, dispassionate detachment, and cast an unmerited suspicion on the soundness of

that part of the book which treates of Bergson's philosophy proper. This unhappy emotional fervor is the result of two facts: first, the author is the pupil, friend, and admirer of Bergson, and second the text has not been sufficiently reworked to eliminate the savor of a vivid lecturer, speaking to foreigners in a holiday course, who aims to exalt not only Bergson the philosopher but also Bergson the Frenchman. One can understand and endure, although not necessarily respond to, the pupil's enthusiasm for the master. But what one cannot help finding distasteful is the deliberate emphasis on the alleged purely Gallic character of Bergson's philosophy. Why, even if Bergson's philosophy were untouched by foreign influences, this purity should be commendable, is not made clear; nor is it altogether clear why the praise of this purity should be couched in terms so chauvinistic and so anti-German as to appear revolting to common sense and foreign to the philosophical attitude which presumably is unconcerned with nationalism. Chevalier has thought it necessary to make Bergson, even as a philosopher, a good Frenchman first. To him, Bergson has heroically liberated French thought from the trammels of odious German philosophy even as Foch liberated the land from the invader; and to have bothered with Kant was to have been guilty of high treason. Such a manner of garnishing the subject may have been suitable to foreigners who are frequently more French than the French, but it is ill-advised in a book which is designed for scholarly use. There is still another reason for objecting to the parochial attitude of such treatment, and that is that neither in his roots in the past nor ramifications in the present is Bergson so purely French as Chevalier would like to have us think. If the author had taken the trouble to read Thomas Brown, the Edinburgh philospher of the early nineteenth century, and Bain, he would have found explicitly much and implicitily even more of what appears in Bergson. Furthermore, if he can so far surmount his distaste for things German, as to become familiar with the doctrine of *Gestalt* psychology, he will see that the same tendency that has produced a Bergson in France has produced a Scheler, a Köhler, and a Wertheimer in Germany. Far from being purely French, Bergson's intuitionism, for example, finds, if Chevalier were only aware of it, its strongest scientific experimental support in the work of such German psychologists as Wertheimer, Köhler, and Gelb. Philosophers and scientists plow in many fields, and here we have an instance of the danger of attempting to confine a philosopher, even with his own consent, within narrow nationalistic limits.

It is a pleasure to call attention to the third and fourth chapters which contain much of value to the psychologist, for here, particularly if he is interested in the *Gestalt* doctrine, he will find a broad speculative field in which to arrange the experimental findings of *Gestalt* laboratory work. The usefulness of these chapters lies in the fact that they are a condensed summary of a variety of articles which Bergson has scattered in a number of philosophical periodicals over a term of years, and they have not been too greatly refracted in passing through the medium of the author's mind. In selecting and compressing, he has naturally permitted his affinitive memory to retain that which is in profound accord with itself; but as the two men are entirely in intellectual sympathy, it is probable that the spirit of these articles is rendered in its entirety and with its own atmosphere.

In the third chapter, the one on method, the basic consideration is that the concept of time must be liberated from the debasing and distorting influence of mechanistic philosophy which has brought it into contact with space. True movement and real duration must be isolated from space. By intuition first and later by more demonstrable data Bergson was able to show the opposition between spatial time and duration, or rather the contradiction in duration represented spatially. This duration, Bergson insists, is intuitively felt as the representation of many reciprocal penetrations and pervasions, altogether different from the numerical multiplicity of space. Or put differently, duration is qualitatively basic and independent of all other facts of experience, just as the color red. What Bergson finds objectionable in mathematical-science is that it reduces movement to something other than itself and substitutes for real duration, the stuff movement is made of, a symbolic image derived from extension in space. Thus it measures movement by bringing it to a standstill, as it analyzes life by killing it. The observing consciousness, differing in this respect from mechanistic science, captures movement in itself, in its inner reality, by ignoring the spatial symbols interposed between reality and ourselves. The principle upon which this is based is that one's intuition of an event or fact is as real as the results of deliberate analysis, and that intuition is the proper basis for apprehending life and movement and duration, while analysis is the proper one for representing that which can be treated mechanistically, namely the inert, the inorganic. Clearly for the psychologist familar with *Gestalt* doctrine this view involves no difficulties.

The fourth chapter, dealing with the immediate data of consciousness, develops the theorem that intuition is not short of intelligence but ahead of it, intelligence being understood as the process involved in analytical operations such as mathematics. Bergson insists upon the superiority of intuition, or the immediate data of consciousness, because all philosophy, whatever it may be, must start from these data. They are immediate and direct, seizable at a glance by intuition, without the middle term which, as Aristotle would say, analytical thought always and necessarily makes use of, and consequently freed as far as possible from all that does not proceed from the object itself.

The remaining chapters, while interesting to the philospher, would probably have less interest for the psychologist. Being purely speculative constructions with little or no opportunity for the application of the experimental data of psychology, they have less value than the third and fourth.

University of Louisville ELLIS FREEMAN

Crossroads in the Mind of Man: A Study of Differentiable Mental Abilities. By TRUMAN L. KELLEY. Stanford University, Stanford Univ. Press, 1928. Pp. vii, 238.

This is a highly technical treatment characterized not so much by investigating differentiable mental abilities as by exploring the possibilities and extent of such differentiation, but it makes important contributions in both respects. The experimental psychologist will demand more evidence than the present statistical treatment affords before he will concede that the 'abilities' here dealt with are separable, rather constant and objective entities and that

they are not merely relative to the particular tests and data here used; and he will demand that additional evidence must be experimental, based on additional subjects and tests in rather different circumstances. The author himself recognizes his contribution to technique as of more importance than the specific findings of this investigation, as to which traits are differentiable, but he does not regard his contributions as to possible differentiable traits as being unimportant.

The first chapter is entitled The Boundaries of Mental Life and the Technique of Their Investigation, and involves the author's own valuable statistical contributions of more than a decade as well as the work of those who have stimulated him. The corroboration of certain of the author's own results, which he finds in Spearman's *The Abilities of Men* (1927), must not be taken too readily as evidence of objective validity without due consideration by later critics of the somewhat similar elements of technique involved in part of Kelley's work, especially since the main contribution is one of method of procedure (largely statistical) rather than objective experimental data. This statement does not in any sense imply that the technique is to an appreciable extent faulty. That is a question to be decided by additional experimental work.

In measuring any alleged ability the first step is to find out just what we are measuring and to isolate its occurrence from all such variables as age, specific training including certain 'sets,' general mentality, present social influences, etc. none of which is a constant physical entity. Even age, for example, is but a statement that certain changes have gone on for so many years, but these changes of growth are neither constant through the various periods of the same individual nor in different individuals at the same age, and therefore controlling age (as time lived) may not give the desired homogeneity as to degree of development. One danger in a study like the present one is to name and define some conceivable independently functioning process or trait and then to build up calculations on the assumption that it is as constant as the name or symbol by which it is designated. Kelley's method of iteration will doubtless tend to safeguard against this danger of error.

Verification by several wholly independent workers is the next step confronting any such isolation of a trait or traits. This is not usually an easy matter because any specific result is so set in (or relative to) various peculiar and more or less fortuitous circumstances as to make it extremely difficult, even by results of numerous such tests or measurements, to establish the objectivity and universality of the data secured. Here is, of course, the opportunity for statistics to come into play, and any contributions toward safe methods of bringing together mathematically these various elements in the different test-situations and of forcing them to yield up their consistency and independence of any particular circumstance may be as valuable finally as they may have to be tentative now. One gets the impression in going over the statistical parts of the present book that there are often rather too many indeterminate factors to establish the uniqueness of any result now, in view of the probable error-limits of the measurements by the tests on which the caculations are based.

There has been much discussion as to the validity of Spearman's criteria for the test of his two-factor theory and as to whether the general factor, *g*,

would not vanish if absolute homogeneity with respect to age, race or family strains, practice, sex, etc. were maintained. Not only have certain authorities held that several overlapping group factors might meet the criteria as to the existence of this *g*-factor and a number of specific factors, but even Spearman's logic as to his theory has had to yield somewhat on rather slim empirical evidence (as supplied by him) to the probability of at least two other general cognitive factors—'oscillation' and 'perseveration'—as well as of a number of group factors like 'memory,' 'space factor,' 'conjugation factor,' which Kelley thinks is probably identical with his own derived 'number factor,' 'conative factor,' and others. Spearman's tools seem to be found somewhat inadequate by Kelley in his own more empirical procedure. Kelly is not inclined to arrange tests so as to give results in hierarchical systems or to avoid getting overlappings. There is no doubt, moreover, that the method of partialing out the *g*-factor, or any other assumed general factor, as has been done, by partial correlation procedures is based on too many contingencies operating successively in the calculations as well as on errors in assumptions of constancy in certain of these factors, to be taken with the same confidence that results by experimentally controlled procedures would warrant. For instance, holding the age-factor constant by such procedures is a most unwise thing to do. Kelley has avoided such methods here.

Despite certain shortcomings of the tools used by Spearman, Kelley believes that this pioneer in statistical methods has used these tools "with rare judgment and has determined the existence of many important mental factors" (p. 15). Kelley has extended the Spearman technique of tetrad differences which he holds is the only alternative to the introspective method (p. 7) in dealing with "purely mental phenomena" (whatever these may be), and he has gone beyond this technique and has stated (and worked out proofs of) a number of basic 'propositions' relating to the possible presence in certain given tests of one or more underlying traits, whether general or specific. In addition to this he has also derived certain much needed formula for probable errors. The equations supporting these propositions may not be wholly conclusive yet, but this remains to be found out. Kelley does not recognize such inconclusiveness here, though he does in the case of his iteration method, to be considered in the next paragraph. He invites, however, both mathematical and empirical tests of them. On the mathematical side it may be questioned whether the solutions by this technique can be made unique. There may be too many interdependent circumstances for the equations to yield definite solutions, even if we assume absolute reliability on the part of the tests used. Kelley has acknowledged that his important Proposition 15, is in error (*J. Gen. Psychol.*, 2, 1929, 169 ff.). The work of developing these tentative equations seems, however, to be of much importance as a contribution toward a method of future research into the nature of human abilities and their determination.

Kelley next proceeds by quite a different technique to develop special methods. Taking data from as many as nine different tests—reading (speed and power), arithmetic (speed and power), memory (of words, numbers, both meaningful and meaningless symbols), and space relations (power)—on 140 seventh-grade and on 110 third-grade children, besides other tests on kindergarten children, all yielding no appreciable sex-differences, he finds by an

empirical iteration method six *general* factors or traits and nine *special* chance and non-chance factors as follows: (a) general—one factor that is some function of heterogeneity, maturity, race, sex, etc., and besides this a verbal, a number, a memory, a spatial, and a speed factor; (b) special factors—x_1, x_2. . . . x_9. These he tabulates with the general factors in columns and the tests and special factors in rows, and in the 'cells' common to pairs of general factors and tests he makes first preliminary and finally (by the method of least squares) more exact estimates of the standard deviations required to account for the respective correlations yielded. These estimates are then compared with actually obtained reliabilities in an attempt to interpret the obtained correlations in terms of 'bonds' between the different pairs of variables. A comparison of the correlations actually obtained with those which would result from the presence of such factors as were tentatively worked out earlier shows an agreement so "decidedly great" as to be promising for the method. The agreements of factors found in the 'populations' already mentioned (kindergarten and third and seventh grades) is also rather close. The author, on the basis of these comparisons of results from the three different 'populations,' concludes: "That the following traits, (1) facility with verbal material, (2) manipulation of spatial relationships, (3) memory, are independent categories of mental life from a very early age (probably from birth) seems hardly open to question. The sensing and retention of geometric forms should probably also be included in this list, also a number factor, because of its indubitable presence in the third- and seventh-grade populations, for it should be recalled that no test for this factor was made in the case of the kindergrarten group" (p. 149). It can, of course, be maintained that this empiral procedure is hardly determinative even after very great amounts of work with different tests on various subjects, because one can never be quite sure of having the arrangement that will yield the very best fit. Kelley so admits.

The book is not most happily written from the standpoint of ease of understanding, especially in view of the fact that a large percentage of psychologists are not well trained in abstruse statistical calculations. But the reader must appreciate the impossibility of presenting full data along with discussion as it advances. The reader must often turn ahead of his reading for the illustrative data. In a number of cases the procedures followed and the intermediate steps are not fully given, and the reader is asked to "take on faith the steps followed in securing the initial factor values for this seventh-grade population of 140 and to judge of the general possibility of securing such values by studying the procedure of the later chapters" (p. 99). Any sympathetic reader will readily imagine the immense difficulties connected with the presentation to psychologists with their present degree of neglect of statistical training a piece of work of this character, although he will also recognize the need of getting the essential procedures and criteria of testing somehow "put across" to the body of more empirical and less statistical experimentalists, so that they may not only try out the procedures but use them either as they are or as finally modified toward the building up of a body of established facts about intelligence and individual traits. This book must be regarded as presenting a piece of pioneering work of major importance to psychologists, first as to methods of procedure in researches in this field and second as to results obtained.

J. P.

Comparative Neurology: a Manual and Text for the Study of the Nervous System of Vertebrates. By James W. Papez. New York, Thomas Y. Crowell Co., 1929. Pp. xxv, 518.

This volume is a valuable reference work for the physiological psychologist. The book is divided into three major sections. Part I contains a consideration of the gross structure of the brains of mammals. In this section the cerebrum, cerebellum, brain stem, medulla, thalamus, spinal cord, and related parts are considered as they appear in the lower mammals, in the cat, the dog, and the primates. Part II contains a consideration of the microscopic structure of the mammalian nervous system. Among the important topics treated in this section are the sensory endings, nerve trunks, fine anatomy of the cord, the sympathetic system, the internal structure of the various parts of the brain, the reflex arc, and functions of the cortex. Part III contains a consideration of the brains of the lower vertebrates. The brains of reptiles, birds, amphibia, fishes, cyclostomes, and of Amphioxus are considered. The volume ends with a treatment of the phylogeny of the forebrain.

The volume is in many ways an admirable piece of work. The exposition is clear, many important references are given to fundamental papers, and the diagrams, many of them original, are models of their kind. The book, however, suffers from one inadequacy. Like many other treatises upon the nervous system it is satisfactory in general in its treatment of the structure of the system, but it leaves much to be desired in its treatment of the function of the nervous system. More space is given to the tergamentum of fishes than to the nature of the nervous impulse. Limitations of space and of interest may well lead to a lack of emphasis on function, but if references to function are included at all, an effort should be made to make them of the same scientific validity as the rest of the work. This is not the case in the present volume. Particularly in regard to the vocabulary used in the treatment of the functions of the cerebral cortex is this inadequacy apparent. The author makes little distinction between words descriptive of the external physical stimulus, the physiological fact of the operation of the response mechanism, and the terminology of introspective psychology. A few examples may make this point clear. "This system of spino-thalamic fibers conducts cerebralward the impulses of pain, heat, and cold" (p. 177). "That is, psychic activity, though it normally involves both hemispheres, is not essentially bilateral" (p. 358). "This means that the various tracts of deep sensibility which bring in sensations from the various regions of the body have each a definite locus of termination in the post-central gyrus" (p. 364). The use of the word *sensation* in the above sentence is not clear, particularly when we note that a few pages further on the word *stimulus* is used in the following manner: "These studies have thrown direct light on the analysis of different stimuli in the cortex and the associations that may occur between them under definite conditions as well as cortical inhibition" (p. 375). As part of the syndrome of zone D of the cortex we are told, "Intelligence is diminished" (p. 373). Similarly in the consideration of ideokinetic apraxia the assertion is made that "simple movements are well performed except when patient wills to do them" (p. 374).

It seems to the reviewer that the time is ripe for the neurologist to acquaint himself with the accepted meaning of terms in psychology, and the known

facts of psychology when he essays to write upon matters concerning mental life. Strange as it may seem the psychologist is in general more scrupulous in his use of the neurological facts than is the neurologist in his use of psychological facts.

Brown University LEONARD CARMICHAEL

Une psychologie objective est-elle possible? By E. AUGIER. Paris, Félix Alcan, 1928. Pp. 290.

In the earlier part of his book Augier criticizes the older, subjective, introspective psychologists who are at present, he says, the predominating group. Psychology under their guidance has come to a state of stagnation (p. 13). Reducing his criticism to its simplest terms it will be seen that most of the faults he imputes to them are connected with a single larger fault; their failure, due perhaps to a lack of training in the sciences, to adopt a clear conception of science. They have not regarded the individual in the scientific biological sense as an organism set in time and space, constantly changing with the changes about it, constantly renewing its very tissue; but they have regarded him in a peculiar way, as something invariable in time and space (p. 61, 70), as the abstract 'I' who has sensations, images and the like. The older psychologists did not say, 'I see,' as though the 'I' were doing something, were undergoing a change which the biological psychologist could study in its spatial and temporal relationships; but they said, "I have a sensation," and then they studied the sensation and not the individual (p. 79-80). Thus these sensations, images and the like, in being studied, are detached from the changes in the individual, from the world of time and space, and are associated with this abstract, unchanging 'I'. The subject-matter of psychology in thus being separated from the world of time and space becomes separated from the other sciences, loses contact with them and therefore lacks the aid which such contact could bring.

But Augier's criticism of the older psychologists, harsh as it may seem, does not amount to a complete repudiation of them. Just as they, he accepts consciousness as the principle source of information (p. 14); and he proposes also to investigate consciousness by the introspective method. But his introspection, he believes, will in one important respect be different from theirs. Whereas they simply reported the results of their investigations in the subjective language of sensations, images and the like, he will, in investigating a conscious experience, seek to discover what is happening physiologically, and to report his results in objective terms, that is, terms involving the elements of time and space (p. 32), in physiological terms.

In the second part of his book the author illustrates his method by the study, as in the case of admiration, of emotions, sensation, image, memory, intelligent act, abstraction, pleasure, pain, desire, and a few other conscious elements. His results are not startling. He finds that fear is essentially the excitation of the motor reflex arcs used in running, anger the reflex arcs used in attacking; in both cases the excitation does not result in the complete action of the external muscles (p. 130-133). In most of his explanations, using many

neurological hypotheses, he simply gives us the most plausible explanation he can of what is happening physiologically when we are having those conscious experiences.

The author attempts to describe the conscious aspect of the individual in terms of the physiological aspect of the same individual, that is, he attempts to describe the individual in terms of himself, not in terms of his relation to his environment. Thus, despite his repudiation of the old conception of an isolated individual, he has not broken free from the influence of that conception.

Despite certain of his progressive attitudes, his dissatisfaction with the older psychology, his insistence that any science must primarily serve life, his assumption that a human being is understandable in physical terms, his attempt to incorporate psychology into the rest of science—despite these attitudes—Mr. Augier has failed to break with the past. His is still a psychology of the armchair—only within easy reach is a textbook on physiology.

Ohio State University ALBERT P. WEISS

Morton Prince and Abnormal Psychology. By W. S. TAYLOR. New York, D. Appleton and Co., 1928. Pp. 137, 1 pl.

In this brief volume the author attempts to synthesize, interpret, criticize, and supplement the contributions of the late Morton Prince in the fertile but poorly cultivated fields of abnormal psychology.

Impelled by the belief that Dr. Prince has been inadequately pictured as "(1) 'a dissociationist,' (2) whose views are 'not dynamic,' but (3) who marks a 'significant step in the development of abnormal psychology,' and (4) whose findings are still very important in a specific and rarely met type of hysteria called multiple personality," Taylor comes to his defence and bases his book upon "Morton Prince's considerable body of writings; upon observations of students' reactions to his formulations and to other authors' points of view; upon various investigations of functional abnormalities in the light of these views; and, especially, upon recent and detailed discussions with Dr. Prince himself."

The fact that Dr. Prince, in the flesh, has gone makes this work all the more valuable. His students, of Professor Taylor's calibre, have been too few. But his brilliant hypotheses, his wide and accurate observations, and his hopes for abnormal psychology, all conspire to make a synthesis of this sort difficult. Of Prince, as of few men, can it be said that his abnormal psychology was not handicapped by a lack of first-hand acquaintance with the abnormal nor yet by a failure to understand or a prejudice against scientific psychology. Hence, whether or not Taylor's efforts are too much influenced by his own interpretations, and whether or not Prince's brilliancy was accompanied by such inconsistencies as make synthesis impossible, the fact remains that this work sets out to do what must be done for abnormal psychology.

The principle virtue of this work lies in its potential stimulative value to the psychologist, well grounded in scientific psychology and widely acquainted with abnormality, who will do for himself what Taylor has tried to do for Prince—*i.e.* construct a coherent, useful, and respectable system of abnormal psychology.

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AN EXPERIMENTAL STUDY OF OBSERVATIONAL ATTITUDES

By O. D. ANDERSON, Cornell University[1]

The importance of the exact state, temper, and condition of the psychological observer has long been realized. It is now generally understood that the results of descriptive, as well as of psychophysical, experimentation must carefully take into account the precise way in which the observer conceives of, and enters upon, his task. Furthermore, the experimenter cannot safely assume that his *O*s actually adopt and constantly maintain the experimental posture which is called for in the formal instruction. In the first place, certain *self* instructions inevitably color the performance (*e.g.* "I am easily distracted in this sort of experiment," "I *must* find a difference between those sounds"). And again, the occasion or setting itself always instructs ("That 's' obviously calls for transposition." "The light went off too suddenly"). That is *occasional* instruction. It is very insidious and powerful. It may have a greater effect upon the functional outcome than the formal instruction itself. Either the 'self' or the 'occasional' form may be related to a variety of attitudes and may lead on to a consequent diversity of experimental results.

We shall understand by *observational attitude*, in this context, a way-of-taking a task and a mode-of-adjusting oneself to an experimental setting.[2] It may involve anticipation or intent, and it

*Accepted for publication July 1, 1929.

[1]From the Psychological Laboratory of Cornell University. The research was directed by Professor Bentley.

[2]Not *Bewusstseinslage*, therefore, and not bodily disposition under automatized performance.

will presumably rest, in part, upon *O's* comprehension of the formal instruction and, in part, upon any kind of non-formal instruction or hint which arises from the *O* himself and from the occasion. Ultimately, of course, the observational attitude will have one strong root in the past history of the organism; but we shall try to discover and to describe the attitudes themselves through direct inspection and report and not substitute for them a doubtful and speculative explanation of them in the loose terms of 'past experience,' 'habit,' 'habituation,' 'perseveration,' and 'conditioning.'

In daily life also we are constantly taking objects or situations in various ways. Certainly the engineer's view of a landscape is different from that of the artist, and the difference lies precisely in the way-in-which the landscape is 'taken.' The common expression 'I *take* it that you mean so and so' points to the same fact. The organism is continually being instructed and charged toward execution. Examples are readily found in the laboratory. Observers are directed by instruction to do certain particular things, to survey now one aspect of experience, now another. Stimulus objects may, *e.g.* be taken as material things, as objects of desire, as pleasant or unpleasant, and so on.

The influence of instruction and attitude upon report has been noted by investigators of sensory discrimination, judgment, perception, association, feeling, thought and action, and of the administration of mental tests. Sometimes the attitudes have been taken just as they spontaneously appear and sometimes they have been experimentally induced.[3] We began our research by securing detailed reports without specific instruction regarding them. Thus we proceeded to clear the way for a more exact and exhaustive description and classification (Part I). Then we sought to ascertain the effect upon functional performance of each kind of attitude taken separately (Part II).

The Experiments

In Part I our *Os* were presented with colors and tones and were orally directed to note and to characterize both the initial way-of-taking experience and also any change in this mode which might occur during the period of perception.[4] The meaning which we attached to 'way-of-taking' was made plain to each *O* in informal discussion. The stimulus-objects consisted of single

[3]For literature and discussion see M. Bentley, *The Field of Psychology*, 1925, 384 ff., 452, 505, 533 ff.; E. B. Titchener, *Experimental Psychology of the Thought Processes*, 1909, 80 ff., 85 ff., 120 ff., 127, 158, 161 ff.; O. F. Weber and M. Bentley, The relation of instruction to the psychosomatic functions, *Psychol. Monog.*, 35, 1926, (no. 163), 1 ff.; M. A. May, The mechanism of controlled association, *Arch. of Psychol.*, 1917, (no. 39), 4 ff.

[4]The *Os* were Mr. D. T. Griffin (*G*), assistant, and Miss E. R. Moul (*M*) and Mr. G. L. Freeman (*F*), graduate students in the Department of Psychology. All were experienced in observational technique.

colors, colored pairs, distorted color-forms, single tones, bi-tonal complexes, and rhythmical tonal sequences. The attitudes reported readily fell into distinct types. A list of these types, with a characterization of each, was later placed in the hands of each *O*, in order that he might verify it in subsequent experiences.

In Part II the *O*s were directed before every series to adopt some particular attitude that had appeared in their reports of Part I. In order to determine the effect on performance of the adoption of a particular attitude, the *O*s were further directed carefully to note their experiences during the presentation of the object, a colored geometrical form pasted on colored backgrounds or a rhythmical succession of tones which was suggestive of a musical theme.

Apparatus and setting. The general conditions were the same in Parts I and II. All colors were presented under film-color conditions in a dark room. The apparatus was adapted from that described by Martin.[5] It consisted of a double screen of scotch gray cardboard, 36 x 56 cm. The cardboards were set parallel at one end of a table and 18 cm. apart. In the center of each was a circular opening 6.5 cm. in diameter through which cards of color, placed in a holder 1 m. beyond the more distant screen, could be viewed. *O* sat at a distance of 2.5 m. from the colors which he observed. The circular visual field, which was exposed by means of a cardboard shutter placed between the openings of the double screen, was 19 cm. in diameter and was directly illuminated by two 100-w. daylight lamps, one on either side of the field and 25 cm. from it. Constant illumination was assured by a variable rheostat and voltmeter in the lamp circuit. The voltage was maintained at 87 volts. The circular field of color appeared in a homogeneous lightless setting. A chin-rest, with black hood attached, kept the position of the head constant with respect to the field.

The tones were produced by two audio-oscillators. One was of the multiple-pitch type, with a range of 450–4600 cycles a sec.[6] The instrument was calibrated for frequency by equation with standard tuning forks. An added stage of audio-frequency amplification, consisting of a Silver Marshall No. 220 audio-transformer and a CX-301-A vacuum tube with 90 volts on the plate, was used to supply a Farrand oval-cone loud-speaker. Adequate control of the intensity of the tones was obtained by varying the bias upon the grid of the amplifier tube with the aid of a 3000-ohm variable rheostat inserted in the C-battery circuit allowing a maximal bias of 22.5 volts. A voltmeter in this circuit permitted accurate reading of the negative grid bias. It is known, of course, that the degree of loudness of a particular tone with this arrangement will increase regularly as the grid bias approaches normal for the plate-voltage employed (in the neighborhood of 3.5 volts for the plate voltage, in this case).

[5] M. F. Martin, Film, surface, and bulky colors and their intermediates, this JOURNAL, 33, 1922, 454.

[6] For a diagram of the circuit see F. A. Pattie, this JOURNAL, 38, 1927, 44.

This type of oscillator does not produce an entirely pure tone. For this reason, the wave form of each frequency was observed by means of an oscilloscope to obtain those frequencies which approximated purity. The frequencies were 640, 768, 896, 1024, 1280, 1536, 1792, 2048, 2300 and 4096 d.v.

The loud speaker was screened from *O* by a black cloth curtain and cardboard sheets were tacked to the edges of the table to prevent visual distractions. *O* sat at one end of the table facing the loud speaker at a distance of 50 cm.

The other oscillator was of the tuning-fork type (No. 213, General Radio Company) and of a frequency of 1000 d.v.[7] The instrument was used throughout the experiments at medium intensity. The oscillator was enclosed in a sound-proof container in an adjoining room, the tone being transmitted by means of an ear-phone suspended directly before the loud speaker.

Every frequency chosen from the range of the multiple-pitch oscillator was equated as to intensity with the tuning-fork oscillator. This was kept constant for the tones of the former instrument by proper adjustment of the grid bias of the vacuum tube-oscillator before sounding the tone.

The control-switches of both instruments were on a single switchboard in the experimental room. Two keys of a Zimmermann multiple-reaction switch, one in the loud-speaker circuit and the other in the ear-phone circuit, provided a convenient keyboard which enabled *E* to obtain a single tone from either oscillator, or else tones from both were sounded simultaneously or in rapid alternation.

Part I

Series i and ii. The design of the experiments of Part I, it will be remembered, was to set up such experimental conditions as would permit the study of a wide range of attitudes in the common observational setting of the laboratory. Accordingly, we chose to make our conditions as simple as possible from the point of view of the observer, a plan followed in all our series.

Stimulus objects. The objects chosen for the initial series consisted of single film colors and single tones produced by the vacuum-tube oscillator. For Series i; reddish yellow, yellowish green, yellowish red, yellow, purple, green, blue, red, violet, orange, greenish yellow, and greenish blue presented in order.[8] For Series ii; the order of tonal frequencies was 2048, 896, 1536, 768, 1024, 1280, 2300, 1792, 4096, and 640 d.v. The stimulation-period was 10 sec. for the members of both series.

Instructions. The formal instruction read at the beginning of every observational hour was as follows for Series i.

"At the signal 'now' you will be presented with a visual stimulus object. Observe carefully your initial way-of-taking the visual object. Watch care-

[7]For description see H. M. Halverson, this Journal, 38, 1927, 295.

[8]Hering colored papers were used throughout the experiment. The Stoelting numbers for the series, in the order in which they appear above, are 4, 7, 2, 5, 15, 9, 13, 1, 14, 3, 6, 11.

fully for any change or shift in the nature of this attitude during the course of the experience. Characterize the initial attitude and any shift occurring in it in as great detail as possible.'

The formal instruction was the same for Series ii, except that the word "visual" was changed to "auditory."

It was made clear to the *Os* at the outset that they were not to attempt to report vagaries or hypotheses, that they were to give a clear and full account of precisely what happened prior to and during the period of stimulation, and to include in the report any anticipative hints and any form of reinstruction that might occur. The *Os* worked without knowledge of the experiment other than the information contained in the formal instruction and implicit in the experimenter's informal elaboration of the conditions.

Results. A total of 263 reports was obtained from the three *Os* in Series i and ii. Careful analysis of these reports reveals the fact that the *Os* do not maintain a constant attitude prior to and during the period of stimulation. Various shifts and modifications of the initial mode occur during the 10-sec. It was found that the various attitudes could be naturally arranged under the following six general types:

Attitude A. Casual survey of objects;
Attitude B. Inquiring survey of objects;
Attitude C. Critical survey of observer as observer;
Attitude D. Critical, particularizing survey of objects;
Attitude E. Personal valuation of objects as pleasant or unpleasant, agreeable or disagreeable, and the like;
Attitude F. Impersonal valuation of objects as conforming or failing to conform to some conventional standard.

(*1*) *Characterization of Attitude A: Casual survey of objects.* Under this attitude the *O* simply apprehends the object. It is a passive, casual or indifferent looking-at or listening-to something as a visual or an auditory object. Color is taken as "simply something out there to be looked at," "simply as a disk of color out there," as "a ball of fire," "a setting sun," "the washed-out yellow one," and the like. Tone is taken as "that tonal object out there in front of me," as "a telephone wire sound," "the whistle of a radiator," "the sound of a stringed instrument," "the sound of a saw mill," and the like.

The objects fall into two fairly distinct classes, generic and specific. Color or tone taken as "simply something out there" is an object of a different order from a color or tone regarded as "a ball of fire" or as "the whistle of a radiator." In the former case the object is of a general kind or pertains to things of the

same group or class; *e.g.* a visual or auditory 'something' indicates merely that the experience lies within the sphere of visual or auditory objects. In the latter case the object is specific; *i.e.* it is not simply 'something' visual or auditory, it is rather a particular object. Characteristic reports serve to illustrate both the general characteristics of Attitude A and the distinction between generic and specific objects.

M: Generic object. [Series i, 4[9]] The visual experience first came as a round something out before me. [Series i, 2] The first attitude upon the visual presentation is very general. I took the thing as something out in front of me, not specific as to color or to object. [Series ii, 3] Then I heard the sound. It was neither tone nor noise. It was a drab, dull sort of experience of hearing a sound out before me. [Series ii, 6] Neither agreeable nor disagreeable. One takes it for granted and doesn't care. After a term of casually noting this sound, ideas came in.

M: Specific objects. [Series i, 1] This didn't last more than an instant when I shifted very quickly to the object attitude. It is that orange color that does it. It becomes a setting sun or a ball of fire. [Series i, 7] The thing was simply a pale ball out in front of me. [Series ii, 2] It meant the familiar sound of the tuning up of a stringed instrument. [Series ii, 10] How like the whistle of a stove or a radiator!

F: Generic object. [Series i, 1] The first thing that seems to happen when that color is presented is a very clear knowledge of it as color. [Series i, 7] The initial attitude was fairly neutral, non-specific, a looking for something to come out in front of me. Some sort of mental jotting to take it passively. The color was recognized as yellow. [Series ii, 5] The initial set was perceptive. It was projected out there. No personal reference except 'I perceiving something out before me.' [Series ii, 7] Indifferent and neutral toward the sound throughout. It kept at some distance from me. I could point to the meaning out there beyond the screen: 'a sound out there.'

F: Specific object. [Series i, 5] Recognition plays a great part in the initial way-of-taking the color. 'Yes; this is the washed-out yellow one' came at once. [Series i, 9] Soon I did seem to get my bearings and the first and definite meaning that came referred to the object in and for itself. It was 'That is marine blue.' [Series ii, 1] A good deal of verbal comment in this experience, such as 'It sounds like a saw mill.' [Series ii, 3] The attitude remained perceptive throughout. Almost immediately it was the sound of an electric motor.

G: Generic object. [Series i, 4] At the beginning I took the object in quite a passive attitude, simply as something out there to be looked at. [Series i, 11] The experience out there was simply a bit of visual stuff to be looked at. [Series ii, 4] There was, I should say, a passive attitude, more passive and less intent than the phenomenological attitude. There wasn't the sort of familiarity with this tone as in the last period, nor was there the attempt to recognize it specifically. It was more 'That tonal object down there in front of me.' [Series ii, 8] After that I believe my attitude was one of watching and observing the sound as it went on.

G: Specific object. [Series i, 3] At first a disk of this particular sort of orange. [Series i, 10] I first took the color as representing the blue of a bright night sky. [Series ii, 1] After the beginning when it was reed-like, it was much more like a telephone wire than any I've heard. [Series ii, 10] There was some idea of an instrument operating there. The tone started and went on like a drill, which comes down with a ping-and-go, on turning very rapidly.

[9] The number following the series designates the stimulus object.

Anticipations or *expectations* of the coming of the visual or auditory objects are frequently found in the reports; chiefly anticipations of *generic* objects.

Generic objects:

M: [Series i, 4] The initial attitude (before visual object was presented) was highly non-specific. I wasn't set to take it any particular way. [Series ii, 7] The initial attitude was very unspecific. It meant 'waiting for something.' Even waiting-for-something was a clouded sort of thing.

F: [Series i, 5] I expected to see a color of some kind. [Series ii, 10] The initial set was perceptive. A definite reference out in front of me. A definite set to hear something out there

G: [Series i, 11] I was of course in the position required, gazing directly in front of me. [Series ii, 11] There was before the sound started what one might call an attitudinal anticipation of it.

Specific objects:

M: [Series ii, 9] The set to observe was rather highly determined, extending to the kind of sound I'm going to hear.

F: [Series i, 5] For some unaccountable reason I rather expected a green color to be shown. [Series ii, 5] There was a sort of meaning that 'Oh yes, it will be another like the preceding.'

G: [Series i, 6] I sat here with my chin in the chin-rest and was looking for something nice and rich. [Series ii, 8] The way it came at first surprised me. If I was predetermined in any way I was disposed to consider it as a musical or potentially musical sound.

(2) *Characterization of Attitude B: Inquiring survey of objects.* As in Attitude A, *O* concerns himself in Attitude B with the object-character of experience. But under this attitude he is a more active, participating observer. He is on the alert to determine what object the color or tone is, what object it is like or what it means. The attitude is primarily one of searching or looking about, *e.g.* for the name of the color or the kind of sound. *O* is above all asking such questions as "what are you or what are you like?" This type is principally distinguished from the preceding through the fact that in Attitude A there is no overt searching or inquiring; the object arises spontaneously and is casually noted. The search explicit in the present type is in many cases fleeting and transitory in nature, and is rewarded almost immediately by the appearance of an object, in which case the attitude changes to Type A.

M: [Series i, 7] I looked to see the kind of object, the color, etc. [Series i, 10] After this attitude had spent itself there was a quick shift that I could observe as a shift but could not have characterized at the moment. I couldn't get hold of it more than to say that it was different. It was a kind of taking-the-experience-as-object. It was more like an attitude of trying to see what the thing was. [Series ii, 3] Something new entered with this shift, a jerking up of myself to look at the experience and to see what it was like and what it was. [Series ii, 9] I looked again at the tone, not exactly with the idea to analyze but rather to see what it was like as a whole.

F: [Series i, 2] Curious! There is a certain strangeness to the pattern as it arose out there. Simply means that that is an unfamiliar color and I ask 'is it red or is it purple?' [Series i, 11] The initial way-of-taking experience was perceptive in character. I simply wanted to see what it was like. It was an inquiring sort of preparation. [Series ii, 2] The initial set was perceptive just to see what the thing was and no determination to value it. Almost immediately there was the meaning 'That is a very funny sound.' [Series ii, 3] The perceptive set is simply a realisation that I'm to see what it is.

G: [Series i, 6] After that there was a change that could be characterized as a set. It might be called a hunting attitude. I had the feeling that the color meant something, belonged to something. There was a questioning, a what-is-it sort of feeling. I asked in subvocal speech 'Is it apple green?' and the answer came 'no.' [Series i, 8] And there was too a sort of searching but not an overt searching, not a searching accompanied by obvious kinaesthetic change but one for a more precise notion of the nature of the exposed phenomenon. [Series ii, 3] There was something indicating that I knew that there was some sound, some object, meaningful sound, like this. But I couldn't quite get what it was. [Series ii, 7] This latter situation seemed to be a looking for some way to take the sound as object. The only thing that came was the word 'wire' in subvocal speech.

(3) *Characterization of Attitude C: Critical survey of observer as observer.*

The observer here is on the alert in surveying or critically scrutinizing himself as an observer in the experiment. He wants assurance that he is "properly set" or is "ready to observe" or that he is following out the command contained in the formal instruction. He feels his responsibility as an observer to carry out to the best of his ability the task which has been set him. This attitude breaks in upon the course of the experience which is bound up with the experimental stimulation, and it temporarily blocks that course. What the observer does, then, is to hark back to the requirements of the formal instruction and reformulate what he is to do in carrying out the original task.

M: [Series i, 1] It (a neutral, lethargic attitude) lasts only an instant. Somehow the general set for observation crowds it out. It is as if I feel that I ought to be doing something with the visual experience and this neutral attitude is an admission that I'm not. There is a sort of re-organization of the general observational set that is similar to what one has had just before, or at the moment that the visual experience begins. It is this re-organization that is the matrix out of which the object-attitude that follows develops. [Series i, 9] During the time I was not observing at all. Then gradually, how or from where I don't know, my old observational attitude returned. I realised what I was doing and what I ought to be doing. [Series ii, 2] The initial attitude was very undetermined. I wasn't even set for auditory experience. I knew that I should have been. At the time, if questioned, I could have told what my set ought to be. I couldn't seem to get worked up to the level of what the instruction required. [Series ii, 6] Then came a jerking up of experience. I realized that I was supposed to be observing this. I had the disagreeable feeling that I wasn't attending to the matter in hand. Then I became more critical of my own way-of-taking these things.

F: [Series i, 2] There was an additional self-instruction that 'You are to do the right thing this time.' [Series i, 7] I wasn't as highly directed this time in the initial moment. Perhaps I didn't follow the instruction. . . . Somehow it immediately seemed that I should have been set for the object. The fact that I had allowed myself to relax into a more passive set disturbed me for a moment. [Series ii, 2] There was a sort of additional self-instruction to listen attentively and possibly also a self-instruction not to let the mood color the experience. [Series ii, 5] My initial set wasn't of the best that time. In common-sense speech I mean that I had not as yet come to center my attention on the sound. During the course of the experience I did sort of pull myself together. With this the sound became highly focal.

G: [Series i, 7] At the same time it occurred to me that it was about time I took this really to mean something. [Series i, 8] In a way it was a stage of effort, an abortive state, too, knowing that something is to be done and still it isn't. [Series ii, 4] There was a sort of slipping or half-way slipping into my laboratory attitude, but I managed to save myself. [Series ii, 5] Just after the signal 'now' I made a slight movement of one of my arms and just then the tone came and, as it were, caught me in the act.

(*4*) *Characterization of Attitude D: Critical, particularizing survey of objects.* This attitude is one under which the observer is on the alert carefully and critically to scrutinize the colors or tones presented. It is the attitude *par excellence* of the descriptive psychologist, being an attitude to observe the character of a given experiential phenomenon for the purpose of giving as careful and as accurate a descriptive account of it as possible. *O* characterizes or notes particulars, and, in some cases, attempts an analysis of the object. With respect to colors, the attitude involves a watchfulness for changes in brightness, chroma, texture, penetrability, shape, richness, glow, and the like. As for sounds, it involves a noting of such characteristics as pitch, denseness, brightness, size, shrillness, intensity, softness, thinness, insistence, and the like.

M: [Series i, 6] But my attitude had become more critical than the one I had termed common sense. . . The individual features of the pattern stood out, especially the color, dimness and size. [Series i, 7] Under this analytic attitude, I noted the quality, *i.e* the tint and the texture of the color and shadings within it. [Series ii, 4] I analyzed the sound in terms of its pitch character, its localization, its size (by which I mean how concentrated it was), etc. [Series ii, 5] Compared it with the preceding in pitch, and intensity and sweetness and tone color.

F: [Series i, 2] At first a change of experience was referred to the stuff out there, to the stimulus. To speak of it in meaningful terms, the color looked richer, more glowy and lively, a sort of scintillating experience. [Series i, 4] The whole pattern of experience was projected out there in front of me. It is true that at times there was a slight darkening around the edge of the color quality, becoming the limits of it, tending to become brighter and then to recede, then all becoming the same brightness. [Series ii, 5] There seems to be some characteristic of the sound itself which is a condition for a shift in attitude. I have characterized it in terms of insistence, brightness, softness, loudness, smoothness, harshness, etc. [Series ii, 9] The attitude shifted almost immediately to participation. I became quite interested in the fine, thin, soft note.

G: [Series i, 8] Although the shape of it didn't seem a positive matter until the idea of stating it came, as the exposure continued parts of the field varied and the nature of the blueness became less luminous chiefly around the outside. [Series i, 10] There was during most of the exposure a fluctuation in dullness near the center of the disk and a fluctuation in size of the somewhat poor blue spot out there. A little after this had begun there were also changes around the inner edge of the disk—changes in chroma, I believe. [Series ii, 5] Still a bit later I began to take the sound as having more than one pitch reference. The tone now seemed to have, as well as its originally-noted pitch, another considerably higher, smaller, and I think relatively more dense. [Series ii, 8] I don't believe a thing came into that except the sound itself, its character and its variation—a high bright, almost shrill, small, pretty, clear-cut tone. Although I first took it as a change in pitch, on second view it seemed that it had simply withdrawn in itself, getting less volumic.

(5) *Characterization of Attitude E: Personal valuation of objects as pleasant or unpleasant, as agreeable or disagreeable, and the like.* Under this attitude the survey of objects is valuative. The colors or sounds, and, in some instances, characteristics of them (see Attitude D), are pleasant or unpleasant, agreeable or disagreeable, pleasing or displeasing or they are liked or disliked. They are found to be interesting or uninteresting, and, not infrequently, annoying or disturbing. Adjectives such as enjoyable, pretty, nice, sweet, rich, vibrant, corking, horrid, terrible, draggy, and dull, are applied to the objects to indicate experiences of an affective kind.

The commentaries consistently point to the fact that the valuation as pleasant or unpleasant, etc., is essentially based upon experiences of an affective nature which occur to a particular individual at a particular 'now,' and is not a valuation made with the aid of standards or criteria obtained from past experience and bearing a conventional or social significance. Pleasantness or unpleasantness is *my* pleasantness or unpleasantness. It is the *way-in-which I* feel. It is an occurrence within experience which indicates the trend or direction or, so to say, the 'whitherness' of my own psychophysical processes at a particular moment.

M: [Series i, 3] It was a weak kind of affection, not much more than the meaning of 'its a rather soft, pretty sort of color.' [Series i, 4] The very weakness and dullness of it became unpleasant. [Series i, 10] Under the affective attitude it comes as something pleasant, *i.e.* more simple. It takes me all at one time. [Series ii, 1] I found myself enjoying it and at the same time I had a feeling of surprise that it was so vibrant. [Series ii, 2] I said to myself 'Why! I like that.' It was bright and vibrant and had a good deal of color and richness to it. When I give these adjectives I'm not analyzing. These words all refer to the pleasantness of tone.

F: [Series i, 1] Immediately the shutter went up I found that I was perceiving with a bias as I said 'I don't like that' before the redness impressed me. And this is one of those experiences you don't question. You simply don't

like it. [Series i, 2] This background somehow tones the color-experience. It is found in this case to be a pleasant color-experience. My general way-of-taking the color seems to be open and receptive. [Series ii, 2] It struck me as being a bit pleasing to the ear at first. [Series ii, 4] It was no longer a sound experience but an unpleasant sound experience. [Series ii, 8] The experience was more than a loud sound. It was first of all an unpleasant sound.

G: [Series i, 3] In taking the disk as 'that sort of color,' there was an implication of approval that this particular *thatness* was pleasing. I can say now that in the pleasantness were involved the penetrability, the glow-likeness and the luminous character of the color. [Series i, 8] Its mellowness and penetrability are pleasant. [Series i 12] My first reaction was 'isn't that perfectly corking'—*i.e.* as to color and texture it was highly pleasing. [Series ii, 1] After the first and reedy part of the sound, the question of affective value occurred to me and it seemed that the reedy part should have been pleasanter than the sound at the moment. [Series ii, 2] It was a relaxed and thus pleasantly-toned experience. [Series ii, 6] I should say that my attitude throughout was a rather pleased attitude.

Anticipations of experiences grouped under this type are reported very rarely, never by *G*.

M: [Series ii, 1] My initial attitude . . . was a rather specific determination for a particular kind of sound. This particular kind of sound that I was waiting for was that squeaky, rather colorless, radio-quality of sound.

F: [Series i, 4] I think I perceived with a bias this time. I'm somehow not as neutrally disposed to the object as before. Immediately the thing was presented there was the meaning 'I don't like that as well as.'

(6) *Characterization of Attitude F: Impersonal valuation of objects as conforming, or as failing to conform, to some conventional standard.* The reports obtained from Series i and ii indicate that the colors and tones were valued not only as pleasant or unpleasant, but also as beautiful or ugly, and the like. The *O*s throughout the observations were constantly attempting to characterize the attitude under which the objects were taken as beautiful or ugly. They referred to this attitude as aesthetic. They 'felt' that there exists a difference of some kind between this way-of-taking experience and that of the affective type, but they found themselves unable adequately to characterize that difference. Perusal of the reports reveals the fact that this characterization was extremely difficult.

Reports indicating valuation of an aesthetic type give at the same time instances of the difficulty referred to above.

M: [Series i, 1] I don't know what actually happened when I looked at the shape but I shifted into an affective attitude. I came very near to the aesthetic while looking at the perfect roundness of the color. [Series i, 2] Sitting here I was lost in simply looking at the beautiful thing out in front of me. [Series i, 4] This aesthetic feeling is weak; yet it was definitely not neutral but was on the side of ugliness rather than beauty. [Series i, 10] I would call this attitude aesthetic rather than affective. It is hard to get at the difference that I feel exists between the two. Many ideas of all kinds enter into the aesthetic. Ideas pertaining mostly to the color. To be specific, I take this blue as the commonly-used color for paintings of the sky and of madonnas. The color somehow

places the object in the realm of the artistic. [Series ii, 5] It became pleasant and it suggested ideas of smoothness and evenness and sweetness. But I don't know whether to call it affective or aesthetic. I'm not sure of the difference between these two. I think if forced to one expression I would call it aesthetic. And this is my reason. I think of affective experience as something very intimately bound up with myself while aesthetic is something that I contemplate from a distance and the pleasantness is in myself as I contemplate the experience.

F: [Series i, 10] All that I was aware of was that quite suddenly the experience did have a definite meaning. The term aesthetic or affective would apply. The meaning still referred out beyond me to the color, the meaning of 'That's a lovely thing.' [Series i, 11] There was hardly a second after I saw the color before a very marked shift occurred in this attitude. In fact I was hardly aware of the purple meaning before I had the other and more pronounced meaning 'That is perhaps the most lovely thing I have seen yet!' There was a lot of imagery coming in after that, recalling a purple dome and imagery of a somewhat similar colored experience of the past. [Series ii, 6] But then I became interested and valued the sound. I simply open the door, as it were, to a great number of aesthetic interpretations which spread over and color the sound-experience. [Series ii, 8] This was hardly the meaning of 'pleasant sound!' I hardly like to put the label of 'pleasantness' on this meaning. It's not enough that way to be called pleasant, *i.e.* as we usually use the term. Its more of an aesthetic, finely-colored, perceptive meaning. The pleasantness doesn't dominate or overrun the perceptive character of the sound. It simply touches it and gives the meaning of 'loveliness.'

G: [Series i, 2] I never took it as the wrong sort of color. After this happened, first I watched the shadings of the disk and then took it as clouds or reddish smoke. You do have a sort of 'entelechian' feel for a thing like this. That is, when I was taking it as a rather poor cloud or smoke I knew it was coming out right in the end. At least so it seems now. And indeed I was able to take it, and it was not particularly a *tour de force*, seeing rather gorgeous lighting effects above and below. I don't know that I can describe it; but I could indicate it by pointing to something similar to it sometimes, or by saying that my feeling for it was such that I would call it rather gorgeous. This last attitude was a sort of aesthetic attitude. [Series i, 1] What I saw was a beautiful, rich, fiery-red disk. [Series ii, 9] It was either simultaneously with its ringing off or just after the whole thing ended that the idea of a musical figure came to represent it. This musical figure was rather general and not specific and might apply to such tonal relations as mi, do, sol, mi, and so on. The tone might be suitable for any theme whose form was of that general sort.

Series iii, iv, v, and vi. In attempting to account for the fact that the *O*s were unable to differentiate clearly between Attitudes E and F, it was surmised that the difficulty might lie in the simplicity of the objects presented. It is fairly apparent from the reports that the attitude which the *O*s repeatedly refer to 'aesthetic' is of a more complex sort than that of Attitude E, the affective. The latter appears to involve a relatively simple type of experience in which objects are declared to be simply pleasant or unpleasant, agreeable or disagreeable, and the like. The *O*s do not characterize it further than this. But in Attitude F it is apparent that the *O*s refer to a type of experience which involves a

pronouncement of a different sort, a pronouncement which seems to be based primarily upon standards or criteria which bear the stamp of social sanction or custom. The distinction which we have drawn here, it must be emphasized, was, however, only implicit in the reports. With the purpose of making more explicit this hint of a difference we designed experiments in which objects of a slightly more complex character than those of Series i and ii were presented. In addition to this, it was thought best to direct the *O*s to note more specifically experiences of a valuational sort to which direct reference had been made in the reports.

Stimulus objects. Two groups of two series each were planned and executed with the above design in view. In the first group (Series iii, visual, and iv, auditory), the stimulus objects were more complex than those in Series i and ii, while those of the second group (Series v, visual, and vi, auditory) were of a still more complex nature. The two series within each group were so devised that the visual and auditory objects were of a like degree of complexity. These objects were presented under the same general experimental conditions as before. A description of each series of objects, with the length of the period of stimulation in each case, is given below.

Series iii. Combinations of two colors were exposed for 10 sec. The colors were pasted on cardboard so that the field was divided into equal halves. Each card of color was presented in such a way that one color lay above the other. The color combinations were (in the presentational order) reddish-yellow-and-violet, yellowish-green-and-purple, red-and-greenish-yellow, orange-and-bluish-green, red-and-orange, blue-and-greenish-yellow, purple-and-yellowish-red, yellowish-red-and-cerulean-blue, green-and-yellow, green-and-blue, greenish-blue-and-yellow, and violet-and-greenish-blue.[10]

Series iv. Bi-tonal combinations, produced by the two oscillators, were presented for 10 sec. The frequencies of the tones combined were (in the presentational order) 1000–640, 1000–896, 1000–1024, 1000–1536 and 1000–2048.

Series v. Combinations of two colors (selected from Series iii) were exposed for five sec.[11] A distortion of the spatial configuration of the circular field was produced by attaching a small irregularly-cut piece of cardboard to the edge of the circular opening in the rear screen. The result was a circular field with an irregular bulge. The position of this distortion was altered for each stimulus-object by attaching the piece of cardboard above, below, to the left, or to the right of the opening. The color combinations, with position of the distortion indicated, were (in order of presentation) as follows; reddish-yellow-and-violet (top distortion), yellowish-green-and-purple (left distortion), orange-and-bluish-green (bottom distortion), and yellowish-red-and-cerulean-blue (right distortion).

[10]The Stoelting numbers for this series are 4-14, 7-15, 1-6, 3-10, 1-3, 13-6, 15-2, 2-12, 9-5, 9-13, 11-5, and 14-11.

[11]The objects were presented for a shorter period of time in Series v and vi in order that the *O*'s general observational attitude might remain constant.

Series vi. Two single tones from the oscillators were sounded in rapid succession during five sec. A simple rhythm was introduced into the discontinuous succession by alternately prolonging one of the two tones. Four pairs were given in the 5-sec. period. The frequencies of the pairs of tones[12] alternately given were (in the order of presentation) 1000–640, 640–1000, 1000–896, 896–1000, 1000–1024, 1024–1000, 1000–1536, 1536–1000, 1000–2048 and 2048–1000.

Instructions. For the series in which visual objects were presented (Series iii and v) the formal instruction read at the beginning of each hour of observation was as follows.

> At the signal 'now' you will be presented with a visual stimulus object. Watch carefully such experiences as your enjoyment of it, your appreciation of it, your approval or disapproval of it, your finding it agreeable or disagreeable, or perhaps indifferent, and the like. You are not to consider that you are limited to the particular experiences named here. These are intended merely as examples of that general side of your experience which you are to watch. At the end of the exposure give a careful descriptive account of this aspect of your experience.

The instruction for Series iv and vi was the same except that the word "visual" was changed to "auditory."

Results. The distinction between Attitudes E and F which was implicit in the reports of Series i and ii became explicit in the reports of Series iii and iv. As the reports of Series v and vi do not indicate any new distinction but rather make clear the distinction drawn in iii and iv, we shall treat the results of the four series together.

On the basis of the reports obtained in these series Attitude F may be characterized in the following way.

We have pointed out before that under this attitude, as also in Attitude E, *O* places a value upon the colors or sounds presented. Under Attitude E the value is personal or individual, while under Attitude F it is fairly impersonal; and, in the latter case, it is based primarily upon conventional criteria or standards. The colors are taken to be suitable or unsuitable for paintings, for clothing, and the like. They are or are not properly balanced, they do not go well together, they are symmetrical or unsymmetrical as to form or shape. The tones, judged in terms of musical standards, are regarded as harmonic or inharmonic, acceptable or non-accceptable (labored, monotonous or non-fitting), and the like. They may form melodies which are complete or incomplete; they may be rather well played or poorly accented. In this category, as will be apparent from typical reports, we have principally

[12]The first tone of a pair was always of shorter duration than the second.

to do with matters of good and bad taste, matters of correct and incorrect technique, etc., which are at bottom of a conventional sort.

The reports from Series iii, iv, v, and vi adequately point to the fact that, at least in the laboratory, reports of this kind are not ordinarily passed upon objects which are below a certain level of complexity. They are made upon objects which serve to place the individual in definite situations. The objects are then, in brief, objects of a situational sort. The very complexity of the experiences which fall within this category clearly indicates this fact. The point will be made more explicit by a perusal of typical reports which serve to illustrate the general characterization of Attitude F.

M: [Series iii, 4] The experience I had was unusual for a laboratory setting. From the first it meant a painting of a sunset over a blue sea. I valued it in terms of a painting, and throughout in terms of its meaning, in terms of the ideas it suggested. I critically examined the sky first. This is different from examining the color. This valuation is in regard to how well the object coincides with all my previous notions, with some standard I had got somewhere, of how a sunset should be painted. [Series iv, 5] Then two little tinkling sounds came in. Clear meaning of tiny bells. No other characteristic fits so well this time as that I approved of the way those little bells were sounding. It somehow involved a setting up of this experience against the standard I have of how bells ought to sound in order to be judged as beautiful. [Series v, 2] I said 'it's a beautiful thing that I enjoy looking at but it's spoiled by that black hump.' [Series vi, 4] This was first taken as a choppy melody. It belonged to simple, non-serious things. Later I felt the melody was completed quite adequately. I had shifted to a different reference-frame or setting, to a more sophisticated, musical one. That ending had made the whole thing into a musical phrase, a unity, a completed melody. I was quite moved by the beauty of it, the finish of it.

F: [Series iii, 4] Aesthetic valuation in terms of some kind of standard. The standard need not be present in experience. But there is apparently a certain feel for the rightness or the wrongness which attaches itself to the perceptual object when seen in terms of this attitude. The first meaning referred to the combination of colors simply as a disagreeable combination. This seemed to usher in other contexts which served to elaborate the perceptual-affective meaning of 'disagreeable combination.' Such ideational supplementation is exemplified by the following meanings which occurred; 'Reminds me of cheap hats displayed in the windows of the twenty-five cent store, or reminds me of loud-colored scarfs or neckties.' A feeling that I would not want to wear such colors or be seen with any one wearing them. [Series iv, 3] The sound was referred to as coming from an organ out in the hall. I thought of the reeds of the organ as being old and the sound thin. It struck me as not being full and robust. It was my critical ear which seemed to be listening in this experience. I mean that the auditory object was judged or valued in terms of preconceived notions of what constitutes a good organ-tone. Just how I acquire these feels I don't know. I suppose one hears music and is told that that's good and that's bad, which goes to make up a rule or measuring stick by which one judges the perceptions of the present. While these feels for rightness or wrongness are very vague and not explicitly formulated, they exist as substantial criteria of appreciation. [Series v, 2] Again I found it spoiled the symmetry

of the object and so colored the meaning and the value I put upon the object as a total. It wasn't the colors that I considered so much as the form, the spatial character of the figure. It was compared with a standard for circularity and found wanting. Of course the standard isn't in experience. It isn't anything like that. Still there's the feeling of not being right. [Series vi, 2] There was a lack of harmony and smoothness in the melodic sequence. The interval between the sounds was not harmonic and there was some hesitancy in the performance. I should be inclined to say that in this judgment past experience played a great part. This sort of interval between notes is not common, not one that I'm used to. It was a rather queer combination. Such meanings as 'queer, peculiar, inharmonic' are values in a sense placed upon experience. They seem to utilize old notions. By 'notions' I mean any relationships or conventional rules which are the tools with which I build up judgments of this sort.

G: [Series iii, 7] Well; at the beginning it seemed that they didn't go together, those two colors. That is, as colors they didn't belong together. There was something in the initial way of taking it that implied that such a verdict was conventional, that they weren't *supposed* to go together. This didn't contradict the opinion that they didn't go well together. It meant rather that the judgment failed to be independent. [Series iv, 4] The two sounds were constant for a very long period and I couldn't make a musical score out of them. It was a failure of acceptance. My attitude here is akin to what it would be if I were trying over a contrapuntal exercise and saying 'yes, this will pass and that won't do.' [Series v, 2] I found that the bounding line at the left added a graceful shape to the whole. [Series vi, 1] There; those were better so far as tone-quality in the musical sense goes. So far as their connectedness is concerned, compared with the previous sets of sounds, they were quite *legato*. The difficulty of accepting the sounds as a musical figure lay in their monotony, in their lack of variety. One way of putting it is that they didn't 'get anywhere.' I thought either a change in rhythm or a change in pitches would make them satisfactory in this respect.

The results of Series i to vi which have been presented above show qualitatively, in addition to the characterizations of the attitudes themselves, that the attitudes adopted do not depend upon the mode of stimulation, and that there is little variation among the *O*s when differences in the terminology employed in making the characterizations are removed. It is clear that every attitude is adopted by every *O* whether the stimulation is visual or auditory. We have pointed out that in the case of Attitude F adoption is to a certain extent dependent upon the complexity of the stimulation; but in this case it is equally independent of the sense-department involved. There occurred differences in terminology in the two modalities; but these do not indicate fundamental differences in the attitudes adopted in the two cases, as the reports show.

The lack of dependence of the results of these series upon the sense-modality stimulated and the agreement among the *O*s can be shown by a statistical treatment of the results. It was found that in a single observation an *O* may successively adopt from

one to four attitudes. If we compare the average number adopted by each *O* in each series (See Table I), we note first that there is

TABLE I

THE AVERAGE NUMBER OF ATTITUDES IN A SINGLE OBSERVATION IN SERIES I AND II

	Series i	Series ii	Ave.
M	2.1	2.5	2.3
F	1.9	1.3	1.6
G	1.7	1.6	1.6
Ave.	1.9	1.8	1.8

no appreciable difference in this respect between Series i and ii; and, secondly, that there is only a slight difference among the *O*s. In this latter case the average of *M* is slightly higher than that of the other *O*s. The average frequency of occurrence of every attitude in every series for all *O*s was obtained from the number of times A appeared in all reports, then B, then C, and so on for all the attitudes. This average frequency is shown in Table II. Attitude A is found to occur with greater frequency in both series. In Series i the rank order of frequency, proceeding from next highest to the lowest, is D, E, (C and F the same) and B. In Series ii the rank order is E, D, C, and (B and F the same). The differences as shown are so small as to be of doubtful significance.

TABLE II

THE AVERAGE FREQUENCY OF OCCURRENCE OF EVERY ATTITUDE IN SERIES I AND II

(Calculated in percentages)

Attitude	Series i	Series ii	Ave.
A	34.3	37.7	36.0
B	7.3	4.1	5.7
C	9.5	11.5	10.6
D	22.9	19.3	21.1
E	16.5	23.7	20.1
F	9.5	4.1	6.9

There appears to be a general sequence with which these attitudes make their appearance in the course of a particular observation. Attitude A usually appears first and is frequently followed by C or B; if C occurs, B or A frequently follows; if B occurs, A almost invariably appears. In cases where E occurs it usually makes its appearance immediately after the presentation of the stimulus-object or it may follow A. Attitudes D and F do not seem to fit into any regular sequence. When these latter two occur at or near the beginning of the observation, they fre-

quently last throughout its course. Attitudes A, D, and E were found to be the most constant.[13]

PART II

Series vii and viii. The purpose of the experiments in Part II was, as we have said, to determine the effect of the adoption of particular attitudes upon performance, or, stated in other terms, to determine what experiences actually occur when *O*s assume particular attitudes. The attitudes which the *O*s were instructed to adopt one by one were those which had appeared under the general instruction in Series i and ii, namely, the Attitudes A, B, C, D, E, and F. We presented visual and auditory objects which were slightly more complex than those in previous series. Prior to a given stimulation *O* was instructed to adopt a certain one of these attitudes and to give a complete account of the course of experience occurring under it. He was to report, in addition, the attitude actually assumed prior to stimulation.

Before the experiments began, *O* was given typewritten sheets characterizing each of the six attitudes. The characterizations were made clear and simple, and contained the descriptive terms previously used by each *O*.

Stimulus-objects. The objects were so designed that they could be immediately taken as object, as an experience whose object-meaning was not at first apparent, as an object whose members might be noted, as a pleasant or unpleasant object, or, finally, as an object eliciting expressions of 'taste.' Series vii was composed of visual objects and Series viii of auditory. Presentation time was 5 sec.

Series vii. Color figures on color grounds. Various types of geometrical figures were cut from colored papers and pasted on backgrounds of color. The figures were approximately 6 cm. in height and all were placed in the center of the color-field. They were (1) a distorted and bulging red figure of roughly circular form pasted on reddish yellow, (2) a less distorted circular orange on red, (3) a triangular red figure on blue, (4) a violet figure of elliptical form on green, (5) a red figure of perfectly circular form on greenish blue, (6) an orange figure of irregular cloudlike form on green, and (7) a greenish yellow figure forming a segment on violet.[14]

Series viii. Rhythmical successions of tone from the two oscillators. The pitch of the two tones used and the rhythm suggested well-known musical selections. Two or three measures were chosen from *Old Black Joe, Bridal*

[13]Reports are quoted *in toto* in Appendix A, which is retained in the archives of the Department.

[14]The Stoelting numbers for the colors in this series are 1-4, 3-2, 2-13, 14-9, 1-11, 3-9, and 6-14.

March from Lohengrin, Onward Christian Soldiers, Funeral March (Chopin), Spring Song (Mendelssohn) and the bugle call *Reveillé.*[15]

Before these series were begun, *O* was given a short series of objects of similar construction in order that he might acquire facility in adopting the attitudes.

Instructions. The following instructions were given the *Os.*

At the signal 'ready,' you are to adopt, and keep as constant as possible, a particular attitude which will be referred to by letter. An example is: '*Ready, Attitude D*' which will mean that you are to adopt the attitude which has been characterized under that type. An interval of time will elapse. At the signal 'now' you will be presented with a visual (auditory) stimulus-object. Your report is to contain (1) a complete description of your experiences occurring during the period of stimulation, and (2) the type of attitude prior to and during the stimulation. Refer to the attitudes by letter.

It was suggested to the *Os* that they first name the attitude actually adopted and then give the commentative account.

From *O*'s report *E* traced out the various functions in their order of appearance. For example, if an *O* reported that he took the presentation as an object, gave its common meaning, and the like, it was evident that he was carrying through what we shall call Function *a*. Function *a* refers to a performance carried out under Attitude A, Function *b* under Attitude B, etc. As a check upon our classification of functions as reported and to avoid misjudgment or misinterpretation, we also asked the *Os* to hark back, at the end of the report, and to indicate by letter the attitude (or attitudes) assumed. In all cases, *E*'s identification of the function involved (*a* to *f*) corresponded with *O*'s designation of the attitude taken (A to F).

Results of Series vii and viii. Since every stimulus-object was presented six times to every *O* and each object was observed under all the attitudes, the total number of reports from the three *Os* was 234.

The *Os* were successful in adopting, in preliminary trials, attitudes A, B, D, E, and F. Attitude C (critical survey of observer) proved to be extremely difficult to assume. In order to account for this difficulty we may note that it is, in some respects, not coördinate with the others. It is, as we have stated, an attitude under which *O* himself is the object of his own survey; while in the case of the other attitudes the objects surveyed are of another kind. It involves chiefly a spontaneous apprehension or realization that the trend or course of the experiences, correlated with stimulation, is or is not in the direction required by the instruction and involves the organism's own mode of guiding or directing this trend, *i.e.* by means of a reinstatement of the

[15]The frequencies of the tones employed were 1000–896, 896–1000, 896–1000, 896–1000, 1280–1000 and 1000–1536. Each melody began with the first frequency of each pair as here given.

original task (self-instruction); while in the other attitudes the course of experience is largely determined by means which arise not only from the originally formulated task but also from external circumstances (occasional instruction). Attitude C may and usually does mean that *O* is temporally embarrassed by the course which experience has been taking and tries to rectify it. The attitude arises spontaneously out of experience as already going on in some direction, and it cannot be successfully maintained unless the direction justifies its appearance. In the attempt to adopt Attitude C the *O*s resort to slightly different procedures, each of which is a form of self-instruction. They note that the attitude may appear for an instant; but it is apparent from their reports that by the time the stimulation begins another attitude has superseded it. This is usually Attitude A or B; and the experiences following usually fall under one of these types. Reports illustrative of the attempts to adopt C are:

M: [Series vii, 4] Fore-period, Att. C for an instant. The rest of the time such an unspecific attitude that I couldn't name it anything—a sort of waiting to see what was going to happen. When the object appeared, it was a patch of purple color on a bigger patch of color. [Series viii, 4] Att. C in the fore-period; it then fluctuates to D and to B. In the first part I observed the notes.

F: [Series vii, 5] Shifted to Att. A before the exposure. Decided to let the thing suggest some object. The meaning that came was that of part of a clown's dress. [Series viii, 2] I wondered what I should do. Decided to wait and see what it was, to see whether it suggested anything. It meant 'that familiar thing.'

G: [Series vii, 1] Att. C and at the beginning of the exposure Att. A. I said "oh, that's it," recognizing it as one I'd seen before. [Series viii, 6] Since the instructions had been given I had been badgering myself subvocally with phrases recalled from your characterization of Att. C. But I don't think at the moment of the sound that sort of thing was there, at least not clearly. The sound itself was predominantly A.

G was, more often than the other *O*s, successful in assuming Attitude C under instruction. As the series progressed he was more and more successful in assuming it, as well as in executing its correlative function. From his reports we discovered that his success was due to his observation of the somaesthetic context for the attitude. Only on one occasion, however, was the attitude kept constant throughout the period of stimulation; and the principal effect on experience was that the object was to some extent obscured.

G: [Series viii, 5] Att. C, which persisted till the end of the sound. There was a somaesthetic context for this attitude, what I should call in commonsense a disturbed feeling—a mass of rather dull and heavy pressures in the forehead. At the same time there was a sort of following through of the sound.

Under the instruction to adopt Attitude C, the reports (as might be expected from the above considerations) show special variability of attitude and consequent variability of performance.

Taken all together, the reports show that there were no appreciable qualitative differences of attitude as between the visual and auditory series or as between the *Os*, all of whom gave the same general type of report in the two series. Moreover, the *Os* show like attitudinal and functional variability in Series vii and viii. The only differences that are apparent are those which can best be treated statistically.

When instructed to adopt a particular attitude, our *Os* were commonly unable to maintain that attitude throughout a 5-sec. period of stimulation. Table III shows the degree of the inconstancy. Instead of a single attitude, there occurred an average

TABLE III

AVERAGE NUMBER OF FUNCTIONS OCCURRING UNDER FORMAL INSTRUCTION FOR EACH OF THE SIX ATTITUDES

Series	Prescribed Attitude	Observers *M*	*F*	*G*
vii	A	2.6	2.5	1.8
	B	3.0	2.0	1.6
	C	3.6	2.1	1.4
	D	2.7	1.5	1.4
	E	2.7	2.0	2.1
	F	3.2	2.0	1.6
	Ave.	2.9	2.0	1.6
	Ave. for all *Os*		2.2	
viii	A	2.6	2.1	1.6
	B	4.0	1.5	1.8
	C	4.0	2.0	2.0
	D	3.0	1.2	1.8
	E	2.5	2.0	1.2
	F	3.1	1.5	3.0
	Ave.	3.2	1.7	1.9
	Ave. for all *Os*		2.4	

of 2.3 attitudes in each period (in Series vii, 2.2; in Series viii, 2.4.) The slight difference between the average number for the two series shows that they are comparable in respect to the number of functions which they elicit.

Table IV presents the average frequency (percentages) with which each of the functions appears under a given attitude, as the attitude was formally prescribed. When an *O* is instructed to assume Attitude A, Function *a* may follow (say) in 60% of the cases, Function *b* in 3%, etc. We might expect, under such instruction, that the corresponding function would occur more frequently than any other. This is true, save in the case of Function

a, which sometimes numerically exceeds the function corresponding to the specified attitude. Upon instruction to adopt a particular attitude, the corresponding function appears in about one-half of all the observations (45.7%).

There are slight individual differences between *O*s with respect to the constancy with which the correlative function appears. In

TABLE IV

FREQUENCY OF OCCURRENCE IN PERCENTAGES OF THE SEVERAL FUNCTIONS UNDER SPECIFIED ATTITUDES

	Prescribed Att.	Series vii Functions *a*	*b*	*c*	*d*	*e*	*f*	Series viii Functions *a*	*b*	*c*	*d*	*e*	*f*
	A	60	3	4	8	15	3	49	19	16	4	0	9
	B	35	14	7	3	21	21	29	23	15	8	0	18
	C	28	4	14	12	18	21	22	6	27	20	10	12
M	D	27	0	17	34	14	6	19	12	4	26	4	23
	E	13	2	2	7	46	27	21	0	4	9	48	5
	F	25	0	14	0	11	45	20	20	0	11	9	27
	A	50	25	7	0	7	3	58	27	8	0	0	5
	B	33	54	0	7	0	4	25	75	0	0	0	0
	C	58	0	8	10	22	0	55	8	13	0	5	16
F	D	13	0	4	75	5	0	0	0	5	88	5	0
	E	27	0	16	0	50	5	31	0	0	0	61	16
	F	15	4	3	3	7	65	16	8	0	0	8	66
	A	66	19	7	7	0	0	83	12	0	0	0	4
	B	66	33	0	0	0	0	52	47	0	0	0	0
	C	57	0	35	7	0	0	55	5	38	0	0	0
G	D	50	0	50	50	0	0	36	0	0	63	0	0
	E	36	0	4	7	47	0	66	0	0	0	50	0
	F	33	0	10	14	4	47	44	9	0	8	8	29

TABLE V

FREQUENCY IN PERCENTAGES WITH WHICH EACH ATTITUDE, AS PRESCRIBED BY INSTRUCTION, IS INITIALLY FOLLOWED BY ITS CORRESPONDING FUNCTION

Series	Presc'ed Att.	Initial Function	Observers *M*	*F*	*G*	Ave.
	A	a	100.0	85.7	100.0	95.2
	B	b	14.3	85.7	28.6	42.8
	C	c	0.0	14.3	14.3	9.5
vii	D	d	71.4	57.1	42.8	57.1
	E	e	71.4	28.6	42.8	47.6
	F	f	57.1	71.4	28.6	52.3
	A	a	33.3	100.0	83.3	72.2
	B	b	50.0	83.3	66.6	66.6
	C	c	16.6	16.6	83.3	38.8
viii	D	d	100.0	100.0	66.6	88.8
	E	e	100.0	50.0	50.0	66.6
	F	f	33.3	66.6	33.3	44.4

both series, *G* was, on the whole, most successful in following the formal instruction; *F* was next; and *M* was last.

The irregularities of Function *c* have already been discussed. When an *O* was instructed for Attitude C (Table IV), his subsequent performance was not as predictable as those for the other attitudes.

Function *a* (with one exception) followed most frequently. Next in order of frequency were *f* and *c*.

Table V expresses the average frequency with which each attitude (as specified by formal instruction) is followed by its corresponding function at the *beginning of the period* of stimulation. Here we find two main differences as between the visual and the auditory series. First, the prepotency of A and D is reversed; and, secondly, the increase in prepotency of Attitude C is increased with the auditory objects. We shall consider each of these in turn.

In Series vii we find that Attitude A is most prepotent for calling forth its functions initially. Its efficacy is 95%. In rank order from highest to lowest, Attitudes D, F, E, and B are approximately 50% efficient in calling forth initially their corresponding performances. In the tonal series we have a slightly different order of prepotency. Attitude D is most effective in initially calling forth its function; Attitude A is second in this respect; B and E, F and C are next in varying degrees of frequency.

A is extremely prepotent in vision and D is only average, while in audition the opposite is true. These differences we believe are not due to any variability of the function, but to the nature of the stimuli. The auditory objects are frequently recognized as objects only after several notes have been given. The visual objects, on the other hand, may be recognized at the first instant. By the operation of the same factor, the prepotency of Attitude D is increased in Series viii.

In the visual series Attitude C is exceedingly low, (lower than chance, which would give it 16.6%). The predominance of other factors and the inefficacy of attitude are positively demonstrated by these figures. When we come to the auditory series it appears that Attitude C is more prepotent; but if we note the figures for the *Os* taken separately, we see that the increase occurs only in the case of *G*. As we have pointed out in our discussion of the qualitative results, during the progress of Series viii, *G* was able to assume Attitude C under instruction and to carry out its cor-

responding function by resorting to an observation of the somaesthetic context for the attitude. The other *O*s never resorted to that means of adopting and maintaining the attitude.

Conclusions

(1) When observers are presented with visual and auditory stimulus-objects, without specific formal instruction, they may assume a variety of attitudes. Among these attitudes we have distinguished six typical kinds; (1) a casual survey of objects, (2) an inquiring survey of objects, (3) a critical survey of observer as observer, (4) a critical, particularizing survey of objects, (5) a consideration of objects as pleasant or unpleasant, and (6) an apprehension of objects as conforming, or as failing to conform, to a conventional standard.

Although we do not, of course, maintain that we have studied all possible attitudes that an observer may adopt, our classification may aid experimenters in attempting to account for discrepancies in result which are attributable to attitudinal factors. Our enumeration appears to comprise with fair adequacy those more general attitudes by way of which the psychological observer (and very likely many another observer as well) relates his performance to apprehended objects, to himself, and to his task. It does not attempt to include a host of special attitudinizing postures which are indicated by such comments as 'This is difficult,' 'I am getting on,' and 'What a bore,' which are commonly comprised under the term *Bewusstseinslagen.*

(2) When observers are specifically instructed to adopt a certain type of attitude, they usually succeed in the pre-exposure period; but they fail, on the whole, to maintain this particular attitude during an entire period of stimulation no longer than five or ten seconds. This failure results in a variety of performances, among which the performance corresponding to the attitude specified by formal instruction appears about one-half the time. We conclude from this fact that the specific instruction, designed as a means of assuring constancy of attitude and performance in experimentation, is by no means adequate even where observers are highly trained and carefully instructed. As an approach to an adequate control of attitudinal conditions it appears that frequent determinations of attitude must be made throughout any series of observations in descriptive and in psychophysical

experiments, and that self and occasional instructions have quite as much to be taken into account as determinants of our psychological performance as have verbal commands of the formal kind. With the appearance of these additional determinants of psychological function, the inadequacy of the view which regards performance as a 'response' unequivocally determined by a 'stimulus' becomes still more apparent than before.

SOME FACTORS AFFECTING THE RELATIVE EFFICIENCY OF CERTAIN MODES OF PRESENTING MATERIAL FOR MEMORIZING

By Helen Lois Koch, University of Texas

Little agreement is to be found among the multifarious studies concerned with the problem of the relation between the sense-avenue or combination of sense-avenues by which material to be learned is presented and the ease with which memorizing takes place.[1] The relation is probably one affected by many factors, and some of these are factors whose influences are thrown into relief by what appear to be minor differences in experimental procedure. We would point out, moreover, that while the problem at hand is frequently dealt with as one of sense-organ efficiency, the sense-organ is probably never the only difference between two methods of exposure when the total learning situation is considered. When, for instance, a complex auditory stimulus, such as a three-syllable word, is presented for two seconds, the exposure of the word parts is necessarily successive and intermittent. The visual presentation of the same word, on the other hand, results, objectively at least, in a constant exposure for two seconds of all of the syllables.

Some of the probable variables other than the size of the stimulus-reaction unit are the imagery of the subject, his effort and emotional tone, his familiarity with the exposure condition, and the physical intensity and extent of the stimulus as well as the accuracy of the stimulus' registration. How significant each of the foregoing is or may be still remains to be discovered.

In view of this state of affairs we shall not center our attention on the order of merit in any absolute sense of the seven different

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[1]The reader who is not familiar with the controversies in this field may find valuable the reviews of the literature given in the following articles: V. A. C. Henmon, The relation between mode of presentation and retention, *Psychol. Rev.*, 19, 1912, 79-97; and F. J. O'Brien, A qualitative investigation of the effects of mode of presentation upon the process of learning, this Journal, 32, 1921, 249-283.

methods of presentation with which we are concerned, but rather upon the question of the relation between their order of merit and certain specific variables. The variables we have chosen to investigate are: (1) the method of measuring learning, *i.e.* whether in terms of speed, accuracy, or accomplishment; (2) the stage in the case of a given unit at which the measurements of accomplishment are taken; (3) the nature of the recording response, *i.e.* whether vocimotor or manumotor; and (4) the degree of the subject's familiarity with the exposure method.

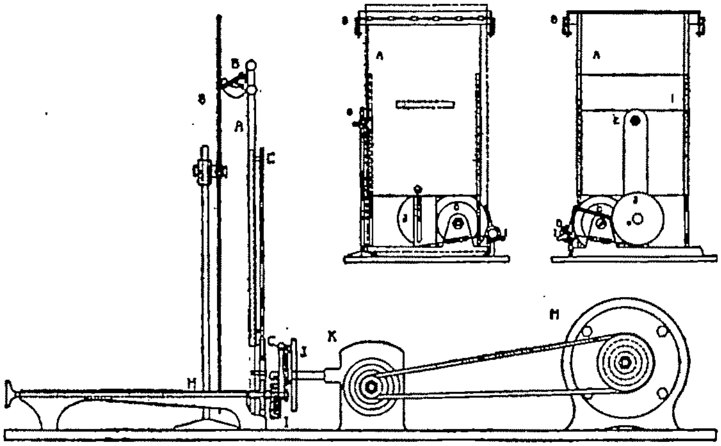

FIG. 1. EXPOSURE APPARATUS
(Front, Side, and Back Views)

We would remind our readers, furthermore, that when for purposes of convenience we refer to the 'visual presentation,' the 'auditory presentation,' etc., we do not imply that the sense-organ was the only difference between the conditions with which we dealt; we are referring rather to the total stimulating situations that were employed.

To advert to the details of method, let it be said that in the case of the so-called 'visual presentation' the nonsense syllables, carefully selected[2] lists of 12, which were the units of material to be memorized, were presented to the *S* by an automatic exposure apparatus[3] at the rate of one every 2 sec. (See

[2]The lists were selected according to the principles laid down by C. W. Luh in his monograph, The conditions of retention, *Psychol. Monog.*, 31, 1922, (no. 142), 1-87.

[3]The apparatus was designed by Mr. Ben Holland. It consists of an Error-No Machine attached through proper gearing to a constant speed motor.

Fig. 1.) The S attempted, when one syllable was exposed, to indicate what the next one in the list was. This is the anticipation method first used by Finkenbinder.[4] Exposures of the units of material were continued with only 4 sec. elapsing between trials until the Ss could anticipate successfully once all of the syllables in the list. The technique permits a fair control of the variable, degree of learning, and reduces, at least as a prominent factor in the results, such influences as the caution of the S, a source of variation which many of the older studies ignored.[5]

In the case of the so-called 'auditory presentation,' the syllables were spelled aloud at a rate corresponding to the one used in the visual series. The simultaneous and successive combination methods employed merely different patterns of these simple auditory and visual presentations. In the 'simultaneous combination' condition, as the S viewed each syllable, it was spelled aloud to him by E. When the alternation techniques were used, the list to be memorized was presented in any one trial according to either the visual or auditory method, but the two types of presentation were alternated in one- and two-trial rhythms. In two cases, furthermore, the trial sequences were initiated by a visual presentation; in two, by an auditory.

Each of the 7 exposure conditions were combined with two forms of recording response, a written or a spoken. We thus dealt with a total of 14 different learning situations. In the case of the written anticipation response, each syllable was covered up as soon as it was recorded; but it is to be noted that to the extent S was able to anticipate a syllable, whether right or wrong, he saw for a brief period what he wrote and, of course, felt the muscular effects of his reaction. When, on the other hand, the response was vocal, S spelled aloud the syllable, thus both feeling and hearing his reaction.

The spelling method used both in the presentation and the recording adjustment was employed because it was felt (1) that opportunities for gross misunderstanding were less than when the syllables were pronounced as wholes; (2) that, in non-laboratory situations, one factor which might distinguish the auditory from the visual method of presentation would be the amount of material dealt with in one reaction unit, the auditory unit in any sequence being smaller than the visual and more frequently subjected to successive integration; and (3) that the spelled response might more nearly parallel in the units emphasized the written response, since, of course, nonsense syllables, a material with which the subjects were not familiar, were being recorded.

At no time did we attempt to interfere with the imagery of the S. Cohn,[6] Curtis,[7] O'Brien,[8] and others have shown that inhibition of subvocal activity is distracting in its effect. In fact, Curtis and O'Brien even claim that voci-

[4] E. O. Finkenbinder, The curve of forgetting, this JOURNAL, 24, 1913, 8-32.

[5] See even the following recent studies as examples of the failure to control the variable mentioned: D. A. Worcester, Memory by visual and auditory presentation, *J. Educ. Psychol.*, 16, 1925, 18-27; and O'Brien, *loc. cit.*

[6] J. Cohn, Experimentelle Untersuchungen über das Zusammenwirken des akustisch-motorischen und des visuellen Gedächtnisses, *Zsch. f. Psychol.*, 15, 1897, 161-183.

[7] H. S. Curtis, Automatic movements of the larynx, this JOURNAL, 11, 1900, 237-239.

[8] O'Brien, *loc. cit.*

motor activity cannot be totally inhibited in the initial stages of the learning. Since our *Ss* were merely requested to suppress gross speech movements, except as they were necessary in the oral response, few disturbing inhibitions were probably set up.

Fourteen young college women[9] similar in age, ability, and interest served as *Ss* during the experiment, learning the 4 practice and 42 experimental units of material at the rate of one a day. The 42 experimental units were arranged in 3 cycles with 14 in a cycle. In order to control practice effects, each of the 14 *Ss* proceeded through the 14 learning situations in a different order. No attempt was made, however, to use all the factorial orders possible. Each presentation-condition was merely placed an equal number of times in each position in the cycle sequence. Since every *S* and each unit of material was used an equal number of times under all of the exposure situations, inequalities in the ability of the *Ss* or in the difficulty of the lists were probably eliminated as significant factors in our results.

When the results of the 3 cycles are combined, each average is based upon 42 measures. The cyclic arrangement and the relatively small number of cases involved in the average for each cycle would render unjustifiable the use of the probable-error technique in estimating the reliability of our findings. For this, consistency of finding, whenever possible, must be the court of appeal.

In the discussion of results, the visual exposure methods will be referred to as V-s and V-w; the auditory, as A-s and A-w; the combined visual and auditory, as V+A-s and V—A-w; and the alternation combinations, as Al. AV-1-s, Al. AV-1-w, Al. AV-2-s, Al. AV-2-w, Al. VA-1-s, Al. VA-1-w, Al. VA-2-s, and Al. VA-2-w. The letters 's' and 'w' in our system of abbreviations indicate whether the anticipatory response was spoken or written; the numbers, in the case of the alternation conditions, indicate whether the alternation units were 1 or 2 trials in length; and the capital letter appearing first in the designations for the latter, the form of the presentation initiating the sequence of trials.

Our results will be described in terms of the following measures: (1) the average number of repetitions required by the *Ss* to learn to anticipate correctly for one time all of the syllables in a list; (2) the average number of errors made in mastering a unit of material; and (3) the average number of letters correctly anticipated in each of the repetitions in the learning sequence.

The number of trials for learning any list was considered to be one less than the number of exposures, and the last or successful trial was viewed not as a part of the learning period but as the occasion on which mastery was demonstrated.

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[9]The *Ss* ranged in age from 18 to 22 years and in Army Alpha score from 149 to 182—*i.e.* they ranked with the upper quartile of American college women. Personal loyalty and knowledge of their own results sustained their motivation throughout the experiment.

The author wishes to express her gratitude to the following subjects: Mary Baillio, Nedra Newkirk, Catherine Davis, Mary Catherine Taylor, Mary Margaret Taylor, Margaret Trippet, Sarah Daniels, Amanda Herring, Jean Granger, Antoinette Bracher, Francis Hatcher, Minnie Pearl Thomas, Maretta Talbot, and Marion DeShazo.

An error was defined arbitrarily as either the misplacement or misspelling of a syllable or both; and it was assumed that only one error could be made in a syllable. The method of scoring is not entirely satisfactory but seemed justified because in the case of a combined misplacement and misspelling it was frequently impossible to tell whether *S* had in mind the syllable we suspected he was attempting to spell. Since omissions were not considered errors, the error record should have value chiefly in indicating marked disturbances in the integrating process—*i.e.* inhibitions and interferences, lack of clarity in the impression, carelessness on the part of the subject, etc.

For a study of the details of the more positive phases of the learning process, *i.e.* the successful integrations, the measure that seemed most satisfactory was that of the number of letters correctly anticipated by an individual in each trial. This measure is the one on the basis of which our learning curves were constructed.

It is clear, of course, since the number of letters correctly recorded by the *S*s were merely averaged for each repetition, that trials representing the same relative stage in the learning process of the various *S*s were not grouped together. Since, furthermore, if *S* had mastered a list, he was credited in all subsequent trials with a constant score of 36 letters, he could no longer, being on what might be called a 'success plateau,' help to raise the group curve by any gains. This condition obscures in places the meaning of some of the figures but an analysis of them should, in a general way at least, aid in the discovery of whether or not the relative effectiveness or ineffectiveness of a method of presentation is associated with a particular stage of the learning.

Relative Effectiveness of the Exposure Methods According to Speed and Accuracy Measures

Conditions V+A, V, and A retain the same position of merit relative to each other according to the two criteria of speed and accuracy, V+A ranking highest and A lowest. (See Tables I, II, III, and IV.) A, furthermore, in all cycles and according to all measures ranks lowest among the 7 conditions. Condition V, on the other hand, relative to the alternation conditions, seemingly makes for less rapid learning than for accurate. At least, in 13 out of the 24 comparisons with one or another of the alternation

TABLE I

Average Number of Trials Required by a Subject for Learning a List of Twelve Nonsense Syllables
(Oral Anticipation Series)

Cycle	Type of presentation: V	A	V+A	Al. VA-1	Al. VA-2	Al. AV-1	Al. AV-2
I	14.00	16.07	13.14	13.72	13.28	13.42	13.50
II	9.86	11.50	9.07	9.86	10.93	9.14	10.79
III	8.57	11.00	8.36	8.79	9.14	9.29	11.43
Average of I, II, and III	10.81	12.86	10.19	10.79	11.12	10.62	11.91

TABLE II

AVERAGE NUMBER OF TRIALS REQUIRED BY A SUBJECT FOR LEARNING A LIST OF TWELVE NONSENSE SYLLABLES
(Written Anticipation Series)

Cycle	Type of presentation						
	V	A	V+A	Al. VA-1	Al. VA-2	Al. AV-1	Al. AV-2
I	15.21	17.64	14.00	13.86	14.71	17.07	14.21
II	10.36	13.00	10.36	11.21	10.21	11.79	10.71
III	10.00	11.00	9.77	8.79	9.71	9.57	10.29
Average of I, II, and III	11.86	13.88	11.37	11.29	11.54	13.14	11.77

TABLE III

AVERAGE NUMBER OF ERRORS MADE BY A SUBJECT IN LEARNING A LIST OF TWELVE NONSENSE SYLLABLES
(Oral Anticipation Series)

Cycle	Type of presentation						
	V	A	V+A	Al. VA-1	Al. VA-2	Al. AV-1	Al. AV-2
I	12.07	14.00	7.21	13.57	11.29	11.43	13.64
II	7.57	8.50	6.64	9.64	8.43	6.43	8.57
III	6.29	9.79	6.07	7.07	8.21	6.57	9.29
Average of I, II, and III	8.64	10.76	6.64	10.09	9.31	8.14	10.50

TABLE IV

AVERAGE NUMBER OF ERRORS MADE BY A SUBJECT IN LEARNING A LIST OF TWELVE NONSENSE SYLLABLES
(Written Anticipation Series)

Cycle	Type of presentation						
	V	A	V+A	Al. VA-1	Al. VA-2	Al. AV-1	Al. AV-2
I	12.21	14.64	10.29	13.07	11.86	12.64	10.86
II	5.71	13.07	7.57	7.79	8.57	10.21	7.14
III	7.22	9.50	7.23	5.07	7.00	5.71	6.43
Average of I, II, and III	8.38	12.40	8.36	8.64	9.14	9.52	8.14

conditions which are offered by the material gathered in the 3 cycles, condition V is outranked when the criterion of merit used is the number of trials required for learning; whereas in the case of errors the same comparisons yield a proportion of 9 in 24. It seems not improbable that the visual method, while stimulating relatively accurate and unified impressions, may not furnish the motivation nor the opportunities for locating errors that the alternation methods do; and, hence, the latter may lead to the more rapid attainment of the required standard of mastery. At any rate, it is questionable whether we are justified in using trial and error records interchangeably.

With respect to each other the alternation conditions show no very consistent superiority-inferiority relationships. This probably accounts in part for the fact that the correlation of the ranks of the 7 conditions arranged according to speed and accuracy measures are occasionally low. When the results for the 3 cycles are grouped, these correlations are +0.89 and +0.63 for the written and oral response series, respectively. (See Table VII.)

TABLE V

ABSOLUTE AND RELATIVE GAIN OF CYCLE III OVER CYCLE I
(Oral Anticipation Series)

	Type of presentation						
Item compared	V	A	V+A	Al. VA-1	Al. VA-2	Al. AV-1	Al. AV-2
Absolute saving in learning trials	5.43	5.07	4.78	4.93	4.14	4.12	2.07
Relative saving in learning trials (%)	39	28	36	36	31	31	16
Absolute reduction in total errors	5.78	4.21	1.14	6.50	3.08	4.86	4.35
Relative reduction in total errors (%)	48	30	16	47	36	43	32

TABLE VI

ABSOLUTE AND RELATIVE GAIN OF CYCLE III OVER CYCLE I
(Written Anticipation Series)

	Type of presentation						
Item compared	V	A	V+A	Al. VA-1	Al. VA-2	Al. AV-1	Al. AV-2
Absolute saving in learning trials	5.21	6.64	4.23	5.07	5.00	7.50	3.92
Relative saving in learning trials (%)	34	38	30	37	34	44	28
Absolute reduction in total errors	4.99	5.14	3.06	8.00	4.86	6.93	4.43
Relative reduction in total errors (%)	41	35	19	61	41	63	41

TABLE VII

CORRELATIONS BETWEEN THE RANKS HELD BY THE EXPOSURE METHODS ON THE BASIS OF MEASURES OF SPEED AND ACCURACY

Cycle	Oral anticipation	Written anticipation
I	$+0.86\pm.02$	$+0.47\pm.11$
II	$+0.58\pm.08$	$+0.63\pm.06$
III	$+0.86\pm.02$	$+0.78\pm.02$
All cycles	$+0.89\pm.02$	$+0.63\pm.06$

TABLE VIII

Average Number of Letters Correctly Anticipated after a Single Exposure of Material

Cycle	Oral anticipation V	Oral anticipation A	Oral anticipation V+A	Written Anticipation V	Written Anticipation A	Written Anticipation V+A
I	8.07	5.05	7.00	6.80	6.57	8.57
II	9.93	7.59	9.43	10.83	7.29	8.93
III	9.75	3.60	11.86	8.95	8.07	9.07
Average of I, II, and III	9.25	7.41	9.53	8.86	7.31	8.86

TABLE IX

Average Number of Letters Correctly Anticipated on the Fifth Exposure

Cycle	Oral anticipation V	Oral anticipation A	Oral anticipation V+A	Written anticipation V	Written anticipation A	Written anticipation V+A
I	23.14	20.36	21.64	22.29	20.64	21.29
II	27.76	22.72	25.00	26.28	20.86	25.86
III	28.72	23.72	25.50	28.00	22.71	25.50
Average of I, II, and III	26.54	22.27	24.05	25.52	21.40	24.22

TABLE X

Correlation between the Ranks Held by the Exposure Methods in the Various Cycles

Cycles correlated	Oral anticipation Trials	Oral anticipation Total errors	Written anticipation Trials	Written anticipation Total errors
I and II	$+0.53\pm.09$	$+0.68\pm.04$	$+0.42\pm.12$	$+0.65\pm.06$
I and III	$+0.32\pm.16$	$+0.75\pm.03$	$+0.47\pm.10$	$-0.04\pm.21$
II and III	$+0.64\pm.06$	$+0.65\pm.06$	$+0.14\pm.20$	$-0.04\pm.21$

Relative Effectiveness of Three Exposure Methods Rated in Terms of Initial-Trial, Later-Trial, and Total-Learning Series Accomplishment

On initial trial accomplishment, measured in terms of the number of letters successfully anticipated, it is possible to compare only conditions V+A, V, and A, as the initial trial in the case of the alternation conditions involves either a visual or an auditory presentation.

It is significant that condition V+A, when one considers the number of letters memorized in the first trial, is not so clearly superior to condition V as when comparisons are made on the basis of the total trials and errors involved in the learning. In fact, in 3 out of 6 possible first-trial and 6 out of 6 fifth-trial comparisons, V rates higher than V+A, whereas, according to the 18 comparisons based upon the speed and accuracy measures previously discussed, only once does condition V fail to rate below condition V+A.

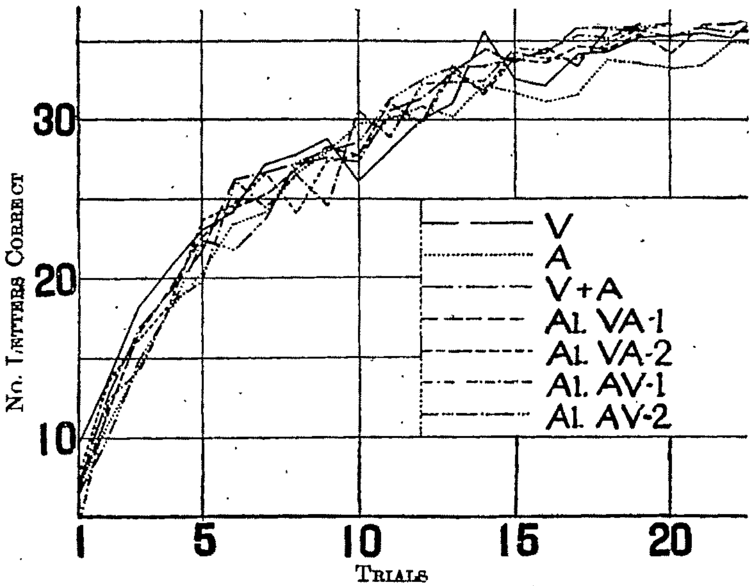

Fig. 2. Learning Curves: Cycle I, Oral Anticipation

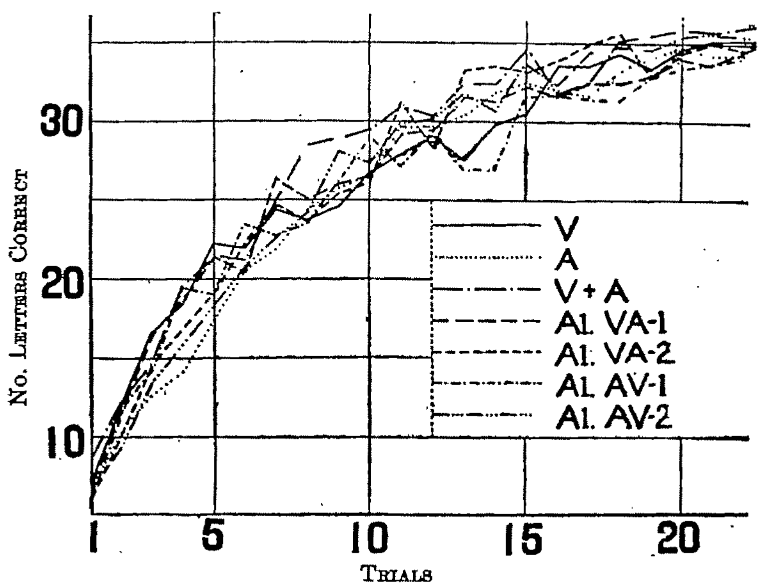

Fig. 3. Learning Curves: Cycle I, Written Anticipation

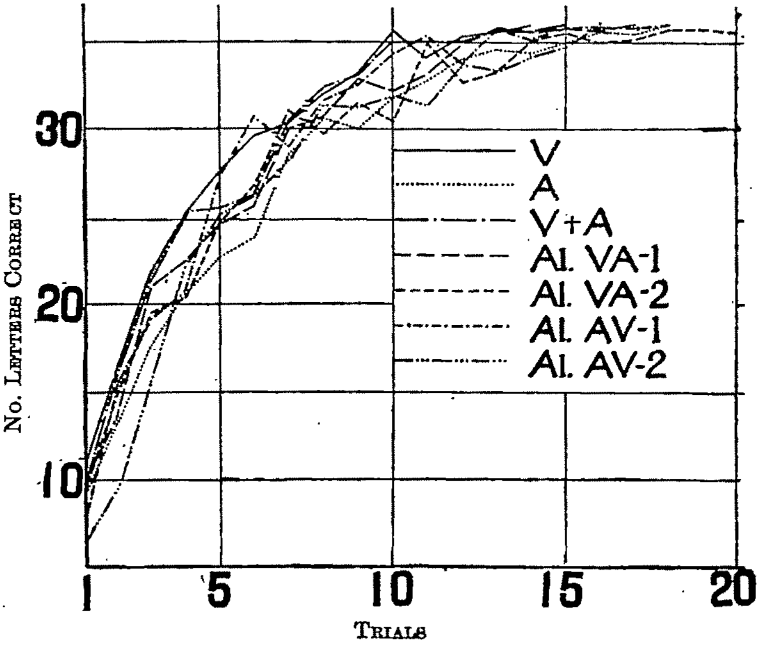

Fig. 4. Learning Curves: Cycle II, Oral Anticipation

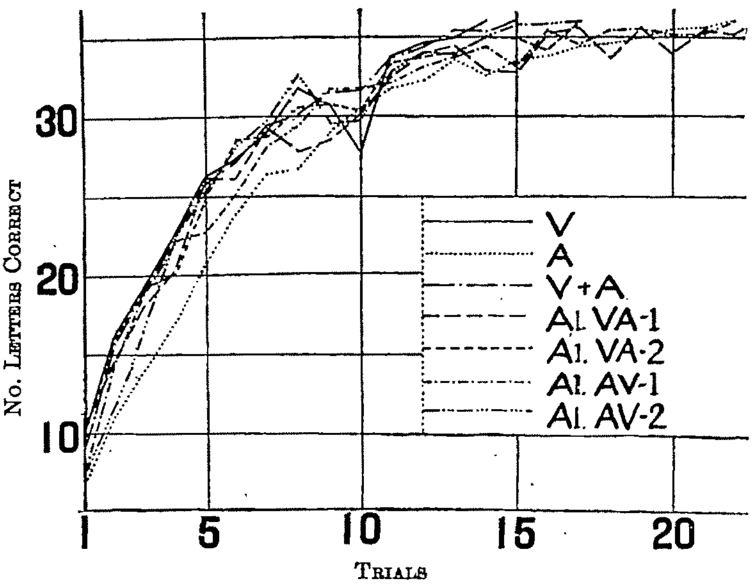

Fig. 5. Learning Curves: Cycle II, Written Anticipation

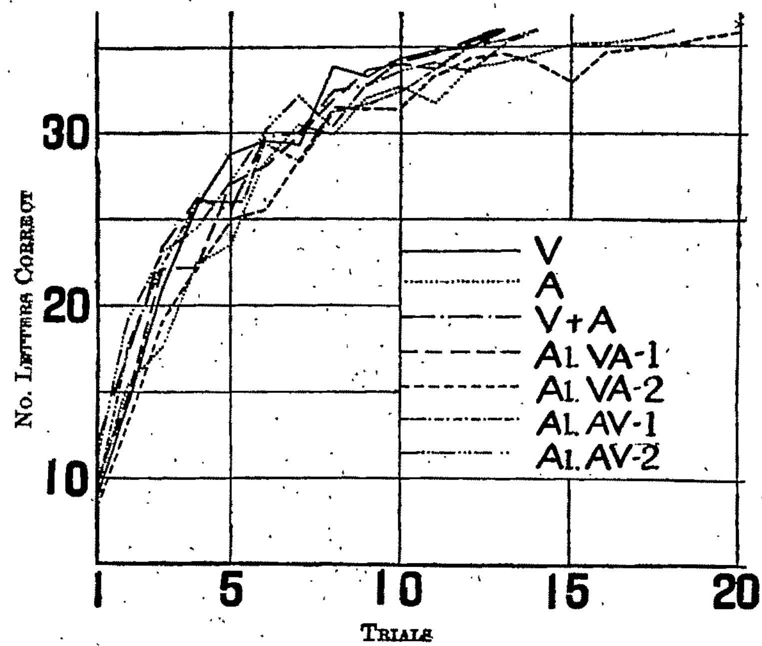

Fig. 6. Learning Curves: Cycle III, Oral Anticipation

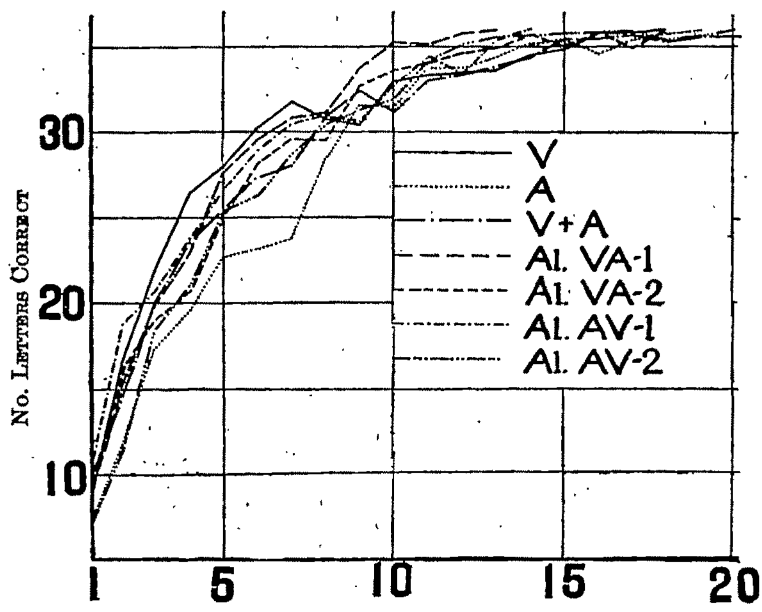

Fig. 7. Learning Curves: Cycle III, Written Anticipation

One reason for this difference is suggested by our *Ss*, who insisted that in the early stages of the learning they felt confused by the combined visual and auditory stimulation, although they put forth additional effort to capitalize all that was given. Significant in this connection also are the observations (1) that the curve for condition V+A is rather uniformly above that for condition V after the first 5 trials, whereas it is only occasionally above in the first 5 (see Figs. 2-7); and (2) that the superiority of condition V+A according to measures of accomplishment is most obvious in the last cycle.

This apparent divergence in the rating condition V+A receives, when evaluated in terms of total learning or later-trial accomplishment as contrasted with initial-trial performance, may throw light on the fact that men like Henmon[10] and others, who used one or a very limited number of presentations of material, did not always find the combined stimulations particularly advantageous.

Effect of Practice upon the Relative Effectiveness of the Exposure Methods

Practice in the use of an exposure method seems to influence its relative standing; or perhaps it is increasing familiarity with the material used that is the major determining factor in whatever shifts in rank appear from cycle to cycle among our results. Whatever the cause, however the visual presentation in the oral anticipation series improves with practice its standing with respect to the alternation presentations. The absolute and relative gains are in the case of the former relatively large. (See Table V.) The one-trial alternation methods also seem to raise with practice their rating in comparison with the two-trial methods, for in 5 out of 8 cases the former rank better in cycle III than in cycle I; in 2 cases, the same. The one-trial methods make both larger absolute and relative gains than the two-trial, according to both trial and error criteria. (See Tables V and VI.) It is worthy of note, too, that in the written anticipation series condition V+A lowers with practice its rank with respect to the alternation conditions. In the oral anticipation series the small gains of condition V+A

[10]Henmon, *loc. cit.*

in the third cycle are probably evidence of the same trend but the influences at work are not sufficiently great to displace the condition from first rank.

Table X shows the intercorrelation between the ranks held by the various conditions in the three cycles. It is to be noted that in the oral anticipation series the correlations are higher than in the written, and that in the former the correlations are higher for error scores than for trial. It is possible that the low correlations are an expression merely of the unreliability of our cycle averages, though it is questionable whether this is the total explanation, since the shifts commented upon in the foregoing paragraph seem rather consistent. Variety in impression, such as the alternation conditions furnish, may be of considerable value when the *S* is unfamiliar with nonsense syllables; whereas, as he becomes practiced, this advantage may be reduced and the disadvantages attendant upon shifting from one sensory set to another may be thrown into relief. The fact, moreover, that the one-trial alternation conditions seem to gain over the two-trial with practice is a finding not out of accord with Jersild's[11] thesis that in the case of shift the more mechanized or automatic the response the greater the loss through shift. Two trials as the *S* becomes practiced may have a relatively greater mechanizing effect than they do earlier in the learning.

Relative Effectiveness of the Exposure Methods in the Vocimotor and Manumotor Response Series

A comparative study of the effect upon memorizing of the vocimotor and manumotor anticipation responses indicates that the former makes for relatively greater speed though not consistently greater accuracy than the latter. That these results may be attributable, in part at least, to the crowding for time in the manumotor series (only 2 sec. were allowed for viewing the syllable and writing the next) is suggested by the facts (1) that improvement with practice was greater as a rule in the conditions characterized by the written response than by the spoken; and (2) that in condition V+A-w, a situation somewhat confusing because of the multiplicity of stimuli given, the written response series is conspicuously at a disadvantage, *i.e.* condition V+A ranks relatively lower in the written response series than the spoken, and

[11] A. T. Jersild, Mental set and shift, *Arch. Psychol.*, 14, 1927, (no. 89), 1-89.

the alternation conditions rank higher. It is to be noted, however, that our finding that written recording does not make for so much speed in memorizing as oral recording is in agreement with Smedley's.[12]

It was hoped that by the use of the two response series we would be able to reveal any marked relation that there might be between the sensory pattern of the impression and the nature of the recording activity. We expected, for instance, to determine whether an auditory stimulus, supplemented by the visual-motor impression gained by writing, would be more effective than the stimulus supplemented merely by the auditory-motor impression produced by speaking. It is interesting, then, that the ratio of trials required or errors amassed under condition V to those accumulated under condition A are essentially the same for both the graphic and oral series. Hence, if interference and facilitation effects are present they are so in different patterns and the sum total of the effects revealed through trial and error scores are about the same for the two recording series.

General Discussion of Results

The complexity of the interrelationships shown among our data make it seem probable that they are not to be interpreted in terms of only a very few principles. Each presentation condition, doubtless, has some disadvantages as well as some advantages, and these latter are not to be conceived as absolute in any sense. They have meaning only in relation to the clearly defined reference points of specific conditions; *i.e.* condition V+A may be superior to condition V for one or a group of reasons and to condition A for a different group of reasons. One type of exposure may be, furthermore, superior to another under one set of circumstances and not under another. The determination of the efficacy of a method, then, seems always a relative and specific issue—one of weighing against each other sums or products or combinations of factors among which are both favorable and unfavorable. The total relationship or end result of the weighing may be readily disturbed and even shifted in the opposite direction by what appear to be minor changes in the multitude of determining conditions. It seems well at this point to summarize

[12] F. W. Smedley, *Rep. of Dept. Child Study and Pedagogic Investigation*, No. 3, Chicago, 1900-1901, 1-63.

and even to expand somewhat upon some of the theories which may explain the interrelationships revealed in our data.

That condition A is so uniformly inferior to all of the others is interesting and in agreement with the findings of Münsterberg and Bigham,[13] Meumann,[14] Pohlmann,[15] Lay,[16] Fuchs and Haggenmüller,[17] and Gates;[18] but in disagreement with those of Hawkins,[19] Schuyten,[20] Henmon,[21] and Worcester.[22]

The lack of agreement with the latter four authors it is impossible to account for with any certainty, but we should be inclined to look for the causes in the differences which exist in the experiences of our *Ss*, the type of material used, our spelling procedure, and our methods of measuring results.

The ineffectiveness of the auditory relative to the other six of our exposure methods we feel can be accounted for as follows: The auditory method centers attention upon relatively small units, the letters, and provides a shorter stimulation time for each of these than do the other conditions. The former also makes for occasional lack of clarity in impression with resultant errors or distracting uncertainty and the habituation of erroneous reactions because they remain long unchecked; there is, moreover, little of such variety in presentation form as would lead to the ready detection of these. For support of our theory of the relatively inaccurate registration of auditory stimuli, we have the behavior and comments of our *Ss*, who not infrequently confessed their inability to discriminate between certain letters. O'Brien's *Ss* also reported the same difficulty.[23]

The rather uniform superiority of our simultaneous combination method is a finding in accord with those of Münsterberg and

[13] H. Münsterberg and J. Bigham, Memory, *Psychol. Rev.*, 1, 1894, 34-38.

[14] E. Meumann, *Vorlesungen zur Einführung in die experimentelle Pädagogik und ihre psychologischen Grundlagen*, I and III, 1911.

[15] A. Pohlmann, *Experimentelle Beiträge zur Lehre vom Gedächtnis*, 1906.

[16] W. A. Lay, *Experimentelle Didaktik*, 3rd ed., 1910.

[17] H. Fuchs and A. Haggenmüller, Studien und Versuche über die Erlernung der Orthographie, *Sammlung von Abhandlungen aus dem Gebiete der pädagogischen Psychologie und Physiologie*, 2, 1898.

[18] A. I. Gates, The mnemonic span for visual and auditory digits, *J. Exper. Psychol.*, 1, 1916, 393-403.

[19] C. J. Hawkins, Experiments on memory types, *Psychol. Rev.*, 4, 1897, 289-294.

[20] M. C. Schuyten, Experimentelle zum Studium der gebräuchlichsten Methoden im fremdsprächlichen Unterricht, *Exper. Päd.*, 3, 1906, 199-211.

[21] Henmon, *op. cit.*

[22] Worcester, *op. cit.*

[23] O'Brien, *op. cit.*, 273.

Bigham,[24] Smedley,[25] Frankfurter and Thiele,[26] Lüdeke,[27] Woody[28] and Von Sybel.[29]

The findings, moreover, of other investigators working at the more complex problems of visual education in the class-room point in the same direction.[30]

Kemsies,[31] Quantz,[32] Schuyten,[33] and Henmon,[34] on the other hand, had results somewhat at variance with this trend.

We have attempted to account for the rather consistent superiority, when total learning is considered, of condition V+A, over one or another of the other six conditions employed in our experiment in terms of (1) the summation of memory cues closely linked with only one type of sensory experience; (2) the stimulation of more than the usual amount of effort on the part of the *Ss*; (3) the simultaneous rather than the successive arousal of the associative retinues of the two sensory modes; (4) the relative accuracy of impression with the attendant result of fewer wrong habits; (5) the reduction of such distractions as uncertainty; (6) the simultaneous combination of impressions, one of which stimulates synthesis and the other analysis; and (7) the freedom from such interferences as may result from shifting from one rather well integrated and exercised sensory set to another.

Those cases in the present study in which condition V+A seems less effective than condition V are, in our opinion, the result of a crowding for time and of confusion following upon the reception of too many unorganized stimuli. In the written anticipation response series, for example, the former presentation method was

[24] Münsterberg and Bigham, *op. cit.*
[25] Smedley, *op. cit.*
[26] W. Frankfurter and R. Thiele, Über den Zusammenhang zwischen Vorstellungstypus und sensorischer Lernweise, *Zsch. f. Psychol.*, 62, 1912, 96-131.
[27] D. Lüdeke, Experimentelle Untersuchungen über das unmittelbare Behalten bei besonderer Berücksichtigung der Prozesse der Aufmerksamkeit und des Wiederkennens. *Arch. f. d. ges. Psychol.*, 48, 1924, 213-247.
[28] C. Woody, The effectiveness of oral versus silent reading in the initial memorization of poems, *J. Educ. Psychol.*, 13, 1922, 477-483.
[29] A. von Sybel, Über das Zusammenwirken verschiedener Sinnesgebiete bei Gedächtnisleistungen, *Zsch. f. Psychol.*, 53, 1909, 257-360.
[30] J. J. Weber, *Comparative Effectiveness of Some Visual Aids in Seventh Grade Instruction*, 1925; and F. N. Freeman et al., *Visual Education*, 1924.
[31] E. Kemsies, Gedächtnisuntersuchungen an Schulkindern, *Zsch. f. päd. Psychol.*, 2, 1900, 21-30; 3, 1901, 171-183.
[32] J. O. Quantz, Problems in the psychology of reading, *Psychol. Monog.*, 2, 1897 (no. 5), 1-51.
[33] Schuyten, *op. cit.*
[34] Henmon, *op. cit.*

probably at a disadvantage because the time interval allowed for observing and recording each syllable was not sufficiently great to permit the subject to write at an entirely comfortable speed. When the stimuli to be capitalized are relatively numerous and wholly unfamiliar also, as they are in the initial trials especially, little time would be given for an organization of impressions. Temporary confusion and effort to a degree to be distracting might result, even though the *Ss* tended to attend to only one of the sensory modes.[35] The inference that this confusion would be prominent in the early trials is confirmed by our results which indicate that even in the series in which the anticipation response was oral, the relative rank of condition V+A tends to be somewhat lower during the initial adjustment period than later.

Our methods involving the alternation of visual and auditory exposures furnish an interesting contrast in many ways to the simultaneous combination scheme and supply results bearing upon several frequently debated psychological issues such as the nature of mental set and the conditions acting as determinants of the nature and degree of *transfer*.

Since the curves for the alternation condition exhibit a rather uniform upward trend, it seems safe to assume that learning accomplished in one trial under one form of stimulation transfers some of its effects to the learning in a successive trial and under stimulation of another sensory sort. Assuming, moreover, relatively complete and positive transfer from trial to trial, at least to the degree found in conditions V and A, one would expect the curves for the alternation conditions to be relatively angular, since visual and auditory presentations are not equally efficacious according to our measures. This expectation is realized. The appearance, however, in the intermediate and final stages of the learning curves for the alternation conditions of more than a normal number of depressions, with these depressions correlated in position with the shift from one exposure type to another, suggests some temporary interference or negative transfer effects as a result of shift.

That the advantage which the simultaneous combination methods hold over the simple visual presentation may be variety of impression, one might be led to believe by the fact that in the

[35] O'Brien, *op. cit.*, 257.

last cycle of the spoken responses series the latter conditions rank lower than in the first cycle. Presumably, as *S* becomes familiar with the material and method, variety in impression becomes less essential. It is significant, moreover, that in the more difficult and confusing written response series three cycles of practice are not sufficient to cause any conspicuous shift in rank on the part of the alternation conditions relative to the visual.

The results for the oral response series just described are out of accord, at least from a superficial point of view, with Jersild's[36] finding that losses through shift decrease with practice. It must be remembered, however, that Jersild's *S*s shifted from one type of material to another, while in our experiment the material remained constant and the change occurred merely in the method of presentation. To one of Jersild's conclusions—viz., the more mechanized the unit processes, the more difficult shifting becomes—our data lend support, for they seem to indicate that the one-trial alternation methods have a slight advantage over the two-trial.

Summary

The relative efficacy of the various presentation types dealt with in the study seems to be a function of the measure on the basis of which an evaluation of the types is made, the stage in the learning at which the estimates of accomplishment are taken, the degree of the *S*s familiarity with the material and with the details of method, and the form of recording response employed. The simultaneous combination of the visual and auditory presentation was, however, according to the measures applied, rather uniformly superior; and the simple auditory presentation was uniformly inferior. The simple visual and the alternation combinations, which tended to rate between the two exposure conditions just enumerated, showed with respect to each other few consistent superiority-inferiority relationships. Varying the size of the alternation unit from 1 to 2 trials seemed to have had no very conspicuous effect upon the speed or the accuracy of learning. If any difference existed, it was in favor of the one-trial unit. Recording responses of a vocimotor sort tended to beget better results than manumotor ones.

[36] Jersild, *loc. cit.*

The data of the experiment have a bearing on some of the theoretical problems of transfer and mental set. Shift in presentation type, when the material to be learned remains constant, does result in some interference effects, though these are usually not so great as to mask the positive transfer which occurs from trial to trial in the learning. Practice seems to reduce whatever advantages do accrue from variety in presentation form; and mechanizing the reaction unit seems to increase the difficulty of learning under conditions involving shift from one type of presentation to another.

RELATIONSHIP BETWEEN THE ABSOLUTE AND DIFFERENTIAL THRESHOLDS FOR AN AUDITORY STIMULUS

By John H. Kenneth and Robert H. Thouless, University of Glasgow

The problem with which this investigation is concerned is the relationship between the absolute and the differential threshold. Are they two different phenomena, or is the absolute threshold simply the limiting value of the differential threshold obtained when the lower of the presented stimuli γ_1 and γ_2 is made equal to zero? If the latter is the case, we should find continuity in the values of $\gamma_2 - \gamma_1$ (*i.e.* $\Delta\gamma$) or in any function of $\Delta\gamma$ which we may choose to measure when γ_1 is increased from zero to any finite value. Such a continuity will, of course, be demonstrable only if the absolute and differential thresholds are measured under conditions which are identical in every respect, except that in the one case γ_1 is zero and in the other it is not.

We may classify the conditions under which threshold experiments can be carried out under four headings: (a) two different stimuli, both of values greater than zero, may be presented simultaneously or successively against a uniform background (itself either of zero or of greater intensity); (b) a stimulus of value greater than zero and an equal period of no stimulation (conveniently expressed as 'a zero stimulus') may be presented against such a uniform background; (c) a stimulus may be presented once or several times against the background of another stimulus; and (d) a stimulus may be similarly presented against a background of zero stimulation. We are calling (a) and (c) measurements of the 'differential threshold' and (b) and (d) measurements of the 'absolute threshold.' In maintaining that thresholds measured under condition (a) are continuous with those measured under condition (b), and that (c) thresholds are continuous with (d), we are only in verbal disagreement with Koffka[1] who maintains that

*Accepted for publication November 18, 1929.

[1]K. Koffka, Perception: An introduction to the Gestalt-Theorie, *Psychol. Bull.*, 19, 1922, 555.

the absolute and differential thresholds are different phenomena—for Koffka applies the term 'absolute threshold' to (c) and (d) and differential threshold to (a). We do not dispute his contention that (c) and (d) are essentially different measurements from (a). We are only concerned with the proposition that (a) is not an essentially different measurement from (b) or (c) from (d).

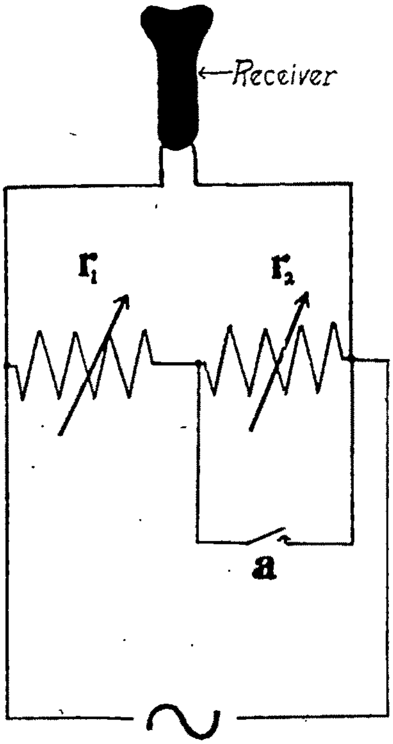

FIG. 1. WIRING DIAGRAM OF THE APPARATUS

It might be supposed that continuity between the absolute and differential thresholds as we have defined them is so obvious as not to be worth demonstrating. Fechner's deduction of Fechner's law depends,[2] however, essentially on the assumption that, when $\gamma_1 = 0$, $\Delta\gamma/\gamma_2$ is unity (or $\Delta\gamma/\gamma_1$ is infinite) and that it then changes abruptly to the value demanded by Weber's law when γ_1 is given the absolute threshold value; and William James states explicitly in his discussion of Fechner's law that this is the case.[3] We find, on the contrary, that there is a continuous fall in the value of $\Delta\gamma/\gamma_2$ from unity when $\gamma_1 = 0$,[4] and that there is continuity in the values of $\Delta\gamma$ itself when these are plotted against γ.

METHOD

The object of making our measurements of the differential and absolute thresholds under strictly equivalent conditions necessitated a departure from the standard methods of threshold

[2] G. T. Fechner, *Elemente der Psychophysik*, 1860.

[3] W. James, *Principles of Psychology*, 1890, 538.

[4] Our curves show $\Delta\gamma/\gamma_2$, since it is more convenient to show a curve falling from unity than from infinity, as would the curve for $\Delta\gamma/\gamma_1$. It is difficult to say which of these ratios is correctly called Weber's ratio, since Weber used sometimes one and sometimes the other.

measurement. On a background of a more intense stimulus, γ_2, we presented several times a less intense stimulus, γ_1. The *S* either perceived the stimulus changes or he had an unchanged experience of uniform auditory stimulation. In Koffka's terminology, he reacted either with a 'plane-experience' or a 'stepwise-experience.' His instructions were to report every time he had the 'stepwise-experience.' So long as he had the experience of uniform stimulation, he made no report.

The stimulus was a note of pitch 724 d.v., produced in a 'loud-speaker' attachment to a gramophone by means of an alternating current from an oscillating vacuum-tube circuit. The intensity of the sound was regulated by means of two inductionless resistences, r_1 and r_2, which short-circuited the receiver (Fig. 1). The difference between the intensities of γ_1 and γ_2 was made by short-circuiting r_2 to produce γ_1. Thus, for γ_2, the short-circuiting resistance was r_1+r_2, while for γ_1 it was r_1 only.

A slight change in the period of oscillation of the vacuum tube is caused by variation in the total external resistance of the circuit. In order to minimize this variation in external resistance, an additional resistance of 9000 ohms (not shown in Fig. 1) was placed in series with the oscillating circuit. This was found to be sufficient to make variations of pitch with alteration of r_1+r_2 immeasurably small.

In order to get a relative measure of the sound intensities for different values of the shorting resistance, the strength of the alternating current through the receiver with large values of r_1+r_2 was measured by means of a thermo-element and a sensitive galvanometer. From these values, the current strengths were calculated when r_1+r_2 was small. These currents are expressed in the diagrams as the strength in millamps of equivalent direct currents. Such figures are taken to be proportional to the energy of the stimulus striking the *S*'s ear, since no direct measure of this energy is possible (and for the purpose of the present experiment would have had no particular value). *S*'s head was kept at a constant distance from the open face of the gramophone (150 cm.). It was demonstrated that within the limits of error of our measurement there was no day to day variation of the current strength.

Two methods of experimenting were used, and the results of them must be kept distinct since (as would be expected) they gave considerable differences in threshold measurement.

In Method A, the short-circuiting was performed by the contacts of a Wundt time-machine. Two contacts were made, each lasting 0.08 sec. and separated by 0.2 sec. After 4.7 sec., another similar double contact was made. During the next interval of 4.7 sec., the value of r_2 was changed, and two similar double contacts were made with the new value. Thus γ_2 was

sounding the whole time and two double presentations of each value of γ_1 were made. *S* reported every time he had the step-wise experience.

This was later replaced by a more satisfactory method of presentation, Method B, in which the periods of γ_1 and γ_2 were more nearly equal so that a finer threshold was obtained. The short-circuiting was by means of a metronome making contacts for 0.66 sec. separated by intervals of 0.72 sec. Five contacts were made for each value of r_2.

The stimulus-patterns for these methods of presentation are shown in Fig. 2.

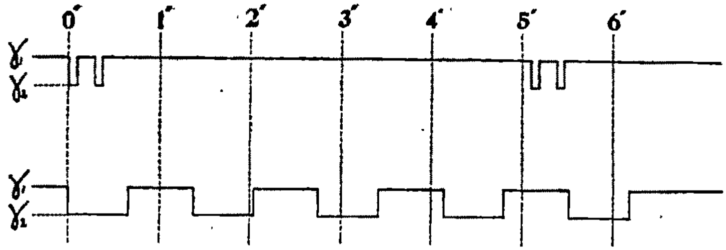

FIG. 2. STIMULUS PATTERNS
Top: Method A; bottom: Method B.

Each observation was started with r_2 at such a low value that *S* reported no step-wise experience. Then r_2 was raised by successive steps until several positive responses had been given. The stimuli were then lowered by the same steps until there had been several failures of response. $\Delta\gamma$ was taken as the mean between the first positive response as r_2 was increased and the first failure to respond as r_2 was decreased.

Generally r_1 was started at zero so that the first $\gamma_1 = 0$ and the first γ_2 was the absolute threshold.[5] The second value of r_1 was equal to the previous value of r_2, so that the second γ_1 was the same as the first γ_2. Similarly, each successive r_1 was greater than the previous r_1 by the previous threshold value of r_2. Each γ_1 was equal to the previous threshold value of γ_2 (or to $\gamma_1 + \Delta\gamma$). Thus $\Delta\gamma$ was measured for successive values of γ separated by just noticeable intervals. In order to show more clearly the con-

[5]As the result of an error this method was not exactly followed for the data shown in the upper curve of Fig. 3. Here the initial value of γ_1 was not 0 but a very small positive amount.

tinuity of $\Delta\gamma$ at low values of γ, measurements were also made of $\Delta\gamma$ at successive values of γ, beginning at o and separated by much less than the value of the absolute threshold.

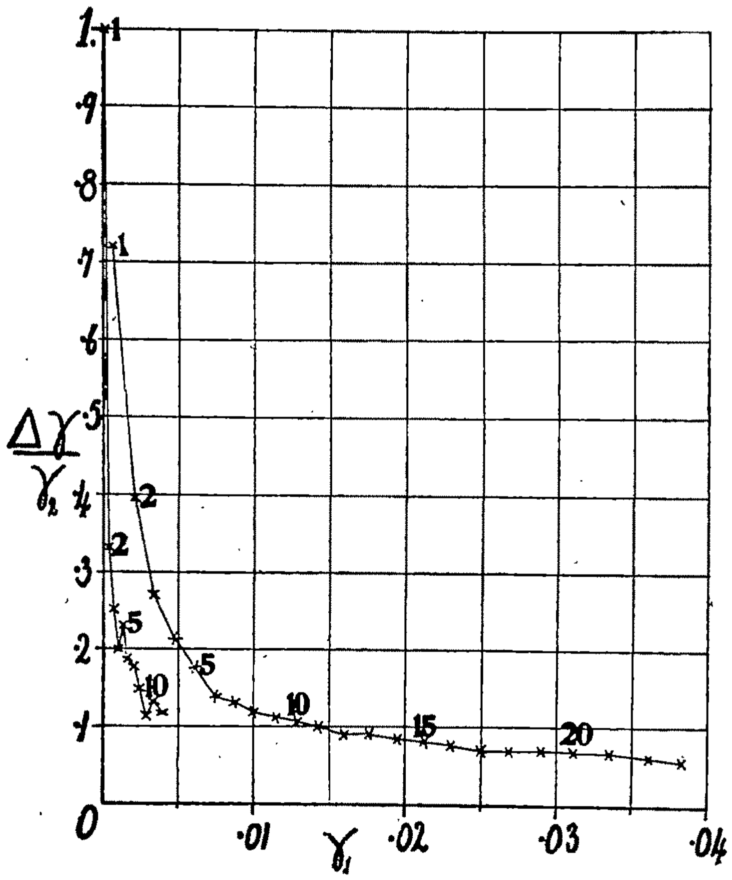

FIG. 3. CURVE OF VARIATION OF WEBER'S RATIO WITH STIMULUS STRENGTH
Upper curve: mean of six determinations by Method A. Lower curve: single determination by Method B. Subject S. Figures on curves indicate successive j.n.d. of stimulation.

No sound-proof room was available for this experiment, so that most of the observations were necessarily made against a variable background of street noises. This was less serious than it would have been for other kinds of work upon the auditory threshold, since our object was the limited one of demonstrating continuity of threshold for different values of γ. While external noises undoubtedly produced irregularities in our curves and a general coarsening of

the threshold,[6] they could not simulate a continuity in our measurements where none would otherwise have existed. The effect of the noises was also rendered less important by the fact that our stimuli were notes of a single definite pitch. We considered it advisable, however, to confirm our results by experiments performed between 12 midnight and 3 A. M., when the street sounds were silenced. All the curves shown as obtained with the subject T were from a set of observations taken under conditions of complete silence at night.

Results

Fig. 3 shows two curves of $\Delta\gamma/\gamma_2$ plotted against γ_1, the upper one the mean of 6 determinations by Method A starting with 0.00061, the lower one a single determination by Method B, both

TABLE I
DATA FOR SUBJECT S AND METHOD A
(Mean of 6)

γ_1	γ_2	$\Delta\gamma$	$\Delta\gamma/\gamma_2$
.00061	.00217	.00156	.720
.00215	.00355	.00139	.393
.00355	.00484	.00129	.266
.00480	.00613	.00133	.216
.00613	.00748	.00135	.180
.00747	.00869	.00122	.140
.00869	.01004	.00135	.135
.0100	.0114	.00145	.120
.0114	.0129	.0015	.112
.0129	.0144	.0015	.106
.0144	.0160	.0016	.101
.0160	.0176	.0016	.090
.0176	.0194	.0018	.093
.0194	.0212	.0018	.083
.0212	.0230	.0019	.081
.0230	.0250	.0020	.080
.0250	.0269	.0019	.072
.0269	.0290	.00205	.071
.0290	.0312	.0022	.071
.0312	.0336	.0023	.070
.0336	.0360	.0024	.068
.0360	.0384	.0024	.062
.0383	.0407	.0024	.059
.0562	.0593	.0031	.052*
.0996	.1035	.0039	.038*
.1857	.1921	.0064	.033*
.2608	.2678	.0070	.026*

*Higher values of γ not shown on graph.

[6]E. G. Wever and S. R. Truman, The course of the auditory threshold in the presence of a tonal background, *J. Exper. Psychol*. 11, 1928, 98-112.

using subject S. The full series from which the upper curve is plotted is also shown in the accompanying table (including values for higher intensities of γ not shown in the graph). In each case it is seen that $\Delta\gamma/\gamma$ falls on a continuous curve dipping sharply at first and later approximating to, but not reaching, the condition demanded by Weber's law (a straight line parallel to the base).

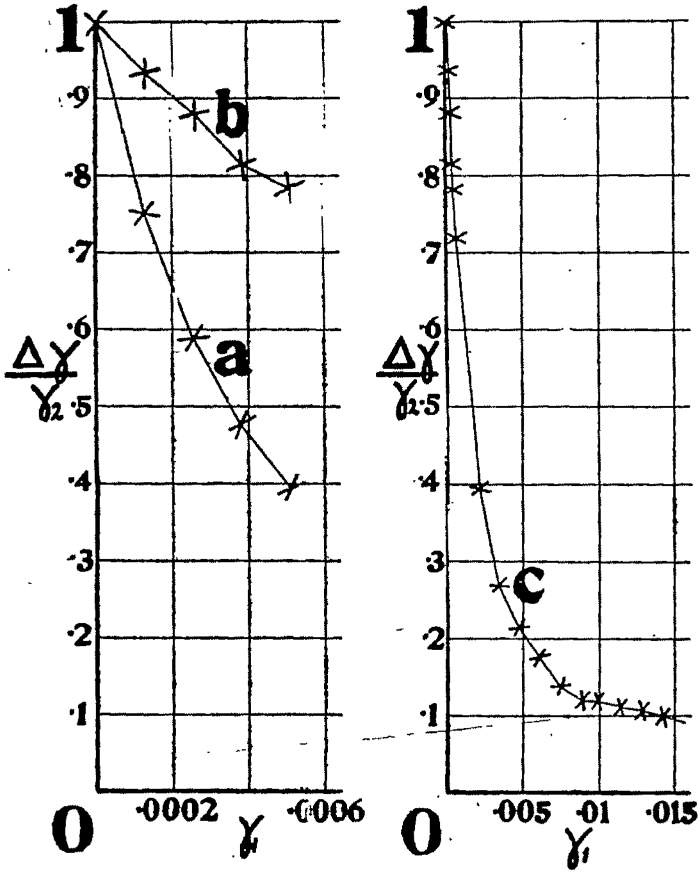

FIG. 4. CURVES OF VARIATION OF WEBER'S RATIO WITH STIMULUS STRENGTH
(a) With the smaller stimulus below absolute threshold value: Subject T, Method A.
(b) With the smaller stimulus below absolute threshold value: Subject S, Method A.
(c) 4b redrawn on same scale as Fig. 3 and joined to upper curve of Fig. 3.

Figs. 4a and 4b also show the change of $\Delta\gamma/\gamma_2$ (on a larger horizontal scale) but for a set of very low values of γ_1, all less than

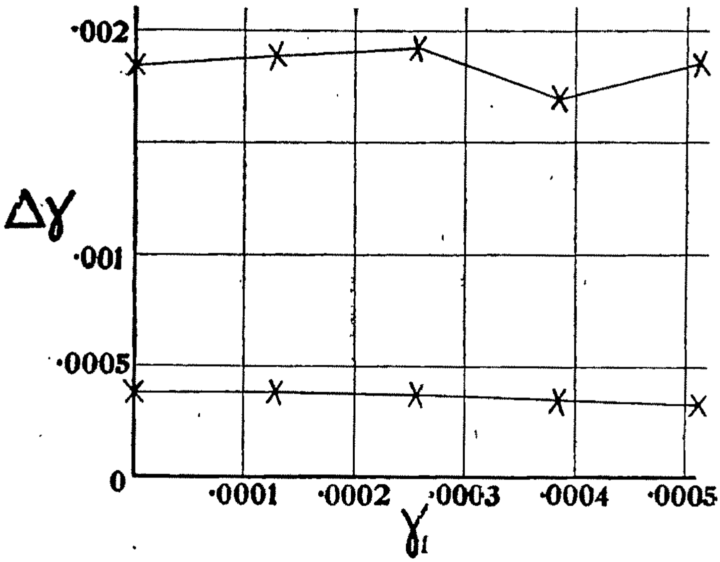

FIG. 5. CURVES OF VARIATION OF JUST PERCEPTIBLE STIMULUS INCREMENT WITH STIMULUS STRENGTH

The smaller stimuli are below the absolute threshold value and successive stimuli are separated by less than perceptible increments. Upper curve: Subject S, Method A. Lower curve: Subject T, Method B.

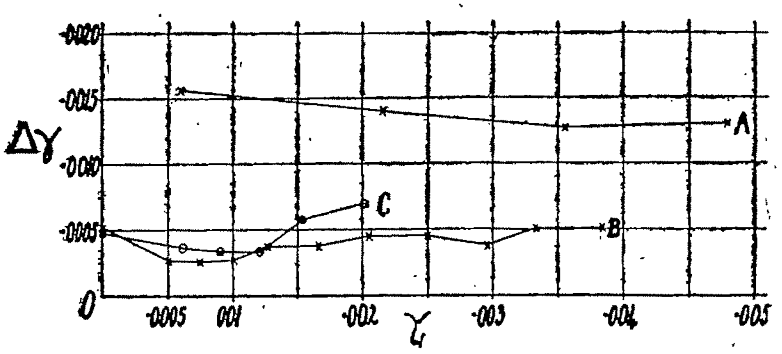

FIG. 6. CURVES OF VARIATION OF JUST PERCEPTIBLE STIMULUS INCREMENT WITH STIMULUS STRENGTH

Successive stimuli are separated by just perceptible increments. A: Subject S, Method A. B: Subject S, Method B. C: Subject T, Method B.

the absolute threshold value, beginning at $\gamma_1 = 0$. Again continuity is seen. To show this continuity more clearly in Fig. 4c, 4b has been drawn to the same scale as the upper curve in Fig. 3 and joined on to it (both curves having been obtained by the same method with the same *S*).

The above results with subliminal values of γ_1 suggested that $\Delta\gamma$ itself was a measure following simpler laws than $\Delta\gamma/\gamma_2$. Thus in Fig. 5 we have plotted the same experiment at results as are shown in Figs. 4a and 4b, this time as absolute values of $\Delta\gamma$

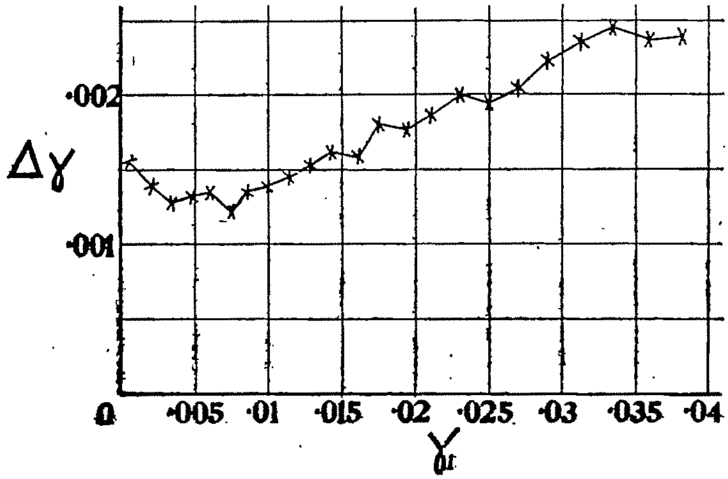

FIG. 7. DATA OF UPPER CURVE OF FIG. 3 SHOWN AS CURVE OF VARIATION OF JUST PERCEPTIBLE STIMULUS INCREMENT WITH STIMULUS STRENGTH

against γ_1. The points prove to lie on a line almost parallel with the base, showing that for very small values of γ_1, $\Delta\gamma$ is almost a constant (*i.e.* is independent of γ).

The curves in Fig. 5 show, however, a slight tendency to fall, so in Fig. 6 we show, on a smaller horizontal scale, $\Delta\gamma$ plotted against γ_1 for the values of γ_1 separated by just noticeable stimulus-differences. This figure shows that the initial tendency of $\Delta\gamma$ to fall is real. By Weber's law, $\Delta\gamma$ should rise uniformly with γ. Before such a rise begins to take place we see that there is a range of values of γ over which $\Delta\gamma$ falls with increasing γ.

A longer curve of $\Delta\gamma$ is shown in Fig. 7. This is the whole curve of which Fig. 6a is the first part, drawn to a still smaller horizontal scale. It utilizes the same readings as the upper curve of Fig. 3.

In all of the curves of Figs. 5, 6, and 7, it is seen that there is no evidence of $\Delta\gamma$ falling on a different curve when $\gamma_1 = 0$ from the curve connecting $\Delta\gamma$ with values of γ greater than 0. The absolute threshold proves to be continuous with the first differential thresholds.

Summary

(1) The absolute and differential auditory thresholds prove to be continuous with each other. There is no sudden emergence of a new law when the value of the absolute threshold of the stimulus is passed.

(2) Weber's law is very far from being fulfilled for low stimulus values. A much nearer approximation to the truth is that, for low stimulus values, the absolute value of $\Delta\gamma$ is nearly constant. This statement is not, however, exactly true. The absolute value of $\Delta\gamma$ seems to fall at first, then to rise on a curve which progressively approximates to the straight line inclined to the base, the function demanded by Weber's law.

SOME QUANTITATIVE EXPERIMENTS WITH EIDETIC IMAGERY

By J. A. GENGERELLI, University of California at Los Angeles

The experiments here reported are exploratory in nature and were carried out in the writer's home during the months of July and August of 1928 and August of 1929. The two *Os* were girls of the ages of 15 and 11 years, respectively, and sisters to the writer who has himself very little visual imagery.

The experiments were inspired by a conversational inquiry into the imagery of the two girls. They both gave evidence of being very strong visual types, and were amazed at the writer's confessed inability to imagine "anything he wanted to." When they were asked whether they "saw" things inside of their head, or out in space, they both replied that they "could 'see' them any place they wanted to." By request, the older of the two, *A*, proceeded to project the image of a brilliant flower upon the topmost branches of a tree some hundred yards distant. The flower itself was about five feet distant. She reported that the image thus projected did not lose any of its structural or color details, and that it retained its natural size. The younger girl, *B*, repeated the same feat, although she experienced more difficulty in doing so. Many other informal attempts were made in this way with natural objects, the *Os* reporting uniform success in each case. They regarded the task as having nothing particularly unusual about it and repeatedly expressed astonishment at the fact that everyone should not possess this capacity of projecting a perceived object into space at will.

Experiment I. Seven cards, 10 cm. sq., were cut out of stiff white cardboard and a circle drawn on each in pencil by means of a compass. The radii of the circles varied from 27 mm to 15 mm., inclusive. The circles were drawn in accordance with a successively diminishing scale, such that the radius of the largest circle (no. 1) was 2 mm. greater than that of the next largest (no. 2); the radius of circle no. 2 was 2 mm. greater than that of circle no. 3, and so on down to circle no. 7 which was the smallest, having a radius of 15 mm. For the sake of clarity, we list below the numbers of the circles together with their respective diameters in mm.

No. 1	No. 2	No. 3	No. 4	No. 5	No. 6	No. 7
54	50	46	42	38	34	30

On another piece of cardboard, 10 cm. × 15 cm., was drawn a square (in India ink) 42 mm. on the side. This, which we shall call the criterion, was placed vertically at varying distances from *O* and at a level with her eyes. The criterion was exposed at all times during the course of the experiment. *O* was seated at a table, and the seven cards thoroughly shuffled. The cards were then placed in a pile on the table at *O*'s right, face downward. The instructions were as follows:

> When I say 'Up' I want you to pick up with your right hand one of the cards in the pile and look at it for 5 sec. At the end of this time I shall say 'Down,' and after you hand the card to me, I want you to project the circle you just saw onto the square which is tacked up here in front of you, and tell me whether it is too big, too small, or just fits. All you have to say is too small, too big, or just right.

By reading the number of the circle written in small characters on the back of each, *E*, upon receiving the card, recorded the judgment on a blank specially prepared for the purpose. When the pile was exhausted, the cards were shuffled again, and the procedure repeated. The period required in shuffling the cards served as a rest interval, during which *O* was urged to keep her eyes off the criterion before her and let them wander about the room.

It is clear that in the above set-up a maximal amount of eidetic endowment would enable the *O*, practically, to give all correct judgments. Consequently, we should expect that circle No. 4, having its diameter equal to the length of the sides of the criterion (the square), would invariably be indicated as fitting the square; the smaller circles as being too small; the larger circles as too large. A lower degree of endowment would scatter the judgments accordingly.

The results of Experiment I are given in Table I. The distance of projec-

TABLE I
RESULTS OF EXPERIMENT I
(Distance of projection 80 cm.)

O	Judgments	Circle Nos. 1	2	3	4	5	6	7
	larger	10	10	10	0	0	0	0
A	smaller	0	0	0	0	10	10	10
	equal	0	0	0	10	0	0	0
	larger	7	0	0	0	0	0	0
B	smaller	0	1	5	10	10	10	10
	equal	3	9	5	0	0	0	0

tion was 80 cm. Ten judgments were obtained for each circle. The table is in terms of the absolute number and kind of judgments given to each.

Two very interesting facts stand out in the above results. First, is the precision of *A*'s judgments; and secondly, the fact that *A*, who is the older by four years, seems to possess a much more highly developed eidetic proclivity than *B*. The work of Jaensch would seem to indicate that eidetic capacity diminishes with age. The facts in the present case, however, seem to point to an original endowment on the part of *A* so superior to that of *B* that at the age of 15 years the one gives a better performance than the other at the age of 11 years.

Introspections. Both *O*s reported that the projected images were in the form of shadow-like circles which were fairly distinct. Although, of course, the two *O*s were tested separately, and communication with each other regarding the nature of the experiment was forbidden, they both reported that the images, "once you got them on the card, were plain and steady." *A*, in particular, reported that she lost the "circle" if she raised her eyes and looked about the room immediately after the fixation-period. She found it always necessary

to project the image immediately onto the criterion if she was to give a judgment with any degree of certainty; although, once she had projected the image on the cardboard she could hold it there indefinitely. This was likewise true of *B* to a lesser degree.

On being questioned as to how they made their judgments, the *Os* reported that they "put the circle on the square, and if the sides of the circle 'stick out' of the sides of the square, the circle is too big; if the circle doesn't reach the sides of the square, it is too small. It's just like placing a real circle on top of a square."

Both *Os* complained of burning and smarting in the eyes after a series of judgments (one judgment for each of the seven cards), particularly toward the end of the sitting.

Experiment II. Wishing to test the effect of distance of projection upon the distribution of the judgments, the card bearing the criterion was removed to a distance of 365 cm. from the *O*. All the other factors remained the same. The results are shown in Table II.

TABLE II
RESULTS OF EXPERIMENT II
(Distance of projection, 365 cm.)

O	Judgments	Circle Nos. 1	2	3	4	5	6	7
	larger	10	10	0	0	0	0	0
A	smaller	0	0	0	10	10	10	10
	equal	0	0	10	0	0	0	0
	larger	7	0	0	0	0	0	0
B	smaller	0	0	10	10	10	10	10
	equal	3	10	0	0	0	0	0

When we compare the above results with those in Table I, it is seen that the maximum of "fit" judgments goes towards the left, *i.e.* in the region of the larger circles. *A*'s record again shows a surprising consistency.

This would seem to indicate that, in the two cases here considered, we have decrease in the size of the eidetic image as the distance of projection is increased. It is difficult to see what other interpretation may be placed on the above facts. In all the present experiments the comparison circles were fixated at ordinary reading distance. This result is similar to Klüver's who used the following distances of projection:—25 cm., 100 cm., and 150 cm.[1] Of the 18 *Os* used, 12 showed a diminution in the size of the EI with increase in distance of projection. This was obtained with exposures of 10 sec.

Our own quantitative results were supplemented by the following rough observations. *O* was given one of the cards and told to look at the circle for the usual period, *i.e.* 5 sec *E* then took a large piece of white card-board and stood at a distance of about 1 m. from the *O* who was told to project the image of the circle on the card-board. *E*, still holding up the card-board, then gradually moved backward away from the *O*. At a distance of about 250 cm. he

[1] Heinrich Klüver, An experimental study of eidetic type, Genetic Psychol. Monog., I, (no. 2), 101-2.

stopped and asked O whether the image had increased or diminished in size. The distance was then again increased by about 1 m. and another judgment obtained. The Os reported in nearly every case that the image seemed to decrease in size. There were a few judgments to the effect that the image retained its original size; but there were no judgments indicating an increase.

Experiment III. In view of the fact that A gave such remarkably precise judgments it was decided to construct a new set of circles having a smaller interval between them. We therefore drew a set of seven circles (in India ink) having a difference in radius of 1 mm. Circle no. 1 now had a radius of 24 mm., Circle no. 2, 23 mm., no. 3, 22 mm., and so on. The standard for comparison was a *circle* having a radius of 21 mm. The procedure was in all respects the same as in the preceding experiments, the O being asked to project each circle on the standard for comparison and to determine whether it was too large, too small, or equal.[1]

A gave 8 judgments for every card. B, on the other hand, because of scatter, gave 50. These were equally divided over a period of 5 days. The results are shown in Table III.

TABLE III
RESULTS OF EXPERIMENT III
(Distance of projection 80 cm.)

O	Judgments	Circle Nos. 1	2	3	4	5	6	7
A	larger	8	8	8	0	1	0	0
	smaller	0	0	0	0	7	8	8
	equal	0	0	0	8	0	0	0
B	larger	39	15	6	2	0	0	0
	smaller	0	4	13	32	46	50	50
	equal	11	31	31	16	4	0	0

The precision of A's judgments is again in evidence. This was, in fact, the reason for not having this O give a greater number of judgments. She found the task so easy that she complained of the monotony of the experiment.

It will be seen that the distribution of the 'larger' and 'smaller' judgments on the part of B conform roughly to the ogive curve, the latter more so than the former. The data are certainly not amenable to quantitative treatment, but give an indication of what may be done with a more adequate set-up.

Experiment IV. In this experiment, the step interval between the circles was made still smaller—the radii of the 7 stimulus-circles varied by steps of 0.5 mm. The circles were carefully drawn on cards 18 cm. sq. There were 3 copies of each of the 7 circles, so that the Os worked with a pack of 21 cards. The standard for comparison—a circle—had a radius of 26.5 mm. This was placed at a distance of about 60 cm. from the O. The standard could be seen only during the period in which the judgment was being made.

[1]Upon seeing the new set of circles, A, who is the more expressive of the two, exclaimed: "Why they're all alike! I never will be able to tell the difference."

After *O* had gone through the pack of cards and given a judgment on each, she was given a rest period of a few minutes, and the cards were again thoroughly shuffled. Fifteen judgments were obtained for every stimulus-circle. The experiments were all performed at one sitting. The results are shown in Table IV.

TABLE IV
RESULTS OF EXPERIMENT IV
(Distance of projection 60 cm.)

O	Judgments	Circle Nos. 1	2	3	4	5	6	7
A	larger	14	11	9	2	0	0	0
	smaller	0	1	1	4	6	12	15
	equal	1	3	5	9	9	3	0
B	larger	4	5	6	0	4	1	0
	smaller	2	1	6	10	7	9	12
	equal	9	9	3	5	4	5	3
J	larger	5	0	1	0	0	0	0
	smaller	0	0	3	9	12	12	15
	equal	10	15	11	6	3	3	0

Here again is evidenced the decided superiority of *A*, although there is a great deal more scatter in judgments than in Table III. The judgments, however, still fail to conform, except roughly, to an ogive distribution.

B's results show that the task was too difficult for her eidetic equipment.

J, a brother of *A* and *B*, is totally devoid of eidetic imagery. He had not been available prior to the above experiment. He is 20 years of age.

Introspections. The introspective information given by *A* again attests to her strong eidetic endowment. Several times she pointed out in her eidetic images of the circles certain small details, such as very slight blurs in the periphery, which were present in the original. She also reported quite often that when the image of the comparison circle fitted the criterion, the periphery of the criterion circle seemed a bit darker and thicker. Another observation which was volunteered was to the effect that she experienced a tendency to superimpose the image of the criterion circle upon the comparison circle as she looked at the latter for the alloted period of 5 sec.

The introspective reports secured from *B* throw some light on the rather erratic scattering of her judgments as given in the tables. She reported time and again that she had but slight *control* over her projected images, and that as often as not they "slipped off" the standard for comparison. An objective corroboration of this is the length of her reaction-times. *B*'s reaction-times were much longer than *A*'s. The strained and intent expression on her face likewise indicated that she was having some difficulty in making her judgments. Several times she reported seeing several circles at once.

J's introspections were quite meager. There were, of course, no eidetic images present. Upon being asked how he formed his judgments, he could give no description. He merely stated that "somehow or other the circles looked bigger, smaller, or about the same size."

Remarks. While the lack of time, apparatus and laboratory conveniences made it difficult to pursue the above experiments with any degree of constancy or refinement, we feel that certain methods of attack are suggested which may prove fruitful in future quantitative investigations of eidetic endowment, and perhaps of visual imagery in general. Certainly there appear to be no *apriori* considerations that would render the method of constant stimuli, so eminently successful in experiments with lifted-weights, of no avail in investigations of eidetic imagery. Such a method, furthermore, would place research in this field on a quantitative basis such as has not been attained heretofore.

THE CORRELATION CORRECTED FOR ATTENUATION IN ONE VARIABLE AND ITS STANDARD ERROR

By Jack W. Dunlap and Edward E. Cureton, Territorial Normal and Training School, Honolulu, Hawaii

The validity of a test is estimated by correlating it against an outside criterion. Since neither the test nor the criterion will ordinarily have a perfect reliability, this correlation will suffer from attenuation in both variables. The validity of the test should properly decrease with its reliability, for if the validity coefficient were corrected for attenuation in the test, this corrected correlation with the criterion might easily exceed the square root of the reliability coefficient, which is theoretically impossible. On the other hand, the validity of a test has nothing to do with the reliability of the available criterion scores, and the validity coefficient should be corrected for attenuation in the criterion. This paper considers formulas for such a correlation between two variables corrected for attenuation in one of them; selects the most practical of them, and presents a derivation of its standard error.

Given a test y_2 and a criterion x_∞ measured by fallible scores x_1 and x_3 with errors e_1 and e_3. Then

$$x_1 = x_\infty + e_1$$

$$x_3 = x_\infty + e_3$$

Assume that the errors are uncorrelated with each other and with the true scores. Then

$$\Sigma x_1 y_2 = \Sigma(x_\infty + e_1)y_2 = \Sigma x_\infty y_2$$

$$\Sigma x_3 y_2 = \Sigma(x_\infty + e_3)y_2 = \Sigma x_\infty y_2$$

$$\Sigma x_1 x_3 = \Sigma(x_\infty + e_1)\ (x_\infty + e_3) = \Sigma x_\infty^2$$

and

$$\sigma_\infty^2 = \frac{\Sigma x_\infty^2}{N} = \frac{\Sigma x_1 x_3}{N} = r_{13}\,\sigma_1\,\sigma_3$$

so that

$$r_{\infty 2} = \frac{\Sigma x_\infty y_2}{N\sigma_\infty\sigma_2} = \frac{\Sigma x_1 y_2}{N\sigma_\infty\sigma_2} = \frac{\Sigma x_3 y_2}{N\sigma_\infty\sigma_2}$$

Each of the last two numerators differs from $\Sigma x_\infty y_2$ by chance only, so the best estimate of $\Sigma x_\infty y_2$ should be their arithmetic mean, whence, substituting also the value of σ_∞^2,

$$r_{\infty 2} = \frac{r_{12}\sigma_1\sigma_2 + r_{23}\sigma_2\sigma_3}{2N\,\sigma_2\sqrt{r_{13}\sigma_1\sigma_3}} \qquad \text{[1]}$$

The value of $r_{\infty 2}$ could be calculated from formula [1], but the computation

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would be rather long and the standard error of this formula would be difficult to determine, and lengthy. If we assume $\sigma_1 = \sigma_3$, it reduces to

$$r_{\infty 2} = \frac{r_{12} + r_{23}}{2\sqrt{r_{13}}} \qquad [2]$$

If σ_1 does not equal σ_3 exactly, $\sqrt{\sigma_1 \sigma_3}$ is their geometric mean, and we substitute this value for σ_1 and then for σ_3 in the numerator. Since the geometric mean is slightly smaller than the arithmetic mean, this substitution will cause formula [2] to underestimate the true value of $r_{\infty 2}$ slightly, but in most cases this systematic error is small in comparison with the sampling errors.

It is not necessary to compute both the intercorrelations r_{12} and r_{23}. Instead, we may compute $r_{2(1+3)}$, the correlation between the test scores and the sums of the pairs of corresponding criterion scores.

$$r_{2(1+3)} = \frac{\Sigma y_2\,(x_1 + x_3)}{N\,\sigma_2\,\sigma_{(1+3)}} = \frac{r_{12}\,\sigma_1\,\sigma_2 + r_{23}\,\sigma_2\,\sigma_3}{\sigma_2\sqrt{\sigma_1^2 + 2r_{13}\,\sigma_1\,\sigma_3 + \sigma_3^2}}$$

Assuming as before that $\sigma_1 = \sigma_3$,

$$r_{2(1+3)} = \frac{r_{12} + r_{23}}{\sqrt{2 + 2r_{13}}} \qquad [3]$$

Then

$$r_{12} + r_{23} = r_{2(1+3)}\sqrt{2 + 2r_{13}} \qquad [4]$$

Substituting in [2]

$$r_{\infty 2} = \frac{r_{2(1+3)}}{\sqrt{\dfrac{2\,r_{13}}{1 + r_{13}}}} \qquad [5][1]$$

Before formula [5] can be used for practical purposes, its standard error must be known. It should be noted that the standard error as derived here applies equally well to formula [2], for $r_{2(1+3)}$ may always be computed by formula [3]. For convenience, we shall call $r_{2(1+3)} = r$, so that

$$r_{\infty 2} = \frac{r\sqrt{1 + r_{13}}}{\sqrt{2\,r_{13}}} \qquad [6]$$

Taking logarithmic differentials, summing, squaring and dividing by the population,

$$\frac{\sigma^2 r_{\infty 2}}{r_{\infty 2}^2} = \frac{\sigma^2_r}{r^2} + \frac{\sigma^2 r_{13}}{4(1 + r_{13})^2} + \frac{\sigma^2 r_{13}}{4\,r_{13}^2} + \frac{2r\, r_{rr_{13}}\,\sigma_r\,\sigma_{r_{13}}}{2r(1 + r_{13})} - \frac{2r\, r_{rr_{13}}\,\sigma_{r_{13}}}{2r\,r_{13}}$$
$$- \frac{2\,\sigma^2 r_{13}}{4r_{13}(1+r_{13})} \qquad [7]$$

To evaluate this expression, we require the values of r, r_{13}, σ_r and $\sigma_{r_{13}}$, which may be determined directly from the data. In addition, we need the value of $r_{rr_{13}}$. From Filon and Pearson's formula for the correlation between correlation coefficients,[2] substituting the subscripts 1, 3, 2 and (1 + 3) for 1, 2, 3 and 4 respectively, we have

$$r_{r_{13}r_{2(1+3)}} = \frac{1}{(1 - r_{13}^2)\,(1 - r^2_{2(1+3)})}\left[r_{1(1+3)}\,r_{23} + r_{12}\,(r_{3(1+3)} - r_{23}\,r_{2(1+3)})\right.$$

[1]This formula has been given without formal proof by C. L. Hull in his *Aptitude Testing*, 1928, 244, formula 6.

[2]Cf. T. L. Kelley, *Statistical Method*, 1923, 179, formula 128.

$$- r_{13}\, r_{1(1+3)}) - r_{3(1+3)}\, (r_{1(1+3)}\, r_{2(1+3)} + r_{13}\, r_{23})$$
$$+ \frac{r_{13}\, r_{2(1+3)}}{2}\, (r_{12}^2 + r^2_{1(1+3)}\, r_{23}^2 + r^2_{3(1+3)})] \ldots\ldots [8]$$

To evaluate this expression, we need in addition to the r_{13} and $r_{2(1+3)}$ calculated from the data, the values of $r_{1(1+3)}$, $r_{3(1+3)}$, r_{12} and r_{23}.

$$r_{1(1+3)} = \frac{\Sigma\, x_1\, (x_1 + x_3)}{N\, \sigma_1\, \sigma_{(1+3)}} = \frac{\sigma_1^2 + r_{13}\, \sigma_1\, \sigma_3}{\sigma_1 \sqrt{\sigma_1^2 + 2r_{13}\, \sigma_1\, \sigma_3 + \sigma_3^2}}$$

Assuming as before that $\sigma_1 = \sigma_3$

$$r_{1(1+3)} = \sqrt{\frac{1 + r_{13}}{2}} = r_{3(1+3)} \ldots\ldots [9]$$

Assuming that $r_{12} = r_{23}$, we have from [4],

$$r_{12} = \frac{r_{2(1+3)}\sqrt{2 + 2r_{13}}}{2} = r_{23} \ldots\ldots [10]$$

If r_{12} does not equal r_{23} exactly, the second expression of [10] is their arithmetic mean. Remembering that $r_{1(1+3)} = r_{3(1+3)}$, it may be seen that r_{12} and r_{23} enter equally and additively in [8], except in the case of the second term of the first parenthesis, $r_{12}\, r_{23}\, r_{3(1+3)}$, in which both enter as factors. The only systematic error introduced by the assumption will be a negligible increase in the value of this term due to substituting for the product $r_{12}\, r_{23}$ the square of the arithmetic mean of these factors.

Substituting the values from [9] and [10] in [8] and simplifying, we have

$$r_{r_{12} r_{2(1+3)}} = r_{rr_{13}} = \frac{r}{2} \ldots\ldots [11]$$

Substituting the values of σ_r, $\sigma_{r_{13}}$ and $r_{rr_{13}}$ in [7] and simplifying, we have

$$\sigma_{r_{\infty 2}} = \frac{r_{\infty 2}}{\sqrt{2N}} \left[\frac{2(1 - r^2)^2}{r^2} + \frac{(1 - r_{13})^2}{2\, r_{13}^2} - \frac{(1 - r^2)\,(1 - r_{13})}{r_{13}} \right]^{\frac{1}{2}} \ldots\ldots [12]$$

An example of the use of formulas [5] and [12] might be the determining of the validity of a test of some character trait by its correlation with the judgements of friends of the members of the criterion group. The uncorrected validity coefficient would be the correlation between the pooled judgments of all the judges and the test scores; the reliability of the criterion, the correlation between the pooled judgments of half the judges and those of the other half. Suppose that the former correlation is 0.30 and the latter 0.50. Then from [5], $r_{\infty 2} = 0.367$, and from [12], if $N = 50$, $\sigma_{r_{\infty 2}} = 0.024$.

It should be noted that the denominator of [5] is the Spearman-Brown formula for the estimated reliability of a test twice as long as the given ones from which the reliability coefficient was computed, and that the values in parentheses in [12], are quite commonly used functions of the correlation coefficient. Values of all these functions are available in various published tables, whose use should lighten the labor of computation considerably.

OBSERVATIONS ON THE TEMPORAL JUDGMENT DURING SLEEP

By EDWARD N. BRUSH, University of Maine

Studies of the temporal judgment have been almost wholly confined to the analysis of relatively short intervals; the least perceptible interval, the largest noticeable interval or maximum conscious present, and the indifference point for the perception of intervals. A study of the temporal judgment involving relatively long intervals of time has been reported by L. D. and E. G. Boring.[1] Their *Os* were awakened at prearranged times during the night, asked to estimate the time, and to report as far as possible the cues upon which the judgment was based. They found that the time could be estimated with a degree of accuracy that is one half of that which chance guesses would have given under the conditions of the experiment. They also found that the *Os*' account of the cues indicated that they were an adequate basis for the judgment. The most frequent and useful cues were those dependent upon the general bodily state. There was comparatively little dependence on external criteria.

The ability to awaken at given times in the morning might give another approach to the problem. The response to cues, external or internal, under a determination, which seems to be the general nature of this phenomenon, is probably the same sort of thing as that involved in certain types of post-hypnotic phenomena. Some understanding of it might be gained by observing the conditions under which it works.

With this general problem in mind, the writer attempted a rough preliminary study, using himself as *O*.[2] The work was necessarily limited by the demands of ordinary living and of other work, and no apparatus was used to obtain highly accurate measurements. Most of the conditions considered were rated on scales, and, while these ratings were made on the basis of definite criteria, they are not precise. The number of cases varied, and is often far too few, because some of the conditions could not be controlled at will. No very extended mathematical treatment need therefore be attempted because of the general nature of the data. If other *Os* had been used, important individual differences might have appeared. The results should be taken cautiously and as merely suggestive.

The procedure was to record, on retiring, the approximate time of going to sleep, the time chosen for awakening, and ratings on such factors as general physical condition, degree of mental alertness previous to retiring, motivation (taking into account the relative desirability or necessity of awakening at the given time), and the amount of sleep on the previous night. Then the sugges-

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[1] L. D. and E. G. Boring, Temporal judgments after sleep, *Studies in Psychology, Titchener Commemorative Volume*, 1917, 255-279.

[2] This paper is from the Psychological Laboratory of the Boston Psychopathic Hospital. The writer is indebted to Professor E. G. Boring for helpful suggestions and criticisms.

tion for awakening was given by repeating ten times subvocally: "Waken me up at x o'clock." The time of awakening was taken as the time when consciousness was regained sufficiently to look at the clock, and it was recorded to the nearest minute. Other conditions recorded were: illumination in the room, dreams (whether remembered, vague, vivid, or disturbing), general physical condition during the first half-hour after awakening (in general, rated on the basis of feelings of fatigue or restedness), and the 'purity' of the determination (*i.e.* the amount of subsequent mental activity before dropping off to sleep). Fifty records were obtained, complete except in the case of items added after the beginning of the experiment

The observations were made during two periods, the first during March, 1926, in Cambridge, Mass., and the second during July and August, 1928, at the Boston Psychopathic Hospital. The records are, within these two periods, chiefly for consecutive days, but as only complete records are included, there are exceptions in several cases where something had interfered. An important difference in the conditions at these two places of residence lay in the matter of illumination, for the room at the hospital was much the darker. This difference was taken care of, as far as possible, by special care in rating this condition. In other respects the general conditions were comparable.

Previous to the experiment the writer had employed this method of awakening at times for a year or more. It is quite possible, therefore, that an unpracticed *O* might have some initial difficulty in awakening in this way.

One might expect a difficulty to arise when awakening occurred much in advance of the time set. This occurrence, however, did not happen during these observations, except for relatively short intervals of time, which were then recorded as *minus* errors. The oversleepings were rated as *plus* errors.

In order to rule out the factors of habit and the utilization of constant unrecognized cues in awakening, the times of retiring and awakening were varied as much as practical considerations permitted. The table shows the distribution of times set to awaken. The times of retiring were similarly distributed from 9:30 P. M. to 2:00 A. M. The interval between retiring-time and time-set-to-awaken was varied from 4½ to 9½ hrs., as may be seen from the table. Further variation was obtained by different combinations of retiring and awakening times, so that the same length intervals at the same absolute times were seldom repeated. Actually, two such intervals were used three times each, six twice each, and the remaining thirty-two were all different, if half-hour intervals are used as a basis of classification.

The chief results are presented in the table. The 'mean error of estimate' in awakening as compared with the predetermined time, the standard deviation, the probable error, and the constant error (indicating a constant tendency to awaken before or after the predetermined time) are all expressed in terms of minutes. At the bottom of the table these values are given for all the data, without regard to the special conditions. It will be noted that the mean error of estimate is 10.6 min., with a standard deviation of 10.0; in other words, given the suggestion to awaken at a given time, in approximately 68% of the cases the awakening will actually occur within 0.6 to 20.6 min. of that time.

Unfortunately there is no direct evidence available as to what the waking times would have been without this special determination. Evidence, however,

TABLE I

SHOWING THE CONDITIONS OF THE EXPERIMENTS, THE NUMBER OF EXPERIMENTS, THE MEAN ERROR OF ESTIMATE, THE STANDARD DEVIATION, THE PROBABLE ERROR, AND THE CONSTANT ERROR

(Number of Experiments 50)

Conditions	N	M.E. of E.	S.D.	P.E.	C.E.
(1) Time Set to Awaken					
5:30– 6:00	[illegible]	25.0	——	——	——
6:00– 6:30	7	11.0	5.8	——	+ 0.7
6:30– 7:00	[illegible]	11.6	8.1	——	+ 2.7
7:00– 7:30	9	14.3	16.4	——	+10.7
7:30– 8:00	12	8.0	9.0	——	+ 2.0
8:00– 8:30	7	11.3	13.5	——	+ 1.3
8:30– 9:00	[illegible]	2.0	——	——	——
9:30–10:00	2	3.0	——	——	——
(2) Hours Allowed Before Awakening Time					
4½–6	3	8.0	5.7	——	− 6.0
6 –6½	3	13.2	10.9	——	+12.8
6½–7	4	10.2	11.1	——	− 2.5
7 –7½	[illegible]	9.9	5.8	——	+ 5.2
7½–8	13	10.1	13.5	——	+ 3.7
8 –8½	13	9.6	8.2	——	+ 3.7
8½–9½	4	15.0	11.4	——	+ 2.5
(3) Hours of Sleep Previous Night					
6 – 7	2	15.0	——	——	——
7 – 7½	6	12.2	10.1	——	+ 0.5
7½– 8	13	5.0	7.1	——	+ 2.6
8 – 8½	9	15.6	14.6	——	+ 5.1
8½– 9	10	8.9	6.4	——	+ 3.5
9 – 9½	4	14.7	7.3	——	+ 2.0
9½–10	6	12.3	11.3	——	− 0.7
(4) General Physical Condition on Awakening					
Below par	9	13.4	9.8	——	+ 8.2
Good	41	9.7	9.6	±1.0	+ 2.0
(5) Bladder Condition on Awakening					
No noticeable pressure	21	11.6	10.9	±2.1	+ 5.5
Slight pressure	25	9.8	9.2	±1.21	+ 3.2
Marked pressure	4	9.8	10.6	——	− 9.8
(6) 'Purity' of Determination					
Little or no subsequent activity	11	5.2	4.5	——	− 1.5
Moderate amount of " "	9	16.2	9.1	——	+ 0.4
Much " "	4	18.5	8.1	——	+ 7.5
(7) Dreams					
None remembered	2	2.0	——	——	——
Vague	27	8.5	8.4	±1.1	+ 1.4
Vivid or disturbing	21	14.2	10.6	±1.2	+ 6.4
(8) Character of sleep					
Sound	40	8.9	10.4	±1.1	+ 0.9
Restless	10	13.6	9.4	——	+ 9.6
(9) Illumination of Room on Awakening					
Dark or dull	19	13.7	11.0	——	+ 4.8
Medium	17	11.3	8.5	——	+ 4.8
Light	14	5.6	6.4	——	+ 1.6
(10) Motivation					
Merely for experiment	9	16.6	11.2	——	+ 5.7
Otherwise desirable	13	10.2	9.5	——	+ 2.9
Otherwise very necessary	3	10.3	13.2	——	+ 9.0
AVERAGE		10.6	10.01	±0.97	+ 3.5

that the determination was operating appears indirectly in several ways. Section (1) of the table suggests no real difference in the accuracy of awakening for any particular period of the morning. The interval, 7:30 to 8:00, is a possible exception, but the somewhat lower value there is probably not significant in view of the variability. Section (2) of the table shows no significant differences according to the interval of sleep elapsing between retiring and awakening. These results seem to rule out to a considerable extent the possibility that the awakenings were due to a habit, either to awaken at a particular time or after a certain interval of sleep. Further, the records show that the average amount of sleep during the experiment was eight hours, which has been approximately the *O*'s habitual amount for several years. The mean difference between the actual time of awakening and a time of eight hours after retiring is 48.0 min., with a large S.D. (42.1), and a P.E. of ±4.0 min. The mean time of actual awakenings was 7:19. The mean difference between this time and the time of actual awakening was 39.5 min., S.D. 27.8, and P.E. ±2.7. Both of these are very significantly larger than the error in awakening as compared with the predetermined time.

The relation between the various other factors recorded and the accuracy of the time-estimate may best be seen by inspection of the table. An optimal amount of sleep on the previous night is suggested. There is possibly a tendency for more accurate estimates when the general physical condition is good on awakening. Bladder condition does not seem to be important, although, when there was marked pressure, there was a constant tendency to awaken before the time set. The amount of mental activity taking place after the suggestion was given seems to be rather definitely related to accuracy in awakening. With little or no subsequent activity the average error is least, and the constant error is very slightly in the direction of awakening too soon, while with much subsequent activity the tendency is to oversleep. Mental activity during sleep seems important also, and there is greater accuracy in awakening when dreams were not vivid or disturbing. Sound sleep seems to be more favorable than restlessness. Light in the room seems important, probably as a cue under the determination. Somewhat less accurate estimates appear when the time was set merely for the purposes of the experiment than when it was desirable or necessary to awaken from a practical point of view. Other factors recorded showed no particularly suggestive relationships.

Conclusions. It is possible, for the writer at least, to 'judge' time with considerable accuracy during a period of sleep. The 'judgment' appeared as awakening under a determination. The average actual time of awakening was far closer to the time set in the experiment than to any time, absolute or relative, that one might have expected for the awakening on the basis of habit.

Certain conditions appear as favorable or unfavorable to the operation of the determination. The more important of these were general physical condition, amount and character of sleep, mental activity subsequent to the setting up of the determination, both before and after going to sleep, illumination in the room on awakening, and motivation.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

LVIII. Reaction-time, Flicker, and Affective Sensitiveness as Tests of Extraversion and Introversion

By M. F. Washburn, E. Hughes, C. Stewart, and G. Sligh

Reaction-time. In a recent number of this Journal[1] there were reported some experiments indicating shorter reaction-times to noise in extraverts, as compared with introverts. The average for introverts was 200σ; that for extraverts 169σ, a difference giving 97 chances out of 100 of a true difference greater than zero. This difference corresponds to the general conception of speed of movement as an extravert trait. The present study attempted among other aims to verify this result with other *O*s. The groups tested in the previous investigation were obtained by the use of the Marston questionary, an *O* being ranked as an extravert if the number of extravert answers was at least 1.5 times the number of introvert answers, and as an introvert if this ratio was reversed. In the present study both the Marston and the Freyd questionaries were used. As before, each *O* was judged by herself and by two of her friends; this time the introvert group was the highest quartile in total number of introvert answers from both questionaries combined, and the extravert group the lowest quartile. The questionaries were answered for 168 young women college students, and the two groups tested each consisted accordingly of 42 *O*s. As in the earlier study, the average of the first ten reaction-times was taken for each *O*, on the supposition that temperamental differences would be more apparent before the reaction process had become mechanized through practice. The average of these average reaction-times was for the introverts 167.4σ, for the extraverts 159.8σ; a negligible difference, giving only 72 chances out of 100 for a true difference greater than zero.

A significant difference in reaction-time between extraverts and introverts does not obtain for this second pair of groups.

Flicker. In the Marston questionary perseveration is regarded as an introvert trait, its absence being defined as ability to turn readily from one activity to another. In the testing of motormen in Boston, perseveration was measured by the rate of rotation of a black and white disk at which flicker disappeared.[2] We determined this rate for the members of our two groups and obtained a difference giving 93 chances out of 100 of a true difference greater than zero, but it was in the wrong direction, showing greater retinal inertia in the extraverts than in the introverts.

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*Accepted for publication January 30, 1930.

[1] M. F. Washburn, K. Keeler, K. B. New, and F. M. Parshall, Experiments on the relation of reaction-time, cube fluctuations, and mirror drawing to temperamental differences, *op. cit.*, 41, 1929, 112-116.

[2] W. V. Bingham, *Service Bull. Per. Res. Fed.*, 1929.

Affective sensitiveness. Since extraverts are supposed to be both more superficially emotional and more sensitive to the external environment than introverts, it seemed possible that there might appear a difference between the groups in what the senior author of this study has called affective sensitiveness. This term means the tendency to find simple sense material, such as colors or articulate sounds, highly pleasant or unpleasant. The material was 50 color combinations on a gray ground, each composed of two Bradley colors 3 cm. in area with a strip of ground 5 mm. wide between them, and 50 nonsense syllables each composed of an initial vowel and final consonant. The *Os* were asked to estimate the agreeableness or disagreeableness of these stimuli by using the numbers 1 to 7, 1 meaning very disagreeable, 2 moderately disagreeable, 3 slightly disagreeable, 4 indifferent, and 5, 6, and 7 increasing degrees of pleasantness. Affective sensitiveness in a given *O* was measured by the ratio of the total number of her extreme judgments, 1 and 7, to the total number of her 'indifference' judgments, 4.[3] The extraverts showed greater affective sensitiveness, their median ratio being 2.14, while that for the introverts was 1.04, a difference indicating 95 chances out of 100 that a true difference greater than zero exists. This test, which gave the most reliable difference, is the least objective of the three here investigated, since it depends on introspective judgments of *O*'s attitude. We may note how far it is from determining whether a given individual is an introvert or an extravert. Suppose we say that a person with affective sensitiveness above 1 is probably an extravert. In these experiments 81% of the extraverts had a ratio above 1, but so did 56.4% of the introverts.

These results do not encourage the belief that objective tests of extraversion-introversion will be found.

LIX. The Effects of the Direction of Initial Pathways on the Orientation of White Mice in a Maze

By M. F. Washburn and R. Ebersbach

Various experimenters, Hubbert and Lashley, Warden, and especially Dashiell, have observed that animals learning a maze quickly acquire an orientation in the general direction of the goal. The aim of the present study was to see how, if at all, this orientation would be affected by differences in the direction of the initial pathway. Two groups of seven mice each learned a maze whose true path consisted of six equal straight pathways. The maze was lighted from above the center and was under a dark canopy.[4] One group entered it through a path which led in the direction opposite to that of the goal; the other group entered it through an equally long path leading in the direction of the goal. One half of the culs-de-sac pointed in the direction of the goal; the other half in directions at right angles to this. We measured the

[3]Misses A. Naylor and H. Mallay did part of this testing. The extraverts numbered 37, the introverts 39.

[4]Food was not at the goal until the mouse reached there (see following study).

degree of an animal's orientation tendency by the number of times it entered culs-de-sac towards the goal, divided by the number of times it entered culs-de-sac leading to the right or left. The average of these ratios for the group entering the maze by the 'positive' initial path was 1.61; the range being from 0.92 to 3 and only one of the seven ratios being below 1. The average for the group entering the maze by the negative initial path was 1.10, with a range from 0.42 to 1.6; three of the seven ratios being below 1.

The direction of the initial pathway, if towards the goal, seems to produce an increased orientation towards the goal, so far as a judgment can be reached with such small group. The work is being continued.

LX. The Effect on Orientation in the Circular Maze of the Presence or Absence of Food at the Goal During Running

By M. F. Washburn, E. Jacobs, and M. Mackenzie

In all experiments with hunger as the drive in maze learning, so far as the senior author of this study is aware, food has been present in the feeding compartment during the running, so that the smell of it may have been a factor in the learning process, and especially in orientation towards the goal. Our investigation aims to find how orientation in the circular maze is affected when food, instead of being present at the goal throughout the running, is placed there when the mouse arrives.

The orienting tendency of a mouse was measured by the number of times it returned from one ring into the ring next further from the center. Two groups of seven mice each were run. With one group a dish of milk was present at the center of the maze throughout; with the other the dish was put in the center compartment after the mouse had reached that point. A little practice enabled us to do this without frightening the mouse out of the center.

For the group 'with food' the average number of reversals during the learning period was 7; for the group 'without food' it was 7. In order to see whether the presence of food at the goal influenced the orientation in the first run, the average number of reversals in first runs was found: it was for the group 'with food' 2.28; for the group 'without food' 2.14.

The presence of milk at the goal during maze running does not affect orientation measured in the manner here described. Food with stronger odor would probably give a different result.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

III. Sex Differences and the Method of Continuous Lists

By Elinor J. Nöh and J. P. Guilford

Four previous studies of sex differences in association have been made by means of the method of continuous lists. In 1891, Jastrow, at Wisconsin, laid down the procedure for this problem.[1] Twenty-five men and twenty-five women students were instructed to write one hundred words as rapidly as possible in order that the lists might be natural and unreflective. The experiment was twice repeated at Wellesley College, once by Nevers,[2] and again by Calkins.[3]

Manchester performed the experiment a fourth time at the University of California,[4] in 1905. She divided her data into three sets, each containing twenty-five men's lists and twenty-five women's, which provides three pairs of groups for comparison.

The earlier conclusions are not univocal, but certain sex differences in associations are indicated. The range of ideas is found to be greater in men than in women by all experimenters. From Jastrow's lists, women's traits are revealed as "an attention to immediate surroundings, to the finished product, to the ornamental, the individual, and the concrete, while the masculine preference is for the more remote, the constructive, the useful, the general, and the abstract."[5] Calkins considers the difference in mental community negligible and she also objects to the emphasis placed upon the greater proportion of abstract terms for men.[6] Manchester finds that reference to time, to the dynamic, and the abstract predominate in the men's lists; notions of the static, space, and the concrete in the women's.[7]

The present experiments were performed with the hope that any genuine sex-differences found in the earlier studies would be more fully verified by additional evidence and that some of the more uncertain differences would be cleared up to some extent.

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*Accepted for publication February 20, 1930.

[1]J. Jastrow, A study in mental statistics, *New Review*, 5, 1891, 559-568.

[2]C. C. Nevers, Dr. Jastrow on community of ideas of men and women, *Psychol. Rev.*, 2, 1895, 363-367.

[3]M. W. Calkins, Community of ideas of men and women, *ibid.*, 3, 1896, 426-430.

[4]S. G. Manchester, Experiments on the unreflective ideas of men and women *ibid.*, 12, 1905, 50-66.

[5]Jastrow, *op. cit.*, 564.

[6]Calkins, *op. cit.*, 428-429.

[7]Manchester, *op. cit.*, 66

Procedure. We followed Jastrow's procedure as closely as possible, and performed the experiment on two groups of *Ss*. The first group was composed of 20 men and 20 women, and the second group of 30 men and 30 women; all were selected from the general and advanced classes in psychology. The *Ss* were told to "write 100 different words as rapidly as possible," and that "the words should not form sentences."

The words were then tabulated under 25 headings: (1) *animal kingdom;* (2) *wearing apparel;* (3) *proper names;* (4) *verbs;* (5) *implements and utensils;* (6) *interior furnishings;* (7) *adjectives;* (8) *foods;* (9) *vegetable kingdom;* (10) *abstract terms;* (11) *buildings and building materials;* (12) *parts of the body;* (13) *miscellaneous;* (14) *geographical and landscape features;* (15) *mineral kingdom;* (16) *meteorological and astronomical;* (17) *stationery;* (18) *occupations and callings;* (19) *conveyances;* (20) *educational;* (21) *other parts of speech;* (22) *arts;* (23) *amusements;* (24) *mercantile terms;* and (25) *kinship*. This classification follows Jastrow's for purposes of comparison.

Before we discuss the results, a few definitions of terms are in order. By 'unique' words is meant those appearing only once in the entire number of lists, that is reported by only one person. Manchester refers to these as "unrepeated" words. Another term which we shall use, borrowed from Jastrow, is 'different' words. By this is meant the total number of different words, repeated and unrepeated, appearing in all the lists. Manchester again changes the term, this time to 'vocabulary.'

Results. Our results do not entirely corroborate the earlier ones. As for mental community, however, they do show a greater percentage of different words for men than for women.

Jastrow's percentages of 'different' words are 55.0 for the men and 44.9 for the women, a difference of 10.1%. Calkin's percentage for women is 52.25. In Manchester's three groups the percentages are: 58.0, 63.32, and 59.36 for the men, and 55.9, 60.36, and 56.28 for the women. The differences are smaller in these groups, and when the groups are combined the difference is even less—41.58% for men and 40.64% for women. This might indicate that sex differences in small groups are due to sampling. Our percentages are 58 and 51.2 for the men, and 50.4 and 47.5 for the women, differences of 7.6% and 3.7% respectively. We can find no reason for the disparity between these differences, except the fact that there were only 20 *Ss* of each sex in the first group and 30 of each sex in the second.

The percentages of 'unique' words for Jastrow was 30 for the men and 21 for the women. In Manchester's results the percentages of unique words, which we have computed from her data, are 40 and 38, 47 and 43, and 43 and 39 for the men and women respectively in her three pairs of groups. When the groups are combined this difference almost vanishes; the percentages are then 26.3 and 26. Again enlarging the groups of men and women decreases the sex difference in community. This may not be due to sampling alone. As more lists are added, more and more of the common vocabulary of both sexes is bound to be utilized. The percentages of different words and unique words decrease as the groups become larger. As the community becomes greater for both sexes, it tends to approach equality for both. Our percentages are 34 and 33.4 for the men, 31 and 30.6 for the women.

The differences are in the same direction, in favor of greater community for the women, in every pair of groups. Yet the differences are small, and we should pause to ask whether they are any greater than between groups of the same sex. In Manchester's groups the average difference between groups of the same sex is 3.21%, based upon the percentage of different words. The corresponding sex difference is only 3.26%. In our own groups the differences are 4.85% and 5.65% respectively, still not significantly different.

TABLE I

MANCHESTER'S TABLE WITH THE NEBRASKA RESULTS ADDED

(+ Men lead; − Women lead.)

Classificatory headings	Wis.	Calif. I	Calif. II	Calif. III	Calif. comb.	Nebr. I	Nebr. II	Nebr. comb.
Animal kingdom	+	−	+	+	+	+	−	−
Verbs	+	+	+	+	+	+	−	+
Proper names	+	−	−	+	−	+	+	+
Adjectives	+	−	−	−	−	−	+	+
Implements and utensils	+	+	+	+	+	+	+	+
Abstract terms	+	+	+	−	+	+	+	+
Wearing apparel, fabrics	−	−	−	−	−	−	−	−
Vegetable kingdom	+	−	+	−	−	+	−	−
Buildings, building materials	−	−	−	−	−	−	+	+
Parts of body	−	+	+	+	+	−	−	−
Geographical, landscape	+	+	−	−	−	−	−	−
Other parts of speech	+	−	+	+	+	−	+	+
Miscellaneous	−	+	+	+	+	+	−	+
Interior furnishings	−	−	−	−	−	−	−	−
Meteorological, astronomical	+	+	+	−	−	−	+	+
Mineral kingdom	−	+	+	+	+	+	−	−
Occupations	+	+	+	+	+	−	+	+
Conveyances	+	−	−	−	−	+	+	+
Stationery	−	+	+	−	+	−	−	−
Foods	−	−	+	+	+	−	−	−
Educational	−	−	−	−	−	+	−	−
Arts, sciences	−	−	−	−	−	−	−	−
Amusements	−	−	−	−	−	−	−	−
Mercantile terms	+	−	−	+	−	+	+	+
Kinship	−	+	−	−	−	−	−	−

But we must remember that the differences between the two sexes are all in the same direction, whereas the differences between groups of the same sex are both positive and negative, and that the six groups combined represent 150 men and 150 women under varying conditions of time and geographical location. The average difference between the six pairs of groups is 4.79, and this is

2.92 times its probable error. In our own groups, the sex difference in community as based upon the percent of *unique* words is five times as great as the difference between the groups of the same sex. By an accumulation of small but consistent differences, we may say that men at Wisconsin, California, and Nebraska have a slightly lower community-index than women in associations by the method of continuous lists. Whether these groups are representative of the two sexes at large we leave to the reader to judge.

As to the topics which men and women think about in this method of free association, we must turn to the classification of responses into the twenty-five classes. We have prepared a table summarizing the results from the three universities. The first column in the table gives the names of the classes. The remaining columns give plus signs when the men exceed the women in the number of words given in that class and a minus sign when the women lead in that class.

When Manchester compared her three groups with Jastrow's one she found an agreement of four out of four or three out of four in eighteen classes. By adding our figures to the others, the agreement in six out of six, or five out of six groups reduces the number of classes in which there is agreement to ten.

This might mean two things: (1) Sex differences decrease as the number of *S*s increases. In other words, as groups are added there is no tendency to find either sex so strongly attached to certain classes. The sex differences found in the early studies were due to errors of sampling. (2) Sex differences are becoming progressively less pronounced. Jastrow and Calkins performed their experiments in 1895, Manchester in 1905. Our work was carried on almost twenty-five years later. It might be inferred from the disagreement in our results with those of former experimenters and the disagreement in eleven out of twenty-five classes of our own two groups, that men and women are becoming more alike, that is, neither sex predominates consistently in any one class. This diminishing disparity between the sexes is also indicated by the facts given above; that the difference in community of ideas is decreasing. The fact that these groups come from different parts of the country, however, prevents any dogmatic conclusion.

By referring to Table I, we find the men lead most pronouncedly in *verbs*, *abstract terms*, *implements and utensils*, and *occupations*. We would agree with Manchester that the notion of action is prominent in those classes in which the men lead. Women lead in *wearing apparel*, *buildings and building material*, *interior furnishings*, *educational*, *arts*, *kinship* and *amusements*, in which no such notion is evident. Manchester also finds references to *time* to be indicated in the men's choices while references to *space* are more common in the women's lists. This distinction did not seem to be so clear in our results.

Manchester finds that women are able to write 100 words in somewhat less time than men. Jastrow gives merely the average time for men and women combined, which is 5 min. 8 sec.; the Wellesley time for women is 5 min. In the three California sets the time for men is 6 min. 2 sec., 6 min. 17 sec., and 5 min. 2.5 sec.; for women, 6 min., 5 min. 55 sec., 5 min. 2 sec. Our average time for the men was 5 min. 3 sec., and 5 min. 4 sec., and for the women, 5 min. 5 sec. and 4 min. 31 sec. The average difference is 21 sec., and although

the probable error of the difference cannot be obtained it must be small in view of the 150 cases of each sex.

This difference in the time of writing suggested another problem to us. What is the effect of the time period on the mental community shown in the lists? Will the *S* who associates rapidly have a higher community-index than the one who associates slowly? Is this possibly the reason that women have a higher community-index than men? We selected 5 papers from the men's group and 5 from the women's that were written in the shortest time and the same number written in the longest time and calculated the number of different words or the 'vocabulary' for these 20 lists. Of the 500 words in each of the four groups of papers the results are as follows: The 5 men who wrote their lists most slowly gave 419 different words, and the men who wrote most rapidly gave 393. The corresponding results for the women are 417 and 389 different words respectively. The difference in time between the men's and women's lists, then, might possibly have something to do with the greater number of different words in the men's lists. Why do the women write their lists in less time? Is it due to more rapid associations, greater speed of writing or to greater coöperation in fulfilling the instruction to "write rapidly?" Our method does not give the answer. The small sex differences in community might, however, entirely disappear if the writing-time of the two sexes were equated.

Conclusions. Our tests, agreeing with those of Jastrow and Manchester on some points, reveal the following:

(1) In unrestricted associations, that is associations made without the use of a stimulus word, the word reactions of men have a slightly greater range, that is less community, than those of women.

(2) In six groups of students out of six, or five out of six, at Wisconsin, California and Nebraska, men predominate in *verbs, implements and utensils, abstract terms,* and *occupations;* the women exceed in *wearing apparel, building and building materials, interior furnishings, education, art,* and *amusements.*

(3) There is some indication that the sexes are becoming more alike in the kind of free associations given.

(4) The notion of activity is more obvious in the men's lists; the static appeals more to the women.

(5) Women complete their lists in less time than men, on the average.

(6) Papers written in the shortest time, by both men and women, show more mental community than those written in the longest time. There may be some causal connection between this fact and the greater community of women in the method of continuous lists.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by JOHN PAUL NAFE

XXXI. THE RELATION BETWEEN AREA AND INTENSITY OF VISUAL THRESHOLDS

By C. H. GRAHAM

The recent work of Adrian and Matthews on the eel's eye brings out the fact that the literature on the problem of the relation between the light intensity of the threshold object and its area is of great importance in a theoretical consideration of visual processes.[1] One of the best pieces of experimentation done on this problem is that of Reeves.[2] It is of greater value than most of the other attempts because the areas used covered a comparatively large range (approximately 0.75–14,400 sq. mm.) As the data stand, however, there is no quantitative formulation expressing the relationship between area and brightness.

Since the thresholds were determined at varying distances from the subject it is necessary to find a common measure for the areas. This is done by using the retinal image as determined for each area at each distance for the reduced eye. Fig. 1 gives a graphic presentation of the relationship between the retinal area (in sq. mm.) and the brightness (in ml.) when expressed in a logarithmic curve. Table I gives the data from which this graph was taken. It is not likely that the cones entered into the determinations. A comparison of the intensity of an area used by Reeves with the closely corresponding area used by Hecht[3] shows that the equilibrium threshold of the former is about 1/30 that of the latter.

In the first part of his work, Reeves used a star of 1 mm. diameter. He does not give a detailed description of it, consequently it is impossible to know the exact area of the image, but it is assumed that it is slightly less than a circle of the same diameter. To arrive at an approximation of the area, r^2 was multiplied by 3 rather than by π. Under these conditions the relationships for the star are purely relative. The error, however, would have to be very large in order for the line joining the star determinations to coincide with the line for the squares.

*Accepted for publication November 22, 1929.

[1]E. D. Adrian and R. Matthews. The action of light on the eye. *J. Physiol.*, 63, 1927, 378-414; 64, 1927, 279-301.

[2]P. Reeves. The effect of size of stimulus and exposure-time on retinal threshold, *Astrophys. J.*, 47, 1918, 141-145.

[3]S. Hecht. The nature of foveal dark adaptation, *J. Gen. Physiol*, 4, 1921, 113-139.

The graph in Fig. 1 makes it apparent that the relation between intensity and area for the star and (within limits) for the squares can be described by an equation of the general type

$$y = kx^n.$$

Analysis of the data shows that the equation in both cases is

$$AI^{1.3} = K,$$

where A is the area of the retinal image in sq. mm., I is the intensity (or more accurately, the brightness) in ml., and K is a constant. The fourth column of

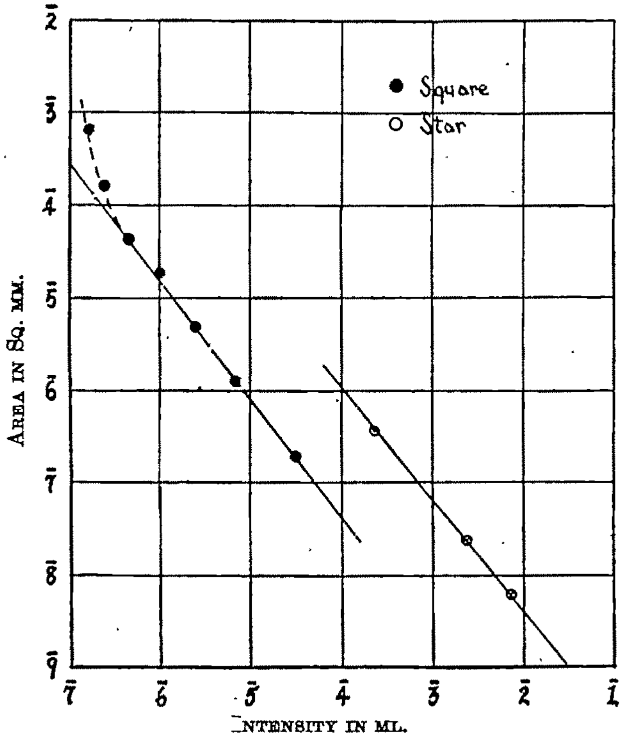

FIG. 1. RELATION BETWEEN AREA AND BRIGHTNESS OF RETINAL IMAGE OF THRESHOLD OBJECT

Table I gives the value of K (multiplied for convenience by 10^{14}) for each threshold determination at each area and corresponding brightness. It is noted that in the case of the squares K is relatively constant for the first five thresholds, but in the last two deviates considerably. This is shown in the graph by the broken line. Obviously, there is some influence entering as the areas get greater to break up the relationship. The constancy of K for the star is not very good. Perhaps a slightly better value of n could be found, but due to the inadequacy of Reeves' report with respect to the stars it is not considered worth while. It is obvious that the Ks are of the same order of magnitude.

TABLE I

RELATION BETWEEN THRESHOLD AREA AND INTENSITY

Object	Log Retinal Area (sq. mm.)	Log Threshold (mL.)	$K \times 10^{14}$
	$\bar{9}.7604$	$\bar{3}.8573$	944.0
Star	$\bar{8}.3617$	$\bar{3}.4150$	1000.0
	$\bar{7}.5911$	$\bar{4}.3802$	768.0
	$\bar{7}.3284$	$\bar{5}.4518$	26.0
	$\bar{6}.1139$	$\bar{6}.8215$	24.1
	$\bar{6}.7160$	$\bar{6}.3820$	25.7
Square	$\bar{5}.3010$	$\bar{5}.0086$	32.4
	$\bar{5}.6435$	$\bar{7}.6532$	24.2
	$\bar{4}.2455$	$\bar{7}.4116$	48.9
	$\bar{4}.8195$	$\bar{7}.2430$	109.0

The question as to the influence causing the deviation in the last two cases for the squares is of considerable importance. It is questionable whether it could be due to the cosine effect which for larger areas might cause a definite change in brightness.[4] With the artificial pupil Troland assumes a loss of 1% or more in brightness at angles greater than 8.1° This general condition would apply to the natural pupil to some extent. The last two areas subtended angles of approximately 9.5° and 19°. Certainly the cosine effect is to be expected, but the question must remain for explanation along with the other questions inherent in the problem of area and intensity.

[4] L. T. Troland. On the measurement of visual stimulation intensities, *J. Exper. Psychol.*, 2, 1917, 1-33.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXXVII. THE CRITICAL TEMPERATURES FOR HEAT AND FOR BURNING HEAT

By ELEANOR LOWENSTEIN and KARL M. DALLENBACH

The experience of heat is, as experiments have repeatedly shown,[1] normally aroused by temperatures adequate to the sensations of warmth and paradoxical cold. The temperature of 45°C. has, since Von Frey's experiments on paradoxical cold,[2] been almost universally accepted as the critical point for heat. Temperatures below that point (and above physiological zero) have been regarded as adequate only to warmth. Experimenters who have studied the synthetic experience of heat have, therefore, used temperatures that did not exceed this point.[3] They have all endeavored to arouse heat by temperatures that were separately inadequate to the experience.

In an experiment recently reported from this laboratory, Burnett and Dallenbach found that the temperature of 45°C. was not critical for all their *Os*.[4] One *O* (*H*) invariably reported an intense heat whenever the temperature of 43° was used, no matter what the temperature of the cold stimulus used in combination with it. It appeared from this result—which stood in contrast with the results of the other *Os*—that the temperature of 43° was, for *H* at least, adequate for heat. In order to test this explanation a supplementary

*Accepted for publication June 15, 1929.

[1]Cf. bibliography given by S. C. Ferrall and K. M. Dallenbach, this JOURNAL, 42, 1930, 72.

[2]M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. königl. sächs. Gesell. d. Wiss., math.-phys. Kl.*, 47, 1895, 172.

[3]The following experimenters have performed the synthetic experiment: Torsten Thunberg (*Upsala Läkaref. Förh.*, 1, 1896, 489 ff.); F. Cutolo (this JOURNAL, 29, 1918, 442-448); J. H. Alston (*ibid.*, 31, 1920, 303-312); N. C. Burnett and K. M. Dallenbach (*ibid.*, I. 38, 1927, 418-431; II. 40, 1928, 484-494); W. B. Gritman and K. M. Dallenbach (*ibid.*, 41, 1929, 460-464); and S. C. Ferrall and K. M. Dallenbach (*ibid.*, 42, 1930, 72-82). The temperatures that they used are given in the following table in degrees centigrade.

AUTHOR	TEMPERATURE	
	WARM	COLD
Thunberg	+44°	+24°
Cutolo	44°–45°	8°–14°
	43°–45°	5°–9°
Alston	43°–44°	20°–22°
Burnett and Dallenbach, I.	34°, 37°, 40°, 43°	2°, 10°, 20°, 30°
Burnett and Dallenbach, II.	34°, 37°, 40°, 43°	2°, 10°, 20°, 30°
Gritman and Dallenbach	34°, 37°, 40°, 43°	12°–30°
Ferrall and Dallenbach	40°–42.5°	9°–25°
	38°–44°	9°–25°

[4]Burnett and Dallenbach, The experience of heat, *ibid.*, 38, 1927, 424 f.

series of experiments was conducted in which the temperature of 43° was used alone. *H* was the only one of the three *O*s who reported heat in the supplementary experiments, and he reported it consistently in every experiment, thus definitely demonstrating that 43° was for him adequate to heat. Because of this result, Ferrall and Dallenbach, in an experiment subsequently undertaken, determined the heat-limens of their *O*s,[5] before beginning the main series of their experiments. They too found that individual differences existed: one of their *O*s had a limen of 43°, and the other two had limens of approximately 45°.

The results of these two studies indicate—as one might have guessed—that the critical temperature for heat varies among individuals. Beyond that mere fact, however, the results tell us nothing regarding the extent or frequency of the dispersion.

Object. Our object in the present study was (1) to determine for a large number of *O*s the values and the dispersion of the critical temperatures for heat; and (2), since opportunity offered, to determine for the same *O*s the values and the dispersion of the critical temperatures for burning or painful heat.

Apparatus. The temperature grill described by Burnett and Dallenbach was used in the present study.[6] One of the two systems of tubing forming the grill was, however, removed. The remaining system was connected by rubber tubing to a copper coil immersed in a water bath which was in turn connected to a faucet leading from the University water mains. The temperature of the water bath could be raised or lowered as desired by means of a Roux regulator. The stimulus-temperatures were controlled in two ways: gross changes were accomplished by varying the temperature of the water bath, and slight changes by altering the water's rate of flow through the grill system.

After the temperature was adjusted it remained constant to 1/10°. The stimulus-temperatures ranged, by steps of 1°, from 40°—51°; they were measured by means of a tenth-degree thermometer, the mercury bulb of which was supended in and bathed by the stream of water jetting from the outlet of the grill. Readings of the thermometer were taken with the aid of a magnifying glass.

Because of the delicacy of the adjustments and the difficulty of bringing the temperatures to the desired points, experiments were conducted when the thermometer readings varied from the desired points by 2/10°. For example, temperatures at and between 40.8° and 41.2° were regarded, for the purposes of this experiment, as being at 41°. Finer adjustments could have been made, but we felt that the results did not warrant the additional expenditure of time. The temperatures reported in this experiment have, therefore, an extreme error of 2/10°.

The temperature of the experimental room varied during the course of the experiments between 20.5° and 25.2° C., but it did not vary more than 0.3° in any single experimental hour.

Observers. One hundred *O*s, graduate and undergraduate students and members of the departmental staff, served in the experiments. They all worked without knowledge of the problem or method other than that given them in the instructions.

[5]Ferrall and Dallenbach, The analysis and synthesis of burning heat, *ibid.*, 42, 1930, 75.

[6]*Op. cit.*, 419.

Instructions. The following instructions were given to the *Os*.

"At the signal 'ready,' hold your forearm above the grill. At 'now,' place the volar surface upon the grill. Relax, and keep the pressure of your arm as constant as possible. At a second 'now,' remove your arm and describe your experience. Make your descriptions short and as simple as possible."

The terms 'heat' and 'burning' or 'painful heat' were not used in or even suggested by the instructions. They had to be supplied, when used, by the experience itself. The last sentence of the instructions was calculated, however, to force even the sophisticated *Os* into the use of these simple descriptive terms.

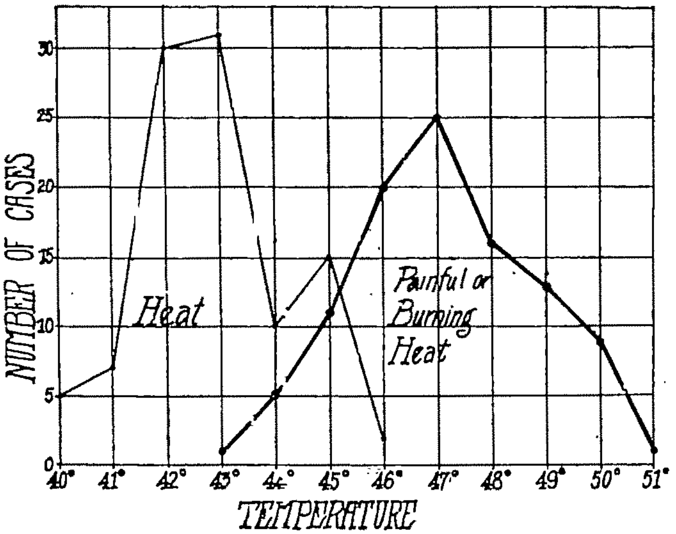

FIG. 1. FREQUENCY CURVES SHOWING THE NUMBER OF TIMES THE FIRST EXPERIENCES OF HEAT AND BURNING HEAT WERE AROUSED AT THE DIFFERENT STIMULUS-TEMPERATURES.

Procedure. Beginning with 40°, the stimulus-temperatures were applied in ascending order to the *Os*. Since it took longer to adjust the temperatures than it did to take the observations, the *Os* usually served in groups of 3 or 4. When the grill had been adjusted to the desired temperature, the *Os*, who had been waiting in an ante-room, were called one at a time. As soon as an *O* entered the experimental room he seated himself at the grill-table and, at the signal, placed the volar side of his right forearm upon the grill; five seconds later, at the second signal, he removed his arm and described his experience. The other *Os* were then called in one at a time. The apparatus was then adjusted to the next higher temperature and the procedure repeated, and so on, until the *Os* reported experiences that they described as "painful" or "burning heat." The experiments with a given *O* were interrupted as soon as a stimulus-temperature was reached that aroused 'painful' or 'burning' heat.

We did but one series of experiments with every *O* and determined his 'limens' for heat and burning heat from the results of single observations. This

procedure is open to obvious criticisms, but we had to choose between precise determinations for a few *Os* and less precise ones for a large number of *Os*. We chose the latter course as we were more interested in the present study in individual differences and in the range of temperatures that aroused heat and burning heat than in limens.[7]

Results. (1) With but 5 exceptions, all the *Os* reported 'warmth' when the lowest temperature (40° C.) was used. Then, as the stimulus-temperature was raised, they reported, in increasing numbers, the experience of heat. Finally, temperatures were reached at which they described their experiences as "burning" or "painfully hot."[8] In spite of the fact that they did not know how the stimulus-temperatures were being varied, not one of the *Os* reported warmth after the first report of heat. Once a temperature had been reached with a given *O* that aroused heat, all the temperatures above that point aroused heat for him.

(2) Table I shows the number of times that the first experience of heat and the first experience of burning heat were aroused at the different stimulus-temperatures. Five *Os* first reported heat at 40°, 7 at 41°, 30 at 42°, 31 at 43°, 10 at 44°, 15 at 45°, and 2 at 46°. A frequency curve, showing the distribution of these reports, is given in Fig. 1. The curve is bimodal: one mode is at 43°, and the other is at 45°—the temperature that has been, as we pointed out above, almost universally accepted as the critical point for heat. The average temperature at which the *Os* first reported heat is 42.87°±1.07° and the median is 42.74°±0.96°.

(3) The experience of burning or painful heat was reported, as Table I also shows, by 1 *O* at 43°, 5 at 44°, 11 at 45°, 20 at 46°, 25 at 47° 16 at 48°, 13 at 49°, 8 at 50°, and 1 at 51°. The distribution curve of the reports of burning heat, shown in Fig. 1, is unimodal, with the mode at 47°. The average temperature at which burning heat was first reported by the *Os* is 47.09°±1.29°, and the median is 47.04°±1.25°.

(4) Table I, which is constructed in the form of a scatter gram, shows, for every temperature at which the *Os* first reported heat, the different temperatures at which they first reported burning heat. For example, 5 *Os* reported heat at 40°; one of them reported burning heat when the stimulus-temperature reached 43°, one when it reached 46°, and the other two when it reached 48°. Again, of 2 *Os* who reported heat for the first time at 46°, one reported burning heat at 48° and the other at 50°.

Differences varying from 1°-8° occurred between the *Os*' 'limens' for heat and burning heat. The number of times that the various difference-magnitudes occurred is shown in Table II. A difference of 1° appeared with 3 *Os*, 2° with 13, 3° with 20, 4° with 22, 5° with 23, 6° with 8, 7° with 6, and 8° with 5. The

[7]The arguments advanced in favor of the Method of Single Stimulation are cogent here. Cf. E. B. Titchener, On ethnological tests of sensation and perception, *Proc. Amer. Philos. Soc.*, 55, 1916, 204-236; and J. N. Curtis, Tactual discrimination and susceptibility to the Müller-Lyer illusion, tested by the method of single stimulation, *Titchener Commemorative Volume*, 1917, 308-322.

[8]No attempt was made in this experiment to differentiate between burning and painful heat. They are, without prejudice, used interchangeably in this study.

TABLE I

SHOWING THE NUMBER OF TIMES THAT HEAT AND BURNING HEAT WERE FIRST EXPERIENCED AT THE DIFFERENT STIMULUS-TEMPERATURES, AND THE CORRELATION BETWEEN THE TEMPERATURES AROUSING THE TWO EXPERIENCES

	Degrees Centigrade	Temperatures arousing heat							Total
		40	41	42	43	44	45	46	
Temperatures arousing painful or burning heat	43	1							1
	44	1	1	2	1				5
	45		1	4	6				11
	46	1	3	7	6	1	2		20
	47			10	7	5	3		25
	48	2	1	2	6	1	3	1	16
	49		1	3	3	2	4		13
	50			2	2	1	2	1	8
	51						1		1
	Total	5	7	30	31	10	15	2	100

TABLE II

SHOWING THE DIFFERENCES BETWEEN THE LIMENS OF HEAT AND BURNING HEAT, AND THE FREQUENCY OF THEIR OCCURRENCE

	Difference							
	1°	2°	3°	4°	5°	6°	7°	8°
Frequency	3	13	20	22	23	8	6	5

average difference between the 'limens' for heat and burning heat is $4.22° \pm 1.35°$. The correlation between the 'limens' is, as computed by the product-moments method, 0.50,[9] P.E. 0.050.

(5) The wide dispersion of the differences between the limens, the large m.v. of the average difference, and the low correlation between the limens indicate, in view of Ferrall and Dallenbach's analysis of burning heat as an integration of heat and pain,[10] that the pain and heat limens do not vary concomitantly among *Os*. Indeed, results such as ours could be obtained only if the heat and pain limens varied independently, and the burning heat and pain limens varied directly—a conclusion which seems to follow from Ferrall and Dallenbach's results. For example, if we classify the differences between the limens of heat and burning heat, we find that 19% of the differences are large, 65% are intermediate, and 16% are small. This is approximately the distribution we should expect if pain is an integral quality of burning heat, and if the pain and heat limens vary independently. *Os* with a low heat limen and a high pain limen would give large differences between the heat and burning heat limens; *Os* with a low heat limen and a low pain limen, and those with a high heat limen and a high pain limen, would give intermediate differences; and *Os* with a high heat limen and a low pain limen would give small differences. A

[9]The coefficient was calculated by the formula: $r = \Sigma xy / n\sigma_1\sigma_2$.
[10]Ferrall and Dallenbach, *op. cit.*, 78, 81.

theoretical distribution of the differences would give large values in 25% of the cases, intermediate values in 50%, and small values in 25%. Whether the assumption—that the limens of pain and burning heat vary concomitantly—upon which this explanation is based is justified can only be determined by future experiments. From the facts on hand, however, the assumption seems to be reasonable.

(6) It becomes clear, in the light of the results of this experiment, why the earlier investigators of the problem of heat obtained discrepant results.[11] Since they did not determine their *Os'* limens for heat and burning heat, they did not know whether or not their warm stimuli were adequate to heat and pain. Identical stimulus-temperatures, as our results show, may arouse very different experiences in different *Os*. Consider, for example, the temperature of 44° (see Table I): 17 *Os* described the experience aroused by this temperature as "warm;" 78 described it as "hot;" and 5 described it as "burning hot." It is not safe, therefore, for an experimenter to assume, in lieu of positive knowledge, that all of his *Os* will respond to a given temperature in the same way. He should, before beginning his experiments, determine his *Os'* limens for heat and burning heat, under the precise conditions which he intends to employ later.

(7) What may we say, in conclusion, regarding the paradoxical sensation of cold? If we assume, with many writers,[12] that the point of heat and paradoxical cold are the same, then we may conclude that the critical point of paradoxical cold varied for our *Os* between 40°-46°, and that the average value was 42.87°±1.07. But is this assumption justified? Does it necessarily follow, simply because paradoxical stimulation is involved in the physiological integration of heat, that the variation and distribution of the limens of paradoxical cold and heat are identical? A stimulus which is effective when acting upon warm and cold spots may be ineffective when applied to a cold spot alone. Areal stimulation, moreover, may have a summative effect and arouse excitations not aroused by punctiform stimulation. We may consequently find that, when the cold spot is separately and punctiformly stimulated, the limen for paradoxical cold lies somewhat above that for heat.

Summary

One hundred *Os* were stimulated by temperatures varying, by steps of 1°, from 40°-51°, and were asked to describe the experiences aroused. Large individual differences were found to exist. Temperatures that were described by some as "warm," were described by others as "hot," and by still others as "burning hot." (1) But once heat was aroused with a given *O* all the stimulus-temperatures above that point also aroused heat. (2) The stimulus-temperatures that aroused heat varied from 40°-46° with an average of 42.87°±1.07°; and (3) those that aroused burning heat varied from 43°-51° with an average of 47.09°±1.29°. (4) Differences varying from 1°-8° occurred between the *Os'* limens for heat and burning heat with an average difference of 4.22°±1.35°. The result of this experiment (5) confirm the analysis of burning heat as an

[11] Cf. Ferrall and Dallenbach, *op. cit.*, 72 f.

[12] Pillsbury, for example, writes: "One obtains warmth alone up to 45°. Here the cold spots are excited in addition to the warmth, and the combination gives hot as opposed to luke warm" (*Fundamentals of Psychology*, 1922, 160).

integration of heat and pain. (6) show clearly why the earlier investigators of the problem of heat obtained discrepant results, and (7) raise the problem regarding the relation between the limens of heat and paradoxical cold.

LXXVIII. The Effect of Training Upon the Rate of Adult Reading

By Carlyle C. Ring and Madison Bentley

The visual 'perceptive span' for words and phrases was determined for 5 *Os* before and after a training period of nearly two weeks. The training consisted in reading as rapidly and as accurately as possible with governed fixations of the eye. After a preliminary test for span and speed, the training began. Vertical lines were drawn up and down upon a printed page dividing the lines according to the number of ocular fixations and flights which had previously been found to be normal to a given *O*. The *O* was then instructed to read the page (the *Forum* magazine), line by line, fixating only at the verticals. He read thus 15 min. morning and afternoon for two days. Then one vertical was omitted (the spacing between equally divided), and he read again—with one fixation less than the normal—for a week; then another fixation was dropped from each line. The amount read was recorded along with *O*'s comments upon the method and manner of reading.

A test of rate, accuracy, span, and number of fixations was made just before and just after this period of training. The fixations were observed by *E* with the aid of a mirror.[1] The material (1-6 short words, with a total of 6-34 letters and spaces) was presented for 0.5 sec. upon a cardboard slide in the Dodge tachistoscope. The amount read (including the errors and omissions) was determined by *O*'s oral recital. The perceptive span was calculated from all incomplete readings (a complete and perfect recital being regarded as less than a full 'span'). Both the minimal and the maximal (incomplete) readings were recorded in letters and in words. Fourteen slides made a *set* of exposures. Before training 5 sets were presented to each *O*, and after training 2 sets. The results follow.

	Before Training						After Training					
	Span				Av.		Span				Av.	
	Min.		Max.		per	Fix.	Min.		Max.		per	Fix.
Os	Wds	Let's	Wds	Let's	Line	Rate	Wds	Let's	Wds	Let's	Line	Rate
1	1	6	6	34	6	3.5	3	16	4	25	4	4.3
2	1	8	4	24	5	3.5	2	11	4	24	4	7.8
3	1	2	5	27	7	3.5	2	10	4	17	5	5.2
4	1	3	3	14	7	3.0	1	6	3	22	6	3.6
5	2	8	6	37	4	7.4	1	10	5	37	3	8.2
	Av.				5.8	4.5					4.4	5.8

*Accepted for publication November 14, 1929.

[1]Cf. C. T. Gray, *Deficiencies in Reading Ability*, 1917, 175.

Conclusions. By reading across the lines of the table *O* by *O*, it is apparent (1) that *rate of reading* is always *greater* after training than before (4.5 and 5.8), and (2) that the *average number of fixations* for a line read is less (5.8 and 4.4). As for (3) the *change of span*, the *minimal* amount read for all exposures increased for four of the five *Os*; but the *maximal* amount was the same or less for four *Os*. The *range* of the span therefore decreased under training; but the span itself cannot be said to have been greatly changed. The increase in facility under training seems therefore to rest chiefly upon a *smaller* number of fixations within the line and a *greater* number of fixations in unit time. The commentaries suggest that training also steadies the tempo of reading and decreases regressive movements toward the early part of the line.

PERCEPTION OF SILHOUETTES

By R. K. White and Carney Landis, Wesleyan University

The purpose of this experiment was to study the perception of silhouettes.[1] The following specific problems were involved: (1) What relations exist between the silhouetted features of a man's face and the judgment of observers as to his character? The question is not one of the validity of such judgments, but an investigation of the process of their formation. (2) What qualities of character are usually linked together in the judgments of an observer? (3) Are certain features of a silhouette perceived more clearly than others? If they are, how may we estimate the factors responsible for the difference in perception? (4) What are the least perceptible differences (LPD) of variation in the silhouettes among children and among adults?

Apparatus. Two silhouettes (Figs. 1 and 2) of the profile of a human face, cut from tin and painted black were mounted behind frosted glass in two similar square frames (25.4 x 25.4 cm).[2] The frames slid behind two square holes in a thin wall, one of which was above and to the right of the other, making direct comparison difficult. Both were illuminated from behind in such a way that only the silhouette shadow was seen. Fig. 1 represents the variable silhouette, which was usually placed above and to the right of the fixed silhouette, Fig. 2. (Fig. 1 is illustrated as seen from behind, with the nose drawn in; Fig. 2 represents the standard sillouette as seen by the observer.) In Fig. 1, the forehead, nose and chin could be altered either forward or backward. The variations were measured by scales (mm). When each feature was at normal, or zero, the two silhouettes were alike. (In Fig. 1 the forehead, F, and chin, C, are at normal, while the nose, N is drawn in by 15 mm., according to the scale). The forehead swings backward and forward from a pivot just over the nose; the nose swings in or out from the same pivot; and the lower lip, chin and neck swing together from a pivot just below the mouth. A difference of 10 mm. on the upper scale produces a difference of 8.5 mm. at the top of the head and 4 mm. at the middle of the forehead; a difference of 10 mm. on the middle scale produces a change of 4 mm. at the tip of the nose; and a difference of 10 mm. on the lower scale produces a difference of 1.5 mm. at the outermost point of the lower lip, 1.5 mm. at the tip of the chin, and 8.5 mm. at the bottom of the neck.

The subject sat in a chair about 3 m. from the silhouettes. Questions as outlined below were asked orally by the experimenter, who stood beside or behind the silhouettes and closed a shutter in front of them whenever an adjustment was to be made.

Method. The experiment was divided into two parts. In each part a group of 10 children (4-9 yrs. old) and 10 college students (17-22 yrs. old) served as *Ss*; the total number of *Ss* being forty.

*Accepted for publication January 9, 1930.

[1]The original idea of this experiment was obtained from the experiments of R. Arnheim (*Psychol. Forsch.*, 11, 1929, 1-132).

[2]The silhouette is one given in the 1803 edition of Lavater's *Physiognomy*.

Part I was exploratory. It included: (a) the determination of each *S's* LPD (least perceptible difference) for F, N and C separately, progressing from small to large differences, and taking the *first* correct comment on any feature as representing the LPD for that feature; (b) the presentation of certain definite

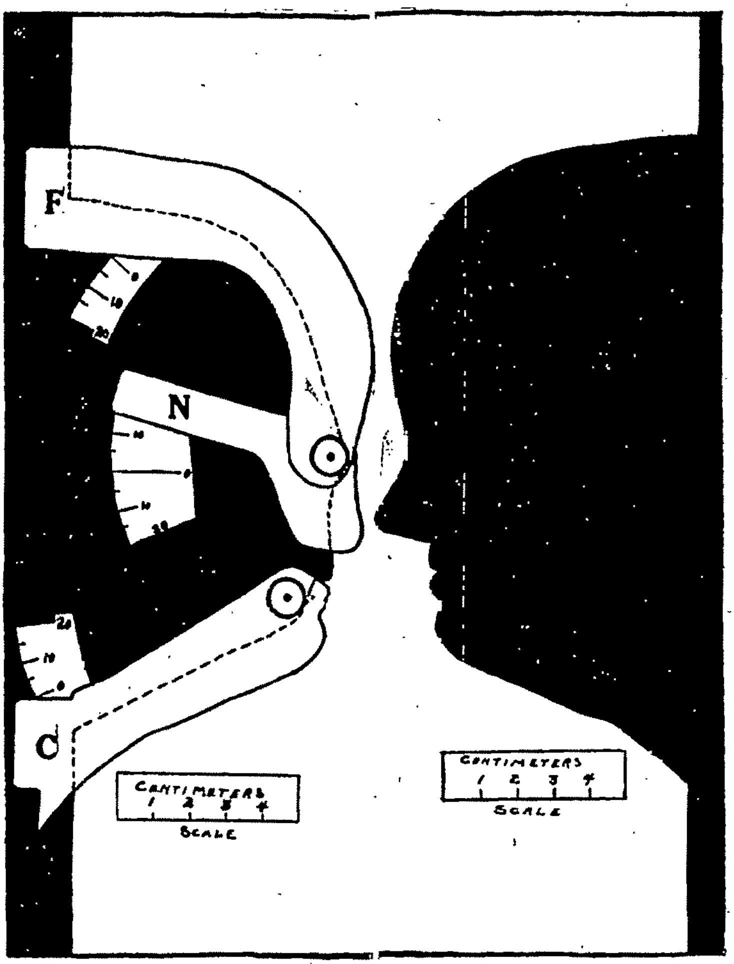

FIG. 1 FIG. 2

FIGS. 1 AND 2. SHOWING THE VARIABLE AND STANDARD SILHOUETTES

adjustments, of a fairly obvious nature, in some of which two features were changed at the same time. The usual procedure for both (a) and (b) was to ask, "Do you see any difference between these two pictures?" (if so) "What is it?"; but in many cases these questions were followed by a much more detailed

inquiry. (c) In this part of the investigation, the lower silhouette was set with the forehead bulging slightly, as compared with the upper, and 50 questions were asked as to the 'character' of the two men represented. The procedure here was to ask, "Does either of these men look more intelligent?"; "More hard working?"; "More athletic?" etc. It was omitted with the children, because of their limited vocabulary.

Part II, conducted with a new group of *S*s, was much more thorough and standardized. The position of the non-variable silhouette was always to the left and below the variable. Every *S* was first given six definite, standardized adjustments. The form and sequence of the questions were the same in every case: "Do you see any difference?" "Any other difference?" and so on until the *S* answered, "No." After this, in each of the six cases, the students were asked, "Do you like one better than the other;" "Does one look more intelligent than the other?" and "Does one look more smooth than the other?" ("Smooth," in campus usage, means "superficially attractive" and includes such qualities as neatness, courtesy, worldly wisdom, ease in small-talk, etc.) The children were asked "Do you like one better?" and "Does one look more dumb?"

After these six adjustments, the LPD was measured in four different ways: moving F in, moving N in, moving N out, and moving C in; but in contrast to Part I, the progression of change was from large differences to small. After each trial the shutter was closed and a new adjustment of a different feature substituted. The LPD found by this method were much smaller than before. In this part of the experiment the procedure consisted of the questions, "Any difference?"; "Any other difference?" and so on until the *S* answered "No." All answers, right or wrong, were recorded.

The main experiment in Part II was followed by a 5-min. test of 'suggestibility.' The *S* was instructed as follows: "Now I am going to show you twelve pictures, and in order to make it go faster, I am going to tell you every time just what changes I am making. Now look. I have moved the forehead of the upper one *in*. Do you see it?" etc. Five of the 12 adjustments were actual blanks, in which the two silhouettes were exactly alike. *S*'s percentage of 'suggestibility' was the proportion of these 5 blanks in which he did not resist the suggestion, and said he saw an actual difference.

Results. The only relation that could be found between the features of a silhouette and the character-judgment of an *S* was a fairly constant tendency for any marked deviation to produce dislike. Unpleasant adjectives tended to cluster around any profile which deviated markedly from the standard profile; and, conversely, pleasant adjectives tended to cluster around any face which conformed closely to the standard profile.

Three lines of evidence bear this out: (1) In Part I (c) there were only ten questions which called forth answers uniform enough to be significant, but in every one of these ten cases preference was given to the standard profile. (2) In every case where any deviation was explicitly mentioned by the *S* (15 times), it was directly connected with expressions of dislike. "I like the lower one because the upper one is funny-looking; he looks dumb;" and "I like the lower one because it's not right for the forehead to be straight, like in the upper one," are typical examples of this type of comment. (3) Combining data from Parts I and II, but not including any of the evidence cited above, the following figures

were obtained. In 40 instances in which slight deviations were noted by the *S*, 67.5% produced dislike; in 65 instances of moderate deviation, 31% were disliked; and in 25 instances of very pronounced deviation, 100% dislike was reported.

In referring to the profiles, certain desirable qualities seem to be linked together. This tendency towards a two-fold good–bad classification was constantly observable, especially in children. That is apparent from the facts stated above, and also from the following figures:

Likability and 'smoothness'	(students) overlap 78% (50 cases)
'Smoothness' and intelligence	(students) overlap 87% (38 cases)
Likability and intelligence	(students) overlap 74% (46 cases)
Likability and intelligence	(children) overlap 87% (61 cases)

Variations in the nose or lips are perceived better than in the forehead, chin or throat. The average LPD (only 3 figures available) are: top of forehead, 6.4 mm.; tip of nose, 2.3 mm.; bottom of neck, 6.1 mm. It is an open question whether or not the difference in magnitude of the thresholds is to be accounted for in mere optical terms (relation to background, angular deviation, etc.) or whether the fact that we are more sensitive to certain positions of the face has to do with the part that these play in expressing mental state or mood. An experiment which would throw light on this question might be conducted with Rubin's goblet figures in which two profiles face each other. The angles of the sides of this goblet might be changed and one would then be able to ascertain more clearly the reason for the perception of certain features rather than others.

In connection with this point, attention may be called to certain results reported by Gestalt psychologists, who have noted that the values indicating the visual acuity of a person vary with the optical configurations used.

The least perceptible difference is much greater in children than in adults. In children, the average LPD is 8.88 scale units; in adults 4.47. This difference may be due to a difference in visual acuity, but more probably the explanation is to be found in the fact, fairly obvious throughout the experiment, that children were relatively unable to maintain a fixation of attention.

With respect to the percentages of 'suggestibility,' it was found that, of the ten students, four were 100% suggestible, four were 60%, and two were 40%. Of the ten children all were 100% suggestible.

Discussion, The results of this experiment are of particular interest in that they indicate promising lines of development. It was hoped when we began that we should find evidence either in confirmation or denial of certain of the theories of perception which have been advanced by the Gestalt psychologists. It seemed possible that our *Ss* might view these silhouettes either as entireties or that they might analyze the perceptual experience. Our results were so confusing and indecisive, however, that we have omitted the evidence bearing upon these points.

The fact that our results show individual variations and give evidence of a method which might be used in the study of aesthetic appreciation leads us to propose certain problems which we believe might be critically examined by use of the procedures of this experiment. The correlation between deviation and dislike, found in our data, could be investigated in other ways. *Ss* might be

presented with photographs of faces, landscapes, architectural designs, etc., details of which might be systematically varied, and asked to make judgments of "like or dislike." In the perceptual analysis of such material our deviation-'dislike' correlations might be developed into a fundamental principle of aesthetics.

A more thorough study might be made with our apparatus to determine whether 'character' judgments are more closely allied to the 'dislike' reactions. If, for example, it were found that 'intelligence' was more important than 'moral character' in relation to judgments of like or dislike, it would raise the presumption that the same thing might be true in ordinary human relationships.

It should be possible to determine quantitatively, within certain limits, what the *optimal* profile is, *i.e.* what are the relative dimensions and configurations of the human profile most often found pleasant by the average subject. It would be possible to compare this average with profiles which have been accepted by art critics as most beautiful, and so to establish a quantitative method of aesthetics. This optimal profile could also be compared to a composite profile made up from an unselected group of men and women. These profiles could be then compared to profiles of actors and actresses, or other individuals noted for personal beauty, and again a basis laid which would be of value in aesthetics.

APPARATUS

A NEW MODEL FOR THE DEMONSTRATION OF FACIAL EXPRESSIONS

By J. P. GUILFORD and MARGARET WILKE, University of Nebraska

Following the suggestions made by Piderit,[1] Boring and Titchener constructed a model of the human face in which brow, eyes, nose, and mouth, could be independently removed and replaced.[2] By means of this model it is possible to show what each of these features contributes to the character of the face and also to synthetize a large variety of expressions. The model serves very well as a demonstrational device, and furthermore it has been used in research studies by Fernberger and others.[3]

The Boring-Titchener model has certain limitations, however, which the originators have not hesitated to point out. It shows the face in profile, which makes the division into parts technically easier, but which does not make it possible to produce certain very important expressions, such as laughing and weeping. Wundt's 'sour' mouth also had to be omitted because it does not show well in profile. In order to supplement the list of total expressions possible with the Boring-Titchener model and to show the facial expressions more completely, we have built a new model based upon the same principles but presenting a view of the full face.

The face was divided into parts according to the thin dividing lines in Figs. 1 and 2. The cheeks belong functionally with the mouth in facial expressions. Changes in the mouth spread up and around the nose to the inner corners of the eyes. The changes in the forehead and brow involve the region which extends down between the eyes. These facts account for the somewhat irregular dividing lines. We have tried to place these lines in the most 'neutral' zones. They are drawn straight, wherever possible, to avoid confusion with the lines of expression, which are usually curvilinear. Indeed, the lines which bound the eyes could be very easily mistaken for the edges of spectacles of the modernistic type.

The construction of the model is much like that of the Boring-Titchener face.[4] The expressions were drawn upon white drawing paper of good quality

[1]Th. Piderit, *Mimik und Physiognomik*, 1867, 1886, 1909.

[2]E. G. Boring and E. B. Titchener, A model for the demonstration of facial expression, this JOURNAL, 34, 1923, 471-485.

[3]D. E. Busby, The interpretation of facial expression, this JOURNAL, 35, 1924, 602-604; E. Jarden and S. W. Fernberger, The effect of suggestion upon the judgment of facial expression of emotion, *ibid.*, 37, 1926, 565-570; S. W. Fernberger, Six more Piderit faces, *ibid.*, 39, 1927, 162-166.

[4]The project was carried out under the supervision of Professor Guilford. Miss Wilke, student in Fine Arts, made the drawings. Our thanks are expressed to Mr. William E. Walton, who supervised the construction of the model.

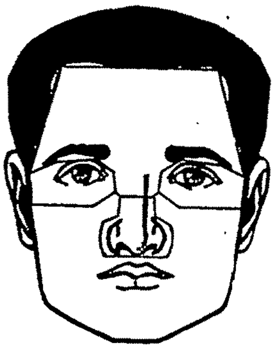

Fig. 1

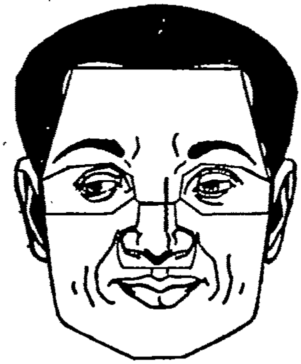

Fig. 2

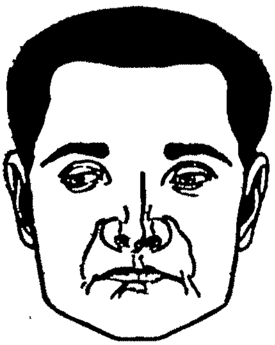

Fig. 3

Fig. 4

Fig. 5

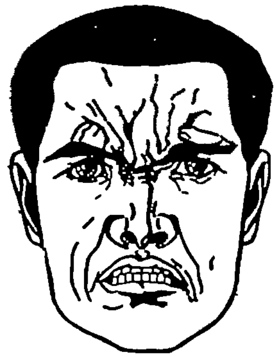

Fig. 6

Figs. 1–6. Examples of Expressions Obtained from the Demonstrational Model
(Figs. 1 and 2 show the divisions)

glued upon three-ply wood 5/16-in. thick. The main piece, which contains the hair and ears, is permanently fastened to a wooden base of the same wood, 18x24x5/16 in. The other pieces have wooden pegs set in the back and these pegs fit into holes in the base-board. The face itself is 17 in. tall and 11 in. wide at the ears. No neck is included in the model, for it would normally change some of the expressions (*e.g.* in anger), and, after all, it is only the facial expressions that we want. The absence of a neck also makes it possible to drop the chin when an expression demands it.

The models for the expressions were chosen from two sources, the Rudolf sketches[5] and the Ruckmick photographs.[6] The Piderit faces were not available. Using the selection of parts of the Boring-Titchener model as a guide, we chose what seemed to be the most typical brows, eyes, noses, and mouths from the Rudolf faces. If we failed to find a satisfactory expression among these pictures, we resorted to the Ruckmick group. The former proved to be better models because they are pencil sketches and give the very lines that were needed in our drawings, whereas the latter, being photographs, are lacking in facial wrinkles which are needed to convey an expression in a drawing.

The following lists include the varieties of expression for each part:

Brows: (1) normal—passive; (2) raised—horizontal wrinkles; (3) contracted—vertical wrinkles; (4) raised and contracted; (5) slightly contracted; (6) slightly raised. The last two milder expressions were found to be necessary to give certain of the expressions which Allport classifies in his 'attitudinal' group.[7]

Eyes: (1) normal—passive; (2) raised—introverted, religious; (3) wide open—attentive; (4) very wide open—very attentive; (5) closed—lowered lid, inattentive; (6) narrowed—suspicious; (7) averted—voluntary inattention; (8) laughing—crow's-feet; (9) weeping.

Noses: (1) normal—passive; (2) raised and dilated nostrils—attentive disgust; (3) dilated nostrils—attention; (4) raised nostrils—disgust.

Mouths: (1) normal—passive; (2) sweet—pleasure; (3) bitter—displeasure; (4) very bitter—great displeasure; (5) set—determined; (6) bitter and set; (7) open—attentive; (8) open and bitter—unpleasant attention; (9) clenched teeth—anger; (10) laughing; (11) weeping; (12) sour.

The addition of two new brows, four new eyes, two new noses, and three new mouths, considerably increases the number of possible combinations. Provided all the combinations give sensible and notably different total expressions, the number would be nearly 3000. Of course, only a fraction of these are significant or useful.

The numbering of our pieces coincides with those of the earlier model in so far as they have common elements. One may therefore follow the procedure of Boring and Titchener with the new model. Figs. 2 to 6 show a meager example of the new expressions to be obtained. We have not attempted to exhaust the new possibilities and to name them. Rather than to follow the scheduled succession of faces laid down by Boring and Titchener, we have used a new plan of demonstration. In fact two methods have been followed.

[5] H. Rudolf, *Der Ausdruck der Gemüthsbewegungen des Menschen*, 1903.
[6] C. A. Ruckmick, A preliminary study of the emotions, *Psychol. Monog.*, 30, 1922, (no. 136), 30-35.
[7] F. H. Allport, *Social Psychology*, 1924, 208.

The first procedure consists in explaining the meaning of each piece. This gives to students a vocabulary of the face, one might say. So far we are in agreement with the profiles. Then each of the outstanding pieces is used in turn as a basis for the total expression. For example, total expressions might be built around the bitter mouth, the raised nostrils, the raised eyes, or the contracted brow. This plan well demonstrates the fact brought out by Dunlap,[8] that the mouth is superior to the eyes or brow in determining the total result. It also demonstrates, incidentally, an important principle of the *Gestalt*, that a change in one part may alter all the others; that there is a readjustment of the parts as they become a new whole.

A second procedure, which may follow the first or be substituted for it, consists in building up the faces according to Allport's six groups.[9] The six groups are first defined and the outstanding characteristics of each are explained. Using the chief characteristic as a basis for each group, variations are introduced. For example, with the raised brow and normal or wide-open eye as a basis for the surprise-fear group, various noses and mouths can be added to produce the modifications of these emotions. The expressions of the model fall very neatly into the six groups.

No tests have as yet been made to determine how well subjects would agree with each other in naming the expressions or how well they would accept names which had been assigned to them. From its use thus far as a purely demonstrational device, it has satisfied the writers that its range of expression is adequate, its facial vocabulary is increased over that of the Boring-Titchener model, and it seems to be convincing to those who have seen it.

THE RELIABILITY OF THE ELEVATED SKELETON MAZE

By Stephen Maxwell Corey, University of Illinois

The records upon which this study[1] is based were gained from an elevated skeleton maze which resembles closely the one described by Miles.[2] Fundamentally similar structures, however, were used in 1912 by Vincent,[3] and in 1913 by Turner.[4]

The maze was constructed of strips of pine 3 cm. wide and 2 cm. thick. Every unit was 24 cm. in length. The total length of the maze from start to finish was 240 cm. The runways were elevated 1 m. above the floor. Every

[8]K. Dunlap, The rôle of eye-muscles and mouth-muscles in the expression of emotions, *Genet. Psychol. Monog.*, 2, 1927, 197-233.

[9]Allport, *op. cit.*, 203-209.

[1]From the Laboratories for Research in Athletics at the University of Illinois.

[2]W. R. Miles, The narrow path elevated maze for studying animals, *Proc. Soc. Exper. Biol. & Med.*, 24, 1927, 454 f.

[3]S. B. Vincent, Function of the vibrissae in the behavior of the white rat, *Behav. Monog.*, 1, 1912.

[4]C. H. Turner, Behavior of the common roach on an open maze, *Biol. Bull.*, 25, 1913, 348-61.

turn was a right angle; 5 of the 8 junctions of the different units were of the T variety, the writer agreeing with Huang,[5] and Hunter and Randolf[6] that this type of junction is the best. Because the runs were without walls, overhead lighting did not throw shadows upon them. The runways were heavily coated with shellac, making it possible to wash them at regular intervals and to remove all possibility of trailing by smell. The entire maze was fenced by wall-board partitions. *E* observed the rats through a small opening in one of the partitions. This opening was screened so that *E* could not be seen by the animals running the maze, although they could be clearly seen by him.

The maze, when experimentally tested, proved to be highly reliable. The norm of mastery, in our own experiments, was 5 consecutive errorless trials; practice being distributed in groups of 5 runs a day. Errors (a complete entrance into a cul-de-sac while running in a forward direction), total time, and active time were recorded. The latter represented the number of seconds of actual running time required to learn the maze; the measurement being made with a cumulative stop watch. Coefficients of reliability between error, time, and active time scores made on odd and even trials are shown in Table I. The correlations between errors made on the first and last half of the maze and between those made on odd and even culs-de-sac are also indicated. These

TABLE I

RELIABILITY COEFFICIENTS FOR THE ELEVATED SKELETON MAZE

Variables correlated	r	p.e.
Error scores on odd and even trials	0.89	0.0155
Time scores on odd and even trials	0.96	0.0065
Active time scores, odd and even trials	0.92	0.0115
Error scores on first and second half	0.85	0.0205
Error scores on odd and even culs-de-sac	0.90	0.0145

correlations are based on the learning records of 186 sixty-day-old albino rats from Wistar stock. No formula similar to Brown's has been applied to raise them. They indicate that the maze measures something with a high degree of accuracy. Stone and Nyswander, using a multiple T-maze, give the following ranges of reliability coefficients comparable to ours;[7] error scores on odd and even trials, 0.59–0.97; time scores on odd and even trials, 0.74–0.88; and error scores on odd and even culs-de-sac, 0.71–0.96.

Stone also gives these median coefficients of reliability for a modified Carr maze:[8] error scores on odd and even trials, 0.74; time scores on odd and even trials, 0.74; and error scores on first and second half, 0.46.

[5] L. Huang, An analysis of the maze technique, *J. Comp. Psychol.*, 8, 1928, 301-11.

[6] W. S. Hunter and Vance Randolf, Further studies in the reliability of the maze with rats and humans, *ibid.*, 4, 1924, 431.

[7] C. P. Stone and D. B. Nyswander, The reliability of rat learning scores on the multiple T-maze as determined by four methods, *Ped. Sem.*, 34, 1927, 497-524.

[8] C. P. Stone, The reliability of rat learning scores obtained from a modified Carr maze, *Ped. Sem.*, 35, 1928, 507-521.

Shirley, using a modified Hampton Court labyrinth, reports the following coefficients of reliability:[9] error scores on odd and even trials, 0.62; and time scores on odd and even trials, 0.34.

In addition to these customary methods of judging the reliability of a maze, the writer has computed the coefficients of correlation between the records of all *S*s as measured by the different criteria; namely, errors, active time, time, and trials. Table II gives these reliability coefficients for 6 groups

TABLE II

Inter-Correlations for All Criteria

Criteria correlated	Group 1 r	Group 1 p.e.	Group 2 r	Group 2 p.e.	Group 3 r	Group 3 p.e.	Group 4 r	Group 4 p.e.	Group 5 r	Group 5 p.e.	Group 6 r	Group 6 p.e.
Errors: act. time	.90	.023	.95	.02[illegible]	.83	.037	.89	.023	.91	.020	.90	.023
Errors: trials	.89	023	.89	.023	.91	.020	.89	.023	.86	.031	.91	.020
Trials: act. time	.93	.018	.90	.023	.87	.027	.89	.026	.78	.045	.82	.039
Errors: time	.63	.073	.63	.073	.41	.100	.31	.110	.60	.078	.31	.110
Time: trials	.58	.081	.46	.090	.39	.103	.23	.115	.28	.111	.21	.116
Time: act. time	.71	.060	.71	.060	.72	.057	.43	.098	.68	.064	.58	.080

of 31 *S*s, all sixty days old. All of the correlations, with the exception of those having total time as one of the variables, are high. The writer agrees with Warden and Hamilton that total time scores are rather poor criteria for measuring learning ability.[10] Total time measures more factors in addition to learning ability than do error, active time, and trial scores. The latter three correlate very highly with each other (see Table II) but whenever total time records enter into the computation the coefficients are appreciably lowered. Total time measures something accurately, as is indicated in Table I, but to the extent that it does not correlate with the other three criteria, it is not measuring the same thing, namely that which we wish to call learning ability.

The chief advantages of the elevated skeleton maze, as they appear to the writer after more than a year's experience with it, are:

(1) The reliability is unusually high.

(2) The narrow path and lack of walls serve to eliminate many of the rat's exploring movements, thereby (a) decreasing running time, (b) giving greater assurance that running is learning activity and not merely spontaneous activity, and (c) making error scores and distance scores practically the same.

(3) The convenient floor and shellac permit of rapid washing to remove all possibility of trailing.

[9]M. Shirley, Studies in activity, IV. The relation of activity to maze learning and to brain weight, *J. Comp. Psychol.*, 8, 1928, 187-195.

[10]C. J. Warden and E. J. Hamilton, Effect of variations in length of maze pattern upon rate of fixation in the white rat, *Ped. Sem.*, 36, 1929, 229.

(4) Overhead lighting does not throw shadows on the floor of the maze.
(5) Observations can be made while the experimenter is hidden.
(6) Falls from this type of structure are negligible, less than 6 occurring in more than 5500 trials.

A PORTABLE PHONOGRAPHIC APPARATUS FOR GIVING OBJECTIVELY UNIFORM SUGGESTIONS

By Clark L. Hull, Robert G. Krueger, and Griffith W. Williams,
Yale University

It is an axiom in all experimental research that but one factor should be varied at a time. Hitherto it has been peculiarly difficult to approach this ideal in the administration of experiments involving direct hetero-suggestion. In order to exploit, for this purpose, recent advances in phonographic record and reproduction, the apparatus here reported was assembled. It is shown in Fig. 1.

The required parts are as follows:

(1) A Gordon electric phonograph-motor with turn-table and adjustable speed-control.
(2) A Gordon electric phonograph pick-up with tone-arm and volume-control.
(3) A Utah dynamic loud-speaker.
(4) A pine baffle-board, 42x42x1½ in., firmly glued and sutured into a solid piece. This aids the dynamic speaker in effecting a superior reproduction.
(5) A seven-tube A.C. table-model radio set.
(6) A Burroughs adding-machine carriage provided with large rubber-tired casters.
(7) A 10 in. aluminum phonograph-disk. On one side of this is recorded, in the voice of the person conducting the experiment, four minutes of suggestions to postural movement. On the other side is recorded four minutes of hypnotic suggestions designed to be used in conjunction with the hypnotic head-piece and polished aluminum fixation-object sold by C. H. Stoelting. The total cost of all parts including the disk and fixation-object is under a hundred dollars.

The loud speaker (A) is attached to the baffle-board (B) just behind a hole of suitable dimensions in the center of the baffle-board. The baffle-board itself is mounted in a vertical position on the top of the carriage-frame somewhat forward of the center, by means of screws and a wooden brace. The phonograph-motor, turn-table and tone-arm are mounted in a simple pine box (C) of suitable dimensions. This is suspended by screws from a convenient pair of cross-pieces of the frame. Beneath this, on a second pair of cross-pieces, rests the radio set (D).

When in use the apparatus is wheeled to the position before the subject that an experimenter would take if personally giving the suggestions. The height and general direction from which the sounds come are thus approximately the same as when the suggestions are given by the experimenter. The needle is placed on the first groove of the record and when all is ready, a silent switch is closed and the motor starts. Uncouth sounds ordinarily produced while the

motor is gaining speed are avoided by having the first few turns of the record silent. The suggestions may instantly be interrupted at any time by turning the knob of the volume-control to zero, after which the motor may be stopped by opening the controlling switch.

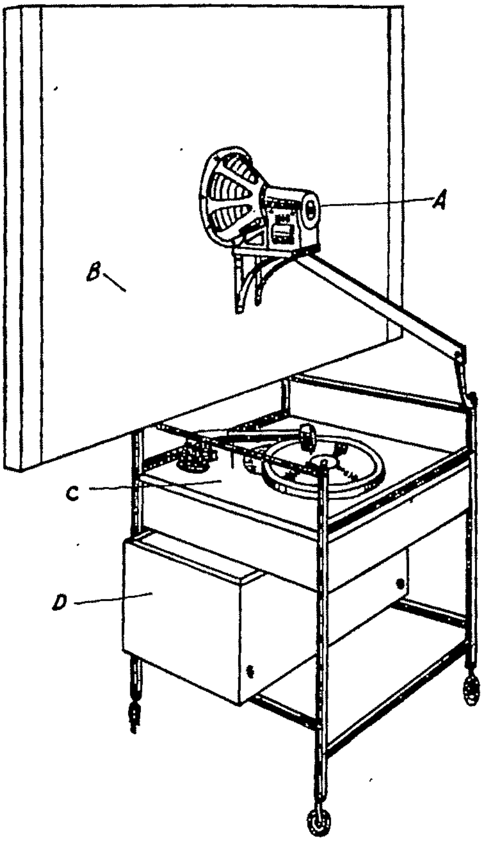

FIG. 1. DRAWING SHOWING THE REAR VIEW OF PHONOGRAPHIC SUGGESTION-GIVING APPARATUS
(A) is the loud speaker, (B) the baffle-board, (C) the phonograph, and (D) the radio set.

When the plan of using phonographic presentation of suggestions was first proposed,[1] fear was felt that the perceptible artificiality of all reproduced speech might so weaken the potency of the suggestions as to render the method of little value. These fears seem to have been largely unfounded. Out of the first 20 subjects tested for waking postural suggestion, 15 proved susceptible.[2] This is

[1]C. L. Hull, Quantitative methods of investigating waking suggestion, *J. Abn. and Soc. Psychol.*, 24, 1929, 161.
[2]*Op. cit.*, 153 ff.

as large a proportion as is usually obtained by direct vocal suggestion. In several cases the hypnotic side of the record, in conjunction with the Stoelting fixation object, was tried on selected but unpracticed subjects. With all, the trance was both induced and terminated easily and without interference from the experimenter. The indication is that the method described, particularly when good non-metallic records are used, will be quite adequate for the ordinary requirements of suggestion under experiment, as well as for the practical testing of suggestibility. Indeed, there is reason to believe that the apparatus and technique here described should have wide research usefulness wherever verbal or other auditory stimulations are to be employed.[3]

APPARATUS NOTES

A New Ambiguous Figure

Puzzle-pictures have long interested psychologists. Gudden's *brain-and-babies* was published in *Jugend* in 1896 at the time of the Third International Congress of Psychology at Munich. Titchener based his experiment on attention in his *Experimental Psychology* (1901) on the then popular puzzle-pictures. Recently Rubin (1915) has given us the *goblet-profile* as one of the more objective of his numerous stimuli illustrating the difference between figure and ground. Many of Köhler's illustrations of sensory organization (cf. *Gestalt Psychology*, 1929) are really forms of puzzle-pictures. Obviously "attention" and "sensory organization" are simply different names for the same phenomenal fact that such figures illustrate. Probably *figure-and-ground* must be thought of as a special form of organization, for, in a case like that of the picture accompanying this note, the two alternating figures interpenetrate each other spatially and there is no definite division of the field by a contour, as in Rubin's cases.

An Ambiguous Figure

The picture presented herewith is not strictly new. It was drawn by the well-known cartoonist, W. E. Hill, and reproduced in the issue of *Puck* for the week ending November 6, 1915. It is, however, relatively unknown to psy-

[3]When this manuscript was submitted for publication, the author had not read the note of G. H. Estabrooks (this Journal, 42, 1930, 115-116) proposing "A standardized hypnotic technique dictated to a victrola record."

chologists, and seems to me to be the best of the puzzle-pictures in the sense that neither figure is favored over the other. In this respect it is the peer of Rubin's goblet-profile, which, however, does not have the two figures interpenetrating in the same region of the total field. The present cut is from a pen-and-ink copy of Hill's published half-tone. I am indebted to Mrs. W. H. Hunt for the copy, which is, if anything, a little better for the psychologist's use than the original.

This picture was originally published under the title "My Wife and My Mother-in-law." It shows in one figure the left profile of a young woman, three quarters from behind The other figure is an old woman, three-quarters from in front. The ear of the 'wife' is the left eye of the 'mother-in-law;' the left eye-lash of the former is the right eye-lash of the latter; the jaw of the former is the nose of the latter; the neck-ribbon of the former, the mouth of the latter.

Harvard University — Edwin G. Boring

THE MUNSELL COLORED PAPERS

The purpose of this note is to call attention to the fact that the Munsell Color Company of Baltimore manufactures colored papers that are carefully standardized. These papers enable the experimenter to obtain relatively independent variations of hue, intensity (tint), and chroma and to repeat with precision the work of other investigators.

The Munsell "Book of Color," in which the fundamental characteristics of the usual qualities are defined and illustrated, is a revision and extension of the firm's well-known "Color Atlas." All the colors and grays are here systematically ordered, thus permitting a simple and unequivocal specification of any desired qualities. All the colors and grays are arranged in psychologically equal steps.

Clinton, N. Y. — Paul Chatham Squires

NOTES AND DISCUSSIONS

The Two-Point Limen and the Error of Localization

The general rule in the tactual perception of space is that the limen of dual discrimination upon the skin is greater—usually much greater—than the error of localization for the same region of the skin. This rule must be taken as applying safely in the case where the comparison is between the more conventional methods of making these two determinations. For the two-point limen the standard method involves some psychophysical procedure with laboratory observers, and these observers presumably tend to report on the perceptual pattern of 'oneness' or 'twoness,' and not to say 'two' when a large 'one' implies the presentation of a dual stimulus.[1] Thus the sophisticated observer tends to give a larger limen than the naïve subject. For the determination of the error of localization the conventional method is for the subject, after tactual stimulation, to localize the spot roughly by pointing to the region, to touch the skin, and then to correct the localization by reference to the tactual perception, all the while using visual imagery freely although denied, by closed eyes or other device, the advantage of visual perception.

Weber's original experiments showed the difference in question. He explained the discrepancy in part by the theory that the perception of duality arises only when the stimulation is of two 'sensory circles' which are separated from each other by at least one other 'sensory circle,' thus making the two-point limen two or three times the average diameter of a 'sensory circle.' He also, attributed the difference to the fact that all tactual perceptions of form are more precise when they involve movement of the stimulus upon the skin, for localization by his method arises, in the last stage, from a moving tactual stimulus.[2]

Other investigators have found this same quantitative difference[3] and the relationship of the results of the two experiments has become recognized as an established fact.[4] There have been various theories and differences of opinion into which it is not necessary to enter here. The view developed that these two experiments measure different psychophysiological mechanisms. Wundt,

[1] Cf. E. G. Boring, The stimulus-error, this Journal, 32, 1921, 465-470, esp. 468 f., and the references there cited, esp. to Titchener, DeLaski and Friedline.

[2] E. H. Weber's original determinations of the two-point limen—the results of the "compass test"—were first described in *De tactu: annotationes anatomicae et physiologicae*, 1834. He made these results the basis of his positing of an *Ortsinn* in his Der Tastsinn und das Gemeingefühl in R. Wagner's *Handwörterbuch der Physiologie*, III, ii, 1846, 524-540 (see esp. 539 f.). He repeated the table of these results, described the method of determining the error of localization (Weber's "second method"), and discussed the relationship of the two determinations in Über den Raumsinn und die Empfindungskreise in der Haut und im Auge, *Ber. d. k. sächs. Gesell. d. Wiss. zu Leipzig, math.-phys. Cl.*, 1852, 85-128, esp. 87-96.

[3] Cf., esp., R. Kottenkampf and H. Ullrich, Versuche über den Raumsinn der Haut der oberen Extremität, *Zsch. f. Biol.*, 6, 1870 37-52.

[4] Cf., *e.g.* W. Wundt, *Physiologische Psychologie*, 4th ed., 1893, II, 5-8, 13-18; V. Henri, *Über die Raumwahrnehmungen des Tastsinnes*, 1898, 90-93.

for instance, pointed to the fact that the tables of these two measurements in different bodily regions do not show exact correspondence for the relative change in the size of the measures when the part of the body stimulated is changed. The results to be compared in this way, however, are apt not to be strictly comparable as to the conditions of localization and as to the definition of the region; and an imperfect correspondence can, moreover, be clearly made out.[5]

More recently Henry Head and his collaborators have reënforced the belief in a fundamental difference in the mechanism of the two perceptions by assigning the capacity for localization to the 'deep sensibility' of the tissues underlying the skin, and the capacity for the discrimination of duality to the 'epicritic sensibility' of the skin.[6] Such a view, however, does not make sense with the known psychology of the tactual perception of space. Does it mean that two points that are differently localized by way of the subcutaneous tissue, where the skin is anesthetic, are nevertheless sensed as one? Or that in every light stimulation of the normal skin, when the subcutaneous tissue is not involved, two perceptual points are felt but are not localized anywhere at all? At any rate, these results have not been confirmed, and there is considerable reason to doubt the validity of the original interpretation of rather meager data by Head and Rivers.[7] Nevertheless, Wundt took Head's experiment as establishing his earlier belief in two different physiolgical mechanisms for the two perceptions.[8]

What seems not to have been said in all the discussion that has taken place is that the limen for duality ought to be several times as large as the error of localization if the two perceptions are fundamentally dependent upon the same physiological mechanism (*i.e.* the potentiality of different tactual receptors for giving rise to different localizing contexts, both conscious and behavioral). Reference to the accompanying schematic diagram, Fig. 1, will make this point clear.

Let us suppose that two similar adjacent points on the skin, A and B, are under consideration. Let us suppose further that stimulation of A, under fixed conditions, is effective at a distance r_1 from A, *i.e.* that the larger circle about A

[5]For instance, Weber determined the two-point limen for 25 bodily regions, and the error localization for 8 regions. Only four of the regions in the two cases are the same, and for these places the two measures are in about the same ratio. Thus:

	Two-point limen L(mm.)	Error of localization E(mm.)	Ratio L/E
Lip	4.5	1.1	4.1
Forehead	22.5	6.3	3.6
Back of hand	31.5	6.5	4.8
Volar forearm	40.5	8.5	4.8

[6]H. Head and W. H. R. Rivers, A human experiment in nerve-division, *Brain*, 31, 1908, 360-365; or Head, *Studies in Neurology*, 1920, I, 296-305.

[7]See Boring, Cutaneous sensation after nerve-division, *Quart. J. Exper. Physiol.*, 10, 1916, 67-72, and esp. *idem*, Relation of the limen of dual impression to Head's theory of cutaneous sensibility, *Proc. VII Internat. Congr. Psychol.* (Oxford, 1923), 1924, 57-62.

[8]Wundt, *Physiologische Psychologie*, 6th ed., II, 1910, 464 f.

is the total region of stimulation. That there is some dispersion of stimulation about the stimulus is clear. Deformation of the skin would cause it. Von Frey's theory of tension as the medium of stimulation would require it. Finally let us suppose that the stimulation at B gives a picture exactly like that at A. What would happen?

If we stimulate A and B simultaneously, we get the crucial case where a unitary pattern is just about to separate into a dual pattern, the familiar 'dumb-bell' or 'double-paddle' of this perception. If the distance AB were greater, the perception would fall apart into two parts; if it were less, it would fuse into one figure. Thus the distance AB represents the two-point limen.

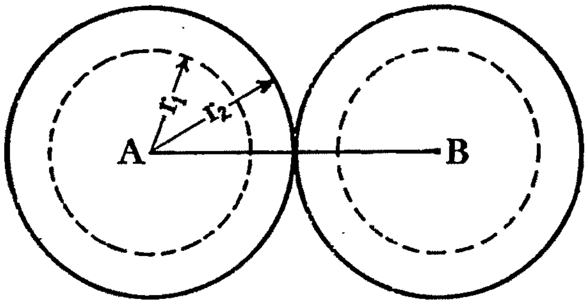

FIG. 1. SCHEMATIC DIAGRAM SHOWING THE RELATION BETWEEN THE TWO-POINT LIMEN AND THE ERROR OF LOCALIZATION

On the other hand, if we stimulate A alone we may conceive that the final localizations, after correction by the tactual cues introduced in localizing, will ordinarily fall somewhere within the zone of stimulation, *i.e.* within the circle of which r_2 is the radius. If these localizations were scattered uniformly within this circle, then the average error of localization would be r_1, provided the circle for r_1 be so drawn that the area it encloses is equal to the annular area between the two circles. This condition is met if the ratio $r_1:r_2$ is $1:\sqrt{2}$, or approximately 1:1.4.

The conclusion follows at once. The two-point limen (AB) ought, on these assumptions, to be about 2.8 times as large as the error of localization (r_1).

This ratio is not so far from the ratios which Weber's data give,[9] but more satisfactory figures, obtained under more certainly comparable conditions, are to be found in the research of Kottenkampf and Ullrich. They give data for eight regions of the volar arm as shown in the Table.[10]

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[9]Cf. foot-note 5 above.

[10]Kottenkampf and Ullrich, *op. cit.*, 44, 47. In the following Table the limens have been computed by the present writer, by means of Urban's procedure for the method of constant stimuli, from the relative frequencies (psychometric functions) that Kottenkampf and Ullrich gave for the volar forearm and the volar upper arm (p. 44). These eight limens are based on from 200 to 525 cases each. The errors of localization are the averages for two observers (the authors), and each derives from only 28 to 37 observations (p. 47). All of the values here have been translated into mm. from Paris lines, on the assumption that 1 Paris line = 2.27 mm.

TABLE SHOWING THE TWO-POINT LIMEN, THE ERRORS OF LOCALIZATION, AND THE RATIO BETWEEN THESE TWO VALUES; COMPUTED FROM KOTTENKAMPF AND ULLRICH'S DATA

Location			Two-point limen L(mm.)	Error of localization E(mm.)	Ratio L/E
Volar forearm	cm. from elbow	21.1	14.21	4.40	3.2
		16.2	20.96	6.17	3.4
		11.3	20.70	8.01	2.6
		6.4	22.64	8.42	2.7
		2.2	27.01	8.74	3.1
Volar upperarm	cm. from shoulder	21.3	33.81	9.42	3.6
		13.6	36.25	10.60	3.4
		5.9	37.39	14.16	2.6
					Av. = 3.1

There is no need to stress the approximation of the ratios in the last column of this Table to the value, 2.8, which the schema yields. The schema is ever so much too simple for precise quantification. In the first place, it leaves out of account the variability which is the constant feature of psychophysical experimentation. In the second place, localizations would not always fall within any fixed zone. They would probably scatter within diminishing frequency to points indefinitely remote from the point of the stimulus. In the third place and for this same reason, they would probably tend to pile up around the point of the stimulus, so that the error of localization would be even smaller than the schema requires. Moreover, the zones of stimulation would not be circular. On the volar forearm the longitudinal limen is much greater than the transverse, and the longitudinal errors are greater and more frequent than the transverse. The errors of localization are free as to direction, but the two-point limen is determined as to direction. A comparison of average results ought properly to be made between the average error of localization and the average of longitudinal, transverse, and diagonal two-point limens.

We need not, however, labor all these short-comings of the argument. It is enough if we realize that the fact, that the two-point limen is several times as great as the average error of localization, may be simply an immediate result of the statistical definition of these two measures, and affords, therefore, no ground for a belief in separate fundamental physiological mechanisms. The two results are not necessarily inconsistent and may easily both be measures of the same discriminatory capacity.[11]

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[11]It ought to be said that this point of view underlies the experiment of W. S. Hulin, An experimental study of apparent tactual movement, *J. Exper. Psychol.*, 10, 1927, 293-320. Cf. esp. Hulin's method for the report of his *Os*, pp. 299-302. Were these *Os* localizing two points or discriminating between them, or was the judgment both of these things at once? Had Hulin followed the more conventional thought about localization and the discrimination of duality, he would have been handicapped in formulating a sufficiently inclusive setting for his experiment.

A SIMPLIFIED SCORING METHOD FOR THE KOHS BLOCK-DESIGN TESTS

A comparatively recent addition to the field of non-language tests is the Kohs Block-Design Tests. Kohs presents considerable evidence which attests their value for measuring some of the higher mental processes, and for use as a diagnostic instrument.[1] The test has, however, a serious limitation in its scoring technique which, the writer believes, has prevented its more universal adoption in clinical use. It is generally accepted that in actual clinical practice efficiency of administration, and economy of time and of effort, are almost as important factors in determining the widespread use of a test, as are such truly scientific criteria as validity and reliability. Kohs thinks "that success, speed, and accuracy each has its own diagnostic importance, and in order to make the tests more effective all should, and must, be taken into account in the final score summation."[2] Thus in order to include accuracy as a refining factor in the test, he assigns specific weights in scoring to the number of 'moves' which the subject makes in completing the design. The scoring of these 'moves' introduces a number of difficulties in administration and scoring, which may desirably be avoided.

Experience with the test in the Educational Clinic of the College of the City of New York has justified the conclusion that examiners may interpret in markedly differing ways the instructions for recording moves.[3] The writer has determined empirically, for example, that when two examiners score the same test a difference in the final score of from 6 to 12 months is frequently obtained. This conclusion is based upon the results of about 10 previous administrations of the test, not included in this study, and upon the findings of several psychologists in other clinics, with whom the writer has had occasion to consult. Another limitation of this scoring technique is that the examiner must give his undivided attention to observation of the movements of the subject's hands throughout the entire duration of the examination (about 30 to 40 min.). He is unable to observe any other phase of the *S*'s total behavior. If he permits himself to look away, even for a moment, he loses count of the number of moves. Thus the administration of the test becomes an uninteresting and even fatiguing process. The writer has attempted, therefore, to eliminate these difficulties by devising a new scoring technique which is of approximately the same diagnostic value as the Kohs method, but which excludes the necessity of scoring 'moves.'

His investigation was limited in the selection of its techniques by two considerations: (1) The new score had to be directly comparable to the old in order to preserve the same diagnostic value. (2) It was desirable, because of the small number of cases, to use the Kohs norms, or mental age equivalents, instead of computing new base-line values for the test. The technique finally selected, which is here described, claims as its chief advantage the simplicity of its scoring method, and consequent ease of administration.

The *Ss* in the experiment were 23 boys and 10 girls between the ages of 6 yr. 8 mo., and 15 yr. 3 mo. Their average and median Stanford-Binet I.Q.'s were both 88.

[1] S. C. Kohs, *Intelligence Measurement*, 1923, xii, 312.
[2] *Op. cit.*, 71. [3] *Op. cit.*, 76.

Previous researches with performance tests have shown that there is a considerable positive correlation between time and moves.[4] This fact indicated a technique for the revision of the scoring scheme, and suggested the method finally adopted. It will now be necessary to consider the scoring method described by Kohs.

The following are two illustrations of this method. Design No. 1 has a score value of 3. The maximum score is attained if an *S* completes the design successfully in less than 21 sec., and with less than 6 moves. If 21 or more sec. are required, one point is deducted from the score, and if 6 or more moves are made, an additional point is deducted. Similarly for design 4, if the *S* requires 10 or more moves, one point is subtracted; for 31 sec. to 1 min. an additional point is subtracted, and if 1 min. and 1 sec., or over, is required, still another point is deducted. There is a maximum time limit for each design, which is usually about 1 min. more than is necessary for its completion. When an *S* fails to complete the design in the specified time he fails on that design and obtains a zero score. The examination is continued until 5 consecutive failures have been recorded. The entire test consists of 17 designs.

The typical division of points in scoring, described above, is based on the ratio of 4:2:1 for success, time, and accuracy respectively, which Kohs believes is a correct weighting of the elements involved in completing the designs.[5] The scores for all of the designs are summated and the total score is then converted into a Kohs Mental Age by means of the table presented by Kohs.[6]

It will be noted that 3 points are allotted for both time and moves for each design. In order to eliminate the scoring of the moves the writer assigned all of these 3 points to time alone According to this arrangement, the score value for each design is retained, but instead of subtracting 1 point or 2 points for time, 1.5 points or 3 points, respectively, are now to be deducted, and no points are assigned to moves.

All of the tests were re-scored according to this scheme. The scores for each individual were then totalled, and once more were converted into mental age equivalents by the use of the Kohs table of mental age equivalents mentioned above.

The following correlations were found: between Stanford-Binet mental age and Kohs 1 (old scoring) mental age, $0.43 \pm .09$; between Stanford-Binet mental age and Kohs 2 (new scoring) mental age, $+0.46 \pm .09$; between Kohs 1 mental age and Kohs 2 mental age, $+0.93 \pm .01$.[7]

The average change in mental age is +2.8 months, and the average change in intelligence quotient is +1.5 points. The middle 50% range of changes in M.A. and in I.Q., which may be interpreted similarly to the probable error, is from 0 to +5 months in M.A., and from 0 to +3 points in I.Q.

[4]R. Stutsman, Performance tests for children of pre-school age, *Genetic Psychol. Monog.*, 1 1926, 3-67 R. H. Sylvester, The form board test, *Psychol. Monog.*, 15, 1913, (no. 65), 1-56. The Clinic has on file an unpublished study which indicates that time alone is an adequate criterion for scoring the Pintner-Paterson Scale of Performance Tests.

[5]Kohs, *op. cit.*, 143.

[6]*Idem.*, 73. The table to which this footnote refers should also be used to transmute the new scores, later referred to, which are based on the revised scoring scheme, into mental ages.

[7]See next page.

One case obtained a much lower Kohs M.A. (2) than Kohs M.A. (1). The following remarks were made during this individual's examination: "Works very slowly and very cautiously. Makes the minimum number of moves." The explanation of the considerable change in score, therefore, lies in the unusual type of response. Since the revised scoring scheme neglects 'moves' and emphasizes 'time,' it yields results which are not closely comparable with the original scoring scheme for those individuals who respond in some unusual manner. These types include those who work very rapidly with a great number of moves, work very slowly with a small number of moves, or respond in some pathological manner. Consequently, the score of any individual who shows some markedly unusual method of attack should be interpreted with caution. It may be added that clinical experience with tests has demonstrated that this caution must be observed in the interpretation of the result of *any* examination where the response is queer or unusual. In such cases as the one mentioned above the discrepancy occurs because the original Kohs method of scoring assigns values to extraneous elements, such as psychopathic slowness, which unduly influence the mental age ratings. Hence for those cases in which the scoring of moves creates the greatest discrepancies, the revised scoring scheme may actually be the more valid.

Since the number of cases is small and there was only one examiner, the results are not entirely conclusive. The data indicate the following:[8]

(1) The revised method of scoring yields results which are closely comparable with the original Kohs scoring scheme. Increased efficiency in the administration of the test, and marked economy of time and effort are secured.

(2) The two methods of scoring are about equally reliable for both low and high mental ages.

(3) The changes in score, and consequent mental ages, are such that the intelligence quotients derived by the original and by the revised methods are approximately identical for both small and large quotients.

(4) The most probable changes in mental age range from 0 to +5 months. The average change is +2.8 months.

(5) The most probable changes in intelligence quotient range from 0 to +3 points. The average change is +1.5 points.

(6) Erratic or unusual responses should be interpreted with caution. The writer assumes that the experienced examiner may be relied upon to detect such responses.

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[7]It may be advisable to present the correlations that other investigators have found between Stanford-Binet M.A. and Kohs M.A. Kohs reports (*op. cit.*, 181 f.) a correlation of $+0.82\pm.01$ for 366 cases. When these cases are divided into two groups of 291 public school cases and 75 feebleminded cases, the correlations are $+0.81\pm.01$, and $+0.67\pm.05$ respectively. E. Broom reports (The validity of four individual tests of mental ability, *Educ. Res. Bull., Los Angeles City Schools*, 8, 1929, 9 f.) a correlation of $+0.65\pm.08$ for 22 elementary school children. The lower correlation presented here might be explained on the basis of several factors, but it is not within the scope of this article to do so. It is significant that the correlation is raised when the revised scoring scheme is used, for although the reliability of this positive change is very low, in so far as any change has taken place, it is a favorable one.

[8]The writer has the data on file in the form of tables.

GESTALT *vs.* EXPERIENCE[1]

The influence of experience upon visual perception has been studied by the *Gestalt* psychologists from a new point of view. The results obtained are interesting not only in themselves, but also because of their theoretical implications. Two studies, for example, recently published by Dr. Gottschaldt,[2] lead to the conclusion that spatial form is primary and experience virtually without residual effect.

The material that Dr. Gottschaldt employed in these studies was composed of series of geometrical figures varying in complexity from a simple cross to a rectangular solid. These were the *a*-figures. Every *a*-figure was presented a definite number of times with instructions to note carefully and to name. The number of exposures was determined by the amount of 'experience' desired. Then a second figure, the *b*-figure, was presented. Of the *b*-figure the preceding *a*-figure formed a geometrical part, which might, however, be more or less concealed in the *b*-figure. The criterion of the effectiveness of experience was the number of times the *a*-figure stood out immediately in the *b*-figure. The results show consistently small percentages of cases in which the *a*-figure stood out at first sight; a similarly small percentage of cases in which the *b*-figure was seen first with, shortly, a spontaneous appearance of the *a*-figure; and a marked preponderance—from 50%–90%—of cases in which the *b*-figure alone was seen. When the *b*-figure was presented with instructions to look for the *a*-figure a considerable increase was found in the 'spontaneously appearing' group. The automatic effect of experience, Dr. Gottschaldt holds, is negligible, although the seeking attitude may constitute a "situation-vector" which is favorable to the secondary spontaneous appearance of the *a*-figure.

At first glance these findings seem somewhat unexpected. It is necessary, however, to keep in mind—what Gottschaldt is careful to state—that perception is used here to designate the bare "seeing" of the geometrical figures. Experience, then, has but little influence in the seeing of figures. But the connotation of experience as tested in these researches—with exception of the series dealing with the effect of the time interval between the exposure of the two figures—is limited to frequency of repetition. Gottschaldt finds specifically that whether the observer has seen the *a*-figure only three or more than five hundred times makes no significant difference in the readiness with which it may be seen in *b*-design. To anyone but a Behaviorist of the left wing the restriction of experience to frequency and recency must seem rather arbitrary and the title of these investigations, in consequence, somewhat misleading.

Dr. Gottschaldt is a little at loss to account for the disturbing influence of the situation-vector, more familiar as 'task' or 'attitude,' since his thesis is that he has found a type of configuration that is primary, and as such it should be absolutely resistant to transformation. It might be assumed that in a *Gestalt*

[1]Communicated by W. B. Pillsbury.

[2]Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren: I. Über den Einfluss gehäufter Einprägung von Figuren auf ihre Sichtbarkeit in umfassenden Konfigurationen, *Psychol. Forsch.*, 8, 1926, 261-318; II. Vergleichende Untersuchungen über die Wirkung figuraler Einprägung und den Einfluss spezifischer Geschehensverläufe auf die Auffassung optischer Komplexe, *ibid.*, 12, 1929, 1-87.

theory the situation-vector would be an integral part of the figure-experience, but this view is not set forth. The point is stressed that naïve *Os*, unemployed, do not look for the *a*-figure in the *b*-design; while *Os* wiser in the ways of psychological experiments sometimes inquire spontaneously whether they are to try to find *a* in *b*. Apparently experience may have an effect in determining the situation-vector even in seeing a simple figure. It is questionable whether the position has ever been taken that frequency or recency necessarily overcome the influence of the present situation, and there should be no reason for concern if that should prove to be the case, unless one is committeed to a nativist position. In fact, Dr. Gottschaldt's best evidence of the optical independence of his figures lies in the fact that in spite of instructions to look for the *a*-figure so few of them stood out.

An effect of experience in increasing the readiness with which figures are found is seen in the series conducted to find the relative difficulty of the figures. The average time required to find the *a*-figure decreases with the increase of the number of figures viewed. Dr. Gottschaldt attributed this to the *Os'* learning where to look. He considers it, however,—and rightly too—somewhat removed from the purpose in hand; but it is precisely in learning that the effectiveness of frequency has been emphasized by the Behaviorists to whom he refers.

In any other associative system, frequency is used as an explanatory principle chiefly in learning and in recall. The figures used by Dr. Gottschaldt are simple enough to be recalled after two or three exposures, and he finds in fact an optimum frequency beyond which further exposure is a detriment. In one of his experiments, an opportunity for recall was given in a rather interesting manner. A figure was partially exposed in a dim light and the *O* instructed to reproduce what he saw. The partial exposure might be such as to suggest either the *a*- or the *b*-figure. In all cases the *Os* saw the portion of the figure actually presented, or, if there were variations, there was no consistent tendency to see the *a*-figure. It is entirely possible that at times a part of the *a*-figure may have served to recall it, perhaps in the form of a visual image. That the *Os* failed to draw it need mean nothing more than that they were able to distinguish between what was recalled and the visual stimulus before them.

An interesting result not mentioned by Dr. Gottschaldt is apparent from an inspection of his arrangement of the figures of the first investigation from difficult to easy. Figures proved to be easy, that is they were readily found, if the *b*-figure contained a part of *a* that could be recognized as similar to the first figure. In most cases the original figure was crossed by several lines forming within it smaller geometrical figures, and difficulty increased as the number of such lines destroyed the similarity to the original. This result shows that experience is not wholly without effect even under Dr. Gottschaldt's conditions.[3]

This brings us to the real substance of Dr. Gottschaldt's research. He has demonstrated carefully and quite conclusively that for adult *Os* visual objects

[3]Cf. the statement of G. E. Müller, to which Dr. Gottschaldt's work is in a sense opposed, that "if a given combination of stimuli has frequently resulted in a collective comprehension, there remains a disposition for the like collective comprehension to recur with the return of a like combination of stimuli" (*Komplextheorie und Gestalttheorie*, 1923, 33).

or units are delimited by lines or contours. This holds from the simplest figures, the "triangle with a tail," to figures with a very complex interlacing of lines enclosed within a boundary outline. Naïve *Os* do not go beyond the contours as seen; trained *Os* may attempt to do so with but indifferent success. Two questions remain: (1) whether the contrary has been maintained; and (2) the bearing of the fact upon the nativism-empiricism controversy to which Dr. Gottschaldt refers.

(1) On the first point a quotation from Müller should be noted: "If two contiguous, let us say, rectangular figures, differing in color and brightness and therefore cohering weakly, are presented upon an indifferent gray ground their coherence can be increased notably by surrounding the outside limits of the two rectangles with a line, so that both fields lie within one and the same linear boundary."[4] This is set forth at the outset as one of the factors operative in the *formation* of complexes, which Müller discusses throughout. He does not consider whether a complex may be destroyed or transformed in the same way. Dr. Gottschaldt has shown that it may. Müller doubtless would hold that a line or contour is a primary stimulus to the attention of an adult. To put it in the simplest terms, the eye tends to follow a continuous line. That contour is fundamental in the formation of optical figures is implicit in the conventional treatment of space perception, although it is not given the consideration it perhaps merits. To this refocusing of the problem Dr. Gottschaldt has contributed.

(2) It is possible, then, to agree readily with Dr. Gottschaldt's conclusion that under the conditions of his experiments "experience alone was not adequate to overcome the autochthonous Gestalt factors" if "frequency" be substituted for "experience." It scarcely follows, however, that "of the great original claims of the empiricists very little is left." The empiricist would reply that if lines and contours have gained their figure-forming capacity as the result of frequency, experience has been so infinitely repeated that one could not hope to break it down in the normal adult. The present investigations have nothing to say of the genesis of the autochthonous Gestalt factors in question. Dr. Gottschaldt researches leave the nativism-empiricism question where they found it.

University of Michigan M. G. MOORE

ON THE TERM 'RETROACTIVE INHIBITION'

Professor Joseph Peterson has recently called in question the fitness of the term 'retroactive inhibition' for the phenomenon which it is meant to designate.[1] In commenting upon the writer's paper before the twenty-fourth meeting of the Southern Society for Philosophy and Psychology he says: "The reviewer would certainly interpret these results in another way than by the assumption of retroactive inhibition which somehow acts back on and undoes certain learning fixed in the past. The paper itself did not, it should be made clear, insist

[4] G. E. Müller, *op. cit.*, 33.

[1] J. Peterson, The twenty-fourth annual meeting of the Southern Society for Philosophy and Psychology, this JOURNAL, 41, 1929, 504.

on the sort of effect implied by the term used. But why not discontinue a term that is so objectionable in its implications of a particular interpretation which may be weak?".

These comments voice a dissatisfaction with the term in question which others, also, have expressed, and bring clearly to an issue the need for either a new term or an attempt to state clearly the implications of the old one. The writer's entire agreement with Professor Peterson's view that 'retroactive inhibition' cannot be taken to imply a backward operation which undoes learning has been expressed elsewhere.[2] Certainly the term can refer legitimately only to a particular phenomenon, not to its explanation. The latter is an independent problem for research, which should not be prejudiced by the terminology employed. The use of the term to designate the phenomenon is, however, another matter.

As used in the experimental literature, 'retroactive inhibition' signifies only the fact of poorer retention when some activity, usually a learning activity, has been interpolated between an original learning and the measurement of its retention, than when a period of comparative rest has intervened.[3] The interpolated activity 'retroacts' the original only in a metaphorical sense. It is *as if* the interpolated material acted backward on the original. According to the major theories of the phenomenon, the interpolated material either stops a perseveration of the processes involved in the original learning, with what amounts to a resulting poorer learning thereof than would have occurred under the rest condition, or else the actual items of the interpolated activity become confused with the items of the original, with a resulting diminution in the retention of the latter. Whether either theory is adequate is not here an issue. The point is that neither assumes, and that probably no other theory may be expected to assume, a backward working effect.

If we grant that the phenomenon described is the one indicated by the current words "retroactive inhibition," the question becomes that of the advisability of the use of a somewhat metaphorical term for it. While granting the descriptive inexactness of the designation 'retroactive inhibition,' certain things may be said in its defense. (1) Its major justification is its long-continued use by students of the phenomenon since the early work of Müller and Pilzecker. There is little to be gained, and there is much to be lost, by discarding an historically sanctioned term, if it can be defined accurately and unmistakably. There is small doubt that 'retroactive inhibition' can be so defined. (2) If accurately defined as a phenomenon, with no implication of an explanation, it can lead to no confusion. And (3) there is good warrant in scientific convention for the use of terms much more metaphorical than the one in question. 'Refractory phase,' for example, carries the anthropomorphic implication of disobedience or obstinacy. A muscle fiber in refractory phase is as if it were obstinate, but no physiologist falls into the error of ascribing anthropo-

[2]E. S. Robinson and F. R. Robinson, *Readings in General Psychology*, 1929, 403.

[3]Acceptance of Skaggs' proposal (E. B. Skaggs, The concept of retroactive inhibition, *Psychol. Rev.*, 33, 1926, 237-244) to confine the term to cases of permanent loss of associates, excluding all emotional and affective influence, would not affect the general suitability of the term as such.

morphic obstinacy thereto Nor does a physicist misunderstand the term 'attraction.' If science were to eschew metaphor and adhere to descriptive exactness in its terms, a remaking of some of its fundamental vocabulary would be required.

It may readily be granted that one might, of course, substitute a more descriptive term such as *intercurrent blocking*, but the reasons adduced for the continued use of the term *retroactive inhibition* urge strongly against any substitution.

University of Arkansas JOHN A. MCGEOCH

ON THE OMISSION OF INTERMEDIATE ACTS

In a recent number of the JOURNAL Freiberg, Dallenbach, and Thorndike question the natural dropping out of intermediate steps in the learning of an action series and cite certain observations which seem to merit further comment.[1] The observations are that a count of errors made by hand-compositors shows that they do not make more errors of omission in the intermediate letters than in the case of the other positions. They do find more omissions than additions of letters and they find omissions more common in the case of words relatively little employed. They conclude that "the facts give no support to the doctrine that the frequent repetition of a series . . . . produces a tendency toward the omission of the intermediate terms of the series." In this connection several points seem worth noting.

In the first place, learning to spell involves a deliberate training *against* such a tendency as there may be to drop out middle letters. Hand-compositors have been, through long training and rigorous selection chosen on the basis of this correction. They are selected on the basis of the degree to which this natural tendency has been trained out of them, so far as spelling words is concerned. Hence the non-occurrence of such errors would have no bearing on the existence of such a law, even if the spelling situation were otherwise relevant. Surely the place to observe a tendency is not in a situation from which it has been deliberately excluded by special training and selection.

In the second place, the tendency as it has been stated has been given as a law of *learning*. Even in learning to spell this tendency is clearly operative; both pupil and teacher must struggle against it. A study of the locus of errors in misspelled words of six and seven letters in the case of school children is already on record which shows the facts clearly enough.[2] The greater number of errors occurred in the middle and post-middle parts of words, and "The children tended to make their misspellings too short rather than too long." Training consisted in part in compelling them to overcome this tendency, so that they might become adequate hand-compositors and serve as subjects for Freiberg, Dallenbach, and Thorndike.

In the third place it is doubtful if the mere act of spelling a word is really relevant to the problem. The law of "omission of intermediate terms" describes

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[1] A. D. Freiberg, K. M. Dallenbach, and E. L. Thorndike, The influence of repetition of a series upon the omission of its intermediate terms, this JOURNAL, 41, 1929, 637-639.

[2] Leta S. Hollingsworth, Psychology of special disability in spelling, *Teachers College Contributions*, No. 88, 1918.

what happens when an irritant leads to a behavior series which eliminates that irritant, thus putting an end to that particular activity segment, through the effect of the terminal act. Questions of the validity of the law should therefore be raised in situations to which the law is supposed to apply. It is at least far-fetched to consider the first letter of a word to be the motive, the middle letters to be unsuccessful responses, and the last letter a terminal adjustment that removes or alleviates the first letter.

So long as the public insists that hand-compositors include all the letters of the words they spell, typographic errors made by such professional spellers, however interesting and instructive in other respects, are about the poorest place to find exhibited the laws of learning. In such a situation the laws may occasionally be illustrated, but only against great odds. This is in fact what Freiberg, Dallenbach, and Thorndike found. For there were really somewhat more omissions than interpolations, and they were more frequent in rare words, in the case of which the tendency had not yet been sufficiently trained out.

There is of course some interest in looking for a thing in places from which it has first been carefully removed. Any appearance of the thing under such circumstances will indicate how very persistent it is. Negative results will of course be wholly meaningless, but any positive indication, however slight, will be good evidence that the thing was really a fact and had not been wholly removed. It is just such shreds of positive evidence that are shown by the actual results of this careful counting of the errors of hand-compositors.

Barnard College, Columbia University H. L. Hollingworth

A Reply to Dr. Hollingworth

Through the courtesy of Dr. Hollingworth I have the privilege of commenting in this number of the Journal upon his Note.

In order to test the 'law' that "the frequent occurrence of a series of physiological events will tend to cause the omission of one or more of the intermediate terms," we examined the errors made by the compositors in setting 1068 galleys of this Journal. We assumed that the action of a compositor in setting a word was a typical case under this 'law;' and that consequently omissions of letters (a) would be more common than additions, (b) would occur more frequently at intermediate than at end positions, and (c) would occur more often in common words than in words rarely set. Much to our surprise none of these expectations was realized: (a) omissions and additions occurred with approximately equal frequency; (b) omissions at the end positions were about 5 times as frequent per opportunity as in intermediate positions; and (c) omissions occurred relatively more often in rare than in common words. We were, therefore, forced to the conclusion that "the facts give no support to the doctrine that the frequent repetition of a series of events in the mind or body in and of itself produces a tendency toward the omission of the intermediate terms of the series."

Dr. Hollingworth questions our conclusions on three grounds. (1) His first point is that the operation of the 'law' in activities like typesetting is counteracted by special training against the dropping out of intermediate terms. Compositors, according to Dr. Hollingworth, "are selected on the basis

of the degree to which this natural tendency has been trained out of them." But why, we ask, should the training that a compositor receives counteract the dropping out of intermediate terms more than it counteracts the omission of end terms, or the addition of terms? One kind of error is as grievous as another. If training is equally efficacious against all kinds of error, then activities like typesetting would fail to show the operation of the 'law' only when training had removed errors entirely. Our results show, however, that errors continually and frequently occur—as every one knows who has printers-proof to read. Training and selection reduce the number of errors, but do not eliminate them. Why, therefore, should not the 'law' be evident in such errors as do occur?

(2) Dr. Hollingworth's second point is that "the tendency as it has been stated has been given as a law of *learning*." Not all the proponents of the 'law' will agree with him. Jennings, for example, states that "the operations of this law are, of course, seen on a vast scale in higher organisms in the phenomena which we commonly call memory, association, habit formation and learning."[1]

(3) Dr. Hollingworth's third point is that 'the mere act of spelling a word is not relevant to the problem.' This statement stands in direct opposition to the argument he advanced in discussing his second point. He says there, "Even in learning to spell this tendency is clearly operative." How could Dr. Hollingworth discover that the tendency was operative in learning to spell except by the act of spelling? Dr. Hollingworth implies in one place that it is operative and states definitely in another place that it is not. It is immaterial to the present discussion, however, what he finally decides, because the "mere act of spelling" has very little to do with the compositor's task. The compositor is taught to follow copy, not to spell; if he learns to spell in the process of becoming a compositor, that is a mere by-product. He presses keys—an ordered series of events—in response to the copy which he endeavors faithfully to follow. How faithfully he follows copy may be seen in the fact that words misspelled in the MS—even common words—will often be misspelled in the galley proof. The compositor understands, as a rule, very vaguely and very partially the meaning of what he is setting. Words are set as wholes, as a series of physiological and psychological events, and not as discrete letters. Why, therefore, should not the compositor's activity fall within the scope of the 'law' that Dr. Hollingworth elects to defend?

Cornell University KARL M. DALLENBACH

THE TWENTY-FIFTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The twenty-fifth meeting of the Southern Society for Philosophy and Psychology was held in the Jesup Psychological Laboratory, Peabody College, and Vanderbilt University, Nashville, Tennessee, on April 18-19, 1930. The morning program on the first day, in the former place named, was largely experi-

[1] H. S. Jennings, *Behavior of the Lower Organisms*, 1906, 291.

mental. It began with a paper by Noel B. Cuff (Eastern Kentucky State Teachers' College) who reported further results on his studies of 'eyedness' and 'handedness.' Tests were made with the manoptometer (an improvement on Parson's manoptoscope) which yielded reliabilities of 0.98, 0.98 and 0.97 on children in grades 1, 2, and 3, respectively. Two hundred thirty-seven children in the grades and 109 college students had been given the tests. In addition to these tests, other data on the use of the hands in various physical performances were included as well as of unilateral winking. *Ss* who performed four activities—writing, throwing, easy reaching, and energetic reaching—with right hand were designated as right-handed. Eight different manual activities, however, were used in the analysis of 'handedness,' and were correlated with 'eyedness.' Results indicated that 'eyedness' is not a character which can be described in general terms such as right-eyed or left-eyed, certain unilateral preferences being much stronger for one individual than for another. Two kinds of 'eyedness' found in addition to these two main divisions are the unstable eyed and equal eyed. Similar results were reported for 'handedness' and a general relation was found between the individuals roughly classed as right-handed or left-handed. Numerous exceptions on detailed activities were found. The manoptometer indicated that 20%–30% of the children were probably left-handed natively and all the left-handed were left-eyed except one case, also that 100% of the right-eyed children were found to be right-handed. Ninety-four percent of the children and 93% of the college students studied are now right-handed. The correlations between 'eyedness' and 'handedness' scores range from 0.03 to 0.40, with but one exception. The imperfections of the tests for 'handedness' were pointed out in the discussion and freely admitted by the author.

Katherine Vickery (Alabama College) discussed the results of a research on the effect of change on work decrement. The experiment began with 80 *Ss* divided into four equated groups who worked through periods of 20 min. per day for 5 days on cancellation and on the J. C. Peterson equation test. This latter is made up of equations into which all the signs but that of equality have to be supplied by the subjects. One group worked continuously on the cancellation test, one on the equation test and the other two groups, beginning respectively on the cancellation and the equation test, changed work from one test to the other every 5 minutes. No work decrement appeared from continuous work on the cancellation test but a decided decrement resulted from the continuous work on the equation test. Change of work reduced the decrement resulting from continuous work on the equation test but the evidence of a similar effect on cancellation was lacking. There seems to be a probability that change of work (closely related to the kinds of work here used, at least) is effective in increasing efficiency in the case of the more complex types of mental activity, whereas the simpler types do not show this effect of change. Unfortunately the number of *Ss* was considerably reduced before the experiment was completed.

Howard Easley (Peabody College) reported an investigation in which he had endeavored to subject to a critical experimental and statistical analysis certain methods of measuring 'attention,' the underlying assumptions of which

he held to have been taken too much for granted. The attention tests used were: (1) the cancellation of *a*'s, by Thorndike; (2) the Münsterberg test, which consists of underlining on a page of mixed letters those adjacent letters which spell words; and (3) Woodrow's attention test. The following tests, other than the specific attention tests, were also used: Alpha, Otis S-A, Peterson Uniform Intelligence, digit-symbol, and word building. The average scores on ten tests in general psychology were also used. The *S*s were 68 undergraduates in general psychology. All of the correlations involving the attention tests were too low to be reliable, 5 of the entire 21 being negative. The empirical median of all the tetrad differences yielded by the table of correlations was 0.042, and the theoretical P.E. of the tetrad differences, 0.041. The empirical median of the tetrad differences involving one or more correlations between attention tests was 0.034. These results are quite within the limits of normal expectancy, and, according to Spearman's theory, indicate the absence of any such group factor as attention. The lack of correlation between pairs of the attention tests themselves and between the attention tests and the other tests, and also the low tetrad differences, indicate either that the attention tests do not measure attention, or that the attention involved in one function is not the same as that involved in another. Requests, in the discussion, for a definition of attention as used in the experiment, brought out the reply from Easley that he was not interested in definitions but rather in evaluating the tests as given by the authors.

S. C. Garrison (Peabody College) reported results of retesting adults after an interval of ten years. A number of college students were given the Yerkes-Bridges-Hardwick Point Scale during the year 1916-1917. Of this number 73 (32 men and 41 women) were retested during 1926-1930. The ages at the time of the retest ranged from 18 to 49 years. It was found that the more intelligent women and the less intelligent men persisted in summer work for a long period of time. Coefficients of variability were much larger in the second testing. The Point Scale has a reliability of 0.81 for college students tested at an interval of two weeks. For the interval of ten years this was found to be for the men 0.58 and for the women 0.76.

Lyle H. Lanier (Vanderbilt) reviewed critically, in anticipation of an experiment on himself, Henry Head's theory of cutaneous sensibility and all experimental studies of the effects of nerve section upon residual sensibility and the recovery of sensibility. Trotter and Davies, Boring, and Sharpey-Schafer repeated Head's experiment and the results of all three studies have been held to contradict Head's hypothesis, although all of them reported essentially the same facts from which Head deduced the existence of the deep sensibility, the protopathic and the epicritic mechanisms. Lanier concluded that Head's functional theory is in reality well substantiated by the clinical and experimental facts. Furthermore, Ranson has shown that the myelinated and the unmyelinated fibers which exist in cutaneous nerves probably subserve different sensory functions, the second group being the more primitive phylogenetically. This is precisely the sort of mechanism which Head's theory would require. The interpretation of protopathic hyper-sensitivity as due to release from the epicritic mechanism, it was contended, is in harmony with other known facts

concerning nervous function, especially the 'over-reaction' of thalamic centers when cortical connections are severed. The view that highly specialized sensory discriminations have special invariable anatomical correlates seems no longer justified. It is fruitless to look for a given pathway for a given 'sensory quality.' The apparently simple sensory reaction is probably a complex physiological achievement, a product of successive integrations at higher and higher levels of the nervous system, not a simple invariable result of stimulating a specialized receptor followed by conduction to a specific cortical locus. The work of Sherrington and others has made this kind of concept familiar and natural as far as motor activities go, but on the sensory side the 'constancy hypothesis' has dominated us. Head's work on the nature of sensory functions in general was found commendable in view of tendencies to over-simplify the neurological foundations of behavior. The discussion added little except that Waterton's investigation was cited as supporting part of the author's conclusions.

J. C. Barnes (Maryville College) sent a paper on uses and results of freshmen psychology tests, which was read by title. It reports a study based on questionaries sent to a large number of institutions, inquiring about the use of tests in the administration of colleges and universities. There is no statement as to the number or the identity of institutions to which the questionary was addressed, of the percentage of the replies received, or of any numerical results obtained; only general quotations and conclusions which are, of course, of questionable validity are given. Nothing new appears in the report on attempts at prediction of college success by tests in Maryville College, referred to in the paper.

The afternoon program, at Vanderbilt University, opened with a paper by Anna Forbes Liddell (Florida State College for Women) on "Challenging the Major Premise." She apparently regards philosophy as having a rather large supervisory function over science, and suggested that in meeting science, philosophy might adopt the method sometimes used in debate of challenging the major premise. She showed signs of lack of acquaintance with the spirit of advanced scientific work today. "Every special science is developed from a fundamental hypothesis—tentative, but while held, held fast. The pure scientist of today assumes the objective reality of the physical universe even as the medieval theologian assumed the objective reliability of divine truth," but it has also its negative limitations of being exclusive, intolerant. "Genuine philosophy," she asserted, "is never dogmatic, for philosophy is the quest for truth, not the dissemination of knowledge. When speculation results in formulated hypotheses, then a science is born of philosophy," etc. She represented philosophy as constantly growing in "the infinite struggle toward absolute truth." "There is no orthodox philosophy." She apparently subscribes to the position that science must give way to philosophy whenever hypotheses are to be suggested or old points of view fail, and that science merely tests the hypotheses thus furnished it, and "disseminates knowledge." How this view among certain philosophers, which we find nowadays appearing in the discussions of the psychology of learning and in their consideration of the rôle of philosophy in scientific advance, as in this paper, arose and received its justification among these individuals is an interesting question. How often

has philosophy set such definite experimentally verifiable hypotheses to science? Why can a scientist obtaining certain results even from his own hypotheses not set himself the further problem of determining experimentally how such results will affect man's health, or social culture, or religion, or his forms of government? On the other hand, how are we to get answers to these questions other than by testing experimentally the questions raised? Scientists can well resent, in the light of history, the charge, now so frequent, that the setting up of an hypothesis is not part of real scientific research, and even the charge that science has nothing to do with morality. It may well be questioned whether these unverified assertions by certain philosophers are not made to justify their own existence in a day when science has turned to its own methods with the excellent results known to everyone.

A. S. Edwards (Georgia) outlined a laboratory course of experiments and problems in social psychology. Where actual experimental attack is yet impossible, problems solved by applying laboratory methods to the gathering and organizing of data may be taken up. Instructions for 55 experiments and problems have been outlined, and 42 experiments and problems are suggested for further work. From these 97 experiments and problems, it was suggested that at least 30 might make the basis for a laboratory course in social psychology. A mimeographed list of the problems was offered, but not the directions and instructions, so a critical evaluation is not possible here. Several of the experiments and problems are suggestive, some are well known and used, and a few duplicate experiments usually given in other courses. One is impressed by a certain lack of organization in the list. It is, of course, desirable to put social psychology on a scientific basis.

J. U. Yarborough (Southern Methodist University), in a paper on the academic background as a basis for predicting success in college, presented data on the student's family in the immediately preceding generation as affecting his own college success. Success in college was measured in terms of scores in the psychological examination prepared by the American Council of Education as well as by actual grades earned, and the relationship for this success was traced particularly to the number of the immediately preceding generation (father, mother, uncles, and aunts) who attended college. This 'academic background' of 2,102 freshmen yielded the following conclusions: that only 4.2% of the student body had an 'all college' background (father, mother, uncles, and aunts who are college graduates); that 8.3% have no 'academic background;' that the former group made better scores than the latter; that a member of the non-college group stands a three to one chance of falling in the lowest instead of the highest ten percentile of his class, whereas for the all-college group there is practically a three to one chance of being in the highest ten percentile; that the average grade earned by the all-college group is 'B' as compared with 'D' for the other group, and that the 'mortality rate' of the all-college group is slightly less than one-half that of the comparable group. Certainly one would doubt from these figures the objectivity of assigning school grades in the institution considered, and one may even question whether the better culture and frankness of the all-college individuals did not appreciably influence the assigning of grades. These data should be checked against other

similar data from various institutions in different environments, and high school environment should be considered, if we are to ascertain roughly the extent of the influence of hereditary factors in determining college success, also more quantitative data for statistical handling are desirable.

H. C. Sanborn (Vanderbilt) followed with a paper considering the question of the inheritance of song in birds. He pointed out that the early experiments of Barrington in the eighteenth century indicate strongly the non-inheritance of the songs of birds and their acquirement through 'imitation.' These results received support through the later work of Witchell, Scott, and Conradi. Sanborn, however, reports that his own experiments, carried on since the summer of 1922, show that the songs of Baltimore orioles, Kentucky cardinals, bluebirds, robins, brown thrashers, indigo buntings, goldfinches, English sparrows, as well as the typical hooting of screech owls, are in fact determined by inheritance. On the basis of the previous results and those reported originally in this paper, the author "suggests the tentative hypothesis that there are mimics in all species of birds." The innate characters for song production in most species, however, are not as specific as in such mimics as the Indian shama thrush, the mocking-bird, and the European bullfinch. He suggested that the earlier experiments perhaps had to do with less specific mimics in the species observed. In most species the general characteristics of the song are probably conditioned by innate factors, though they are often considerably modified through individual differences and imitation. Sanborn reported experiments of surrounding certain of his young birds by so many birds of different species that there would be little possibility of influence by one type of song only. In such cases the young birds adhered to the pattern characteristic of their own species. The work by Sanborn is not yet completed. In the discussion, the question arose as to whether the songs of the numerous other species of birds might not simply confuse the young birds, but Sanborn held that this was the object of his experiment. If the birds were thus confused it would be more natural, he held, to suppose that their own song would be that which is natively determined. There was also some discussion of 'imitation' as an adequate explanation of modification in song.

Joseph Peterson (Peabody College), in reporting re-calculations of group comparisons in learning, pointed out the fact that the standard equations used in determining reliability of such comparisons were worked out on the tacit assumption that all measures considered are true measures. As a matter of fact, obtained measures are only approximative to the true ones, their divergence from the latter depending upon many factors. It is obvious that no difference between means of very large groups can appear if the reliability of the measure used is zero. Corrective formulas have been worked out by Kelley and Tryon. In case of low reliability, corrections are, of course, poor substitutes for more reliable scores. Peterson reported that when allowance is made for these unreliabilities, the differences between central tendencies in groups of white and negro children of ages 8, 9, 10, and 12 yr., respectively, on the Rational Learning problem, show a marked consistency throughout as compared with uncorrected differences. Such corrected data show that a few differences formerly reported as unreliable, are really reliable, that there is a consistent

decrease in the size of differences between groups as ages advance from 8 to 12 years, and that some of the differences in the 12-yr.-old groups in different studies are really very small and unreliable. In all cases children in both races were selected according to age, irrespective of grade classifications. These results are contradictory to Arlitt's results (*J. Applied Psychol.*, 6, 1922, 378-384) and to the view often met that negroes fall more and more behind the whites in intelligence with the advance of years. Is this convergence of means with advance of years, within the limits here cited, due to more rapidly increasing growth of abilities in negroes with increase of age up to 12 years, or to a more rigid elimination of the dull among the negroes? We do not know. It is true that the elimination from school among the negroes is very great, but that is not due to any single thing, innate or environmental.

The presidential address by Max Meyer, given at the Andrew Jackson Hotel, just after the banquet, dealt with the commotion stirred up in Missouri by the questionary on sex, now well known to psychologists. Since this address will be published in full, there is no need of making any statement of the contents here, especially as any such attempt would be inadequate in a few lines and probably would be unfair to one or the other of the parties concerned. The results of the investigation made by the American Association of University Professors are known to the informed reader.

In the first paper Saturday morning, in Vanderbilt University, J. B. Miner (Kentucky) presented briefly the history of the Society under the title, "Our Twenty-Fifth Anniversary." The early part of this history was taken from records by the founder of the society, Edward Franklin Buchner, sent to Miner in 1929 soon after Buchner's death. They show that as early as February 8, 1904, Buchner, while professor in the University of Alabama, wrote to a number of leading psychologists for advice as to the inauguration of a Southern organization and its possible affiliation with national organizations. The American Psychological Association was then 12 years old, the Mid-Western 2 years, and the American Philosophical Association was in its 4th year. In these letters a statement was made to the effect that preliminary consultations in the South indicated an "awakening interest." In a preliminary conference held at the meeting of the Department of Superintendents of the N. E. A. at Atlanta, February 23, 1904, J. M. Baldwin was elected the first president. Buchner's minutes and the news note in the *Psychological Bulletin* state that the Society was organized at this date, and the first printed program announced the "First Annual Meeting" of the Society at Johns Hopkins University on December 27, 1904. So the present meeting (1930) actually completes the first 26 years of the Society's existence, but it is probably the 25th meeting, since no meeting seems to have been held in 1918. The nine men present at this first gathering represented various lines of interest as the following positions show: Buchner (Alabama—philos. and educ.), Baldwin (Johns Hopkins—philos. and psychol.), B. B. Breese (Tennessee—psychol. and ethics), H. E. Brierly (Florida State College—exper. psychol. and biology), A. C. Ellis (Texas—educ.), A. R. Hill (Missouri—dean of Teacher's College), R. P. Halleck (Louisville—prin. of the Boy's H. S.), W. Rose (Tennessee—philos. and educ.), and W. T. Harris (U.S. Com. of Education.) In December, 1904, there were 36

charter members; there are now 82, most of whom are psychologists, so far as the writer's own observation goes. It may be noted here that there are now over 400 'higher' educational institutions in the Southern states, indicating even yet very little interest on the part of most teachers of psychology and philosophy, though some members and institutions are now, fortunately, really alive to their opportunities. Only one state university in the South has now no qualified psychologist. Miner sketched briefly certain advances in the nature of the psychological work in the South and in attempts to improve the training of teachers in psychology, and he showed that in 1915 a suggestion to include in the Southern Society also experimental education was defeated. The writer can hardly agree with the point taken that the Association of philosophers and psychologists is still to be desired; unless separate sessions are held for these two groups of workers, whose methods, as psychology is becoming more experimental, are diverging correspondingly. It might be added that much of the so-called experimental education still leaks into the program, unfortunately, in some rather superficial papers on testing, personnel work, etc.

H. M. Johnson (Mellon Institute of Industrial Research, Pittsburgh) discussed the question, "Can Religion Blend with Modern Science?" He assumed that both science and religion are built upon certain basic axioms which serve as rules of description and also as rules of procedure in each case. A certain variety of such axioms in one case would give a scientific treatment; a different variety, a religious treatment; etc. Proceeding on one combination of axioms, we arrive at certain results which may be very different from and contradictory to those based on another variety. The axioms themselves are not subject to any tests as to truth. In themselves they are neither false nor true. The axioms of modern science are contradicted by conventional religion. To deny those in science would destroy scientific endeavor, and to deny those in religion would be equally fatal there. In science some such axioms are the doctrine of physical determinism, the acceptance of the validity of knowledge only as verifiable to anyone by experiment and observation, etc. Religion accepts a kind of knowledge which contradicts these scientific axioms. This 'knowledge' or mystic factor is based for religion on claims to intuition or personal insight, private revelation, infallibility of a prophet or leader, a collection of writings, or a priesthood. The point was taken that science and religion can therefore not blend and that they become more divergent with greater progress. Any person might, it was admitted, though this may be questioned, act now on the postulates of science and now on those of religion, but he could not act on both simultaneously. The discussion brought out from Graham the view that religion nowadays has a tendency to be interpreted in terms of social values and not of axioms regarding the transcendental. Dunlap held the view of the paper to be a narrow one of religion, though he admitted that it might be maintained if one chose to do so. It may be said that genetic studies of religions show them to arise, like various other attitudes and sentiments, not from rationally accepted axioms, but from the larger economic factors in the lives of groups of people. The paper under discussion somewhat confounded religion with dogma and theology, which, of course, are later rational products of conflicts in religious practices. It may be added that nowadays 'physical determinism' is

receiving considerable discussion in science, particularly as to the interpretation of the concept

J. L. Graham (Kentucky) reported on conditions which aid the subject in discovering relationships in certain rational learning processes. In the Rational Learning test it has been found desirable to use fore-exercises with only brief directions rather than merely the printed instructions at first used for adults. Graham gave this learning test individually to two groups of college students—Group A (100 *Ss*), who learned the 10-letter problem after reading the original instructions, and Group B (14 *Ss*) who, after going through a 5-letter fore-exercise problem, learned successively problems involving 4, 6, 8, 9, 10, 12, 15, 16, 18, and 20 letters. Results showed for Group B an enormous and reliable advantage, so far as efficiency in learning is concerned. (There was no attempt to evaluate the two methods as to diagnostic or predictive values.) These *Ss* after taking the fore-exercise, solved new problems of 4, 6, 8, 9 and 10 letters, respectively, with a great saving of time, repetitions, and errors. The difference in the three medians of the two groups were, for the factors named and in the same order, 3.0, 2.5, and 1.5 Q_B, and all were reliable statistically. Approaching the problem through a succession of solutions of very simple up to complex problems aided greatly in the organization of attacks on, and in the analysis of difficult aspects of, the problem. Both the Disk Transfer problem (given individually), which has also been widely used in comparative studies of negroes and whites, and the Related Problems (Bean) test, developed by Graham as a group form of the problem first used by J. C. Peterson (*Psychol. Monog.*, 28, 1920, (no. 129), 1-131) revealed similar advantages in actual attacks by each *S* on a series of learning problems advancing from simple to very complex forms, again, however, applying only to efficiency in learning. Encouraging the use of abstract and general terms through the succession of problems facilitated the discovery of helpful principles. This work should be continued with probably better objectification of aids in the methods used by the B group, and with a view to increasing reliability and of improving the diagnostic and predictive values.

Paul L. Boynton and Keh-ching Chen (Kentucky) sent a paper on a study of variation of 120 freshmen college students through a series of 13 different measures (tests, school marks, etc.), the scores of which had been converted into standard scores. The average freshman was found to vary as much as 56% of the average group range on a combination of the 13 measures, "and by as much as 147% of the difference between the average inferior and the average superior individual in the various measures. The 78 inter-alienation coefficients varied from 0.527 to 0.999, the median being 0.916. These findings, which they regard as important in education and vocational work, need to be considered further in relation to the well-known statistical facts that reliability measures of the distributions of individuals are much larger and more 'unreliable' than those of central tendencies and deviations of groups. Moreover, the unreliabilities of the various measures used probably vary considerably, thus, giving P.E. or σ values correspondingly exaggerated as compared with their true values.

Charles A. S. Dwight (Keuka Park, New York,) read a paper on the function of philosophy in the life of man. emphasizing the fact that philosophy aims to make one "sure, secure, and serene;" that it makes one free because he becomes master of himself. There was nothing technical or new in the paper. The serenity of many philosphers as compared, say, with other well-trained men is not always striking. But when the author also holds that philosophy "inspires action" he seems to contradict himself, and to show a desire merely to name a number of good traits, rather than to arrive at conclusions from a study of actual philosphers.

The first item on the program of the last session, held in the Jesup laboratory, was a series of moving pictures by Knight Dunlap (Johns Hopkins) showing compensatory head-movements in fowls. Some of these pictures were in slow movement. The compensatory movements of the head in chickens and starlings were shown to be quite different in nature from those of the duck, and to be due to visual stimulation. The relation of nictitation to head movement was shown. The apparent back-and-forth movement of the head of the chicken, pigeon, and starling was found to be an illusion, the head really moving forward only, periodically stopping each time for the body to catch up.

The first paper, by P. F. Finner (Florida State College for Women), considered measurements obtained in laboratory studies of a variety of 'capacities' of individuals, expressed in two forms—the mean and the σ. The usual individual differences in both of these measures were indicated as well as correlations with certain practical and complex activities. It was reported that speed of tapping and reaction-time were only slightly related to intellectual capacity; that reaction-time decreases with age between 6 and 15 yrs.; that short reaction-time is definitely related to rapid work in typewriting and to efficiency in certain athletic activities. A positive relation of variability of response to intellectual capacity was found. Academic success seemed to be independent of steadiness scores and weight limens but the latter two correlated positively with certain athletic performances. The usual low correlations between anthropological measures and intelligence were found. In the absence of the relevant reliability measures of these various relationships it is impossible here to evaluate the conclusions arrived at, but most of them are not unusual.

As the final contribution to this two-day meeting of the Society H. J. Peterson (Hattiesburg State Teachers College, Mississippi) reported, by means of demonstrations, a new and objective device by which individuals can check their own results in multiple choice learning of various sorts, leaving objective records of their errors which make them immediately aware of their successes so that they can pass to the next item in any series of problems or tests. Mechanical and chemical forms of the device were shown. He emphasized the use of the device in various psychological training processes and researches as well as in education and indicated its unique merits in improving extension work, putting it on an objective basis of much value to the student who can know as he proceeds when his answers are correct. The low cost of the device and its application to group teaching or testing was pointed out.

At the business meeting the following officers were elected: President, Ethel Bowman, Goucher College; members of the Council, Max Meyer (Mis-

souri) as retiring president, H. M. Johnson (Pittsburgh) and Roy M. Dorcus (Johns Hopkins). Twelve or thirteen states were represented in the membership present. On the invitation extended by the University of Virginia, the Society voted to hold its next annual meeting there on the Friday and Saturday just before Easter.

Peabody College JOSEPH PETERSON

ITHACA MEETING OF THE EXPERIMENTAL PSYCHOLOGISTS

The 1930 meeting of the Experimental Psychologists, organized at Princeton University during the spring of 1929, was held under the chairmanship of Professor Madison Bentley at Cornell University on April 17-19. After a business meeting, the experimental researches of the various laboratories represented were informally reported and discussed.

The following members attended the meetings: Madison Bentley, *Cornell;* E. G. Boring, *Harvard;* E. A. Bott, *Toronto;* Warner Brown, *California;* K. M. Dallenbach, *Cornell;* S. W. Fernberger, *Pennsylvania;* W. S. Hunter, *Clark;* Kurt Koffka, *Smith;* H. S. Langfeld, *Princeton;* K. S. Lashley, *Chicago;* Walter Miles, *Stanford;* E. S. Robinson, *Yale;* H. C. Warren, *Princeton;* M. F. Washburn, *Vassar;* H. P. Weld, *Cornell;* and R. S. Woodworth, *Columbia.*

The 1931 meeting of the Society will be held under the chairmanship of Professor M. F. Washburn at Vassar College.

K. M. D.

A CORRECTION

A clerical error led me, in the last number of the JOURNAL, falsely to accuse the *Psychological Register* of omitting the name of William Brown. I was wrong and am sorry.

My correspondence with the editor of the *Register*, Professor Murchison, reveals the fact that he welcomes constructive criticisms of the *Register*, and is already laying the foundations for a larger and more accurate second edition to come from the press just before the next international Congress. I am sure he would appreciate from readers of the JOURNAL suggestions concerning omissions or possible improvements in the *Register*.

E. G. B.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Mind. By J. S. HUXLEY, R. J. S. MCDOWALL, F. A. P. AVELING, J. A. HADFIELD, F. A. LINDEMANN, W. R. MATTHEWS, J. D. WILSON, R. G. COLLINGWOOD, C. G. SELIGMANN, and L. T. HOBHOUSE. With a preface by Ernest Barker. Edited by R. J. S. McDowall. London, Longmans, Green & Co., 1927. Pp. xvi, 316.

The book before me is a symposium on the subject of the mind as conceived by eminent authorities in the fields of biology, physiology, psychology, psychotherapy, physics, philosophy, education, aesthetics, anthropology, and sociology. It is a noteworthy achievement which ought to receive the widest possible attention among professional men the world over, comprising as it does a series of lectures given at Kings College, in the University of London, for the most part by members of the faculty with a few additions from the University of Oxford. The tenor of the book throughout is even and in very few instances are there radical differences of opinion. Each man seems to do his part in filling in the canvas without noticeable overlapping or distinct traces of the boundary which marks off his particular patch.

Huxley begins by noting the distinct place which mental phenomena have in the animal series. He is not at all inclined to interpret mind from the material point of view, but marks off transitional stages between matter and mind through the intermediate process which he should like to call 'mentoid', but prefers to call 'mind-like'. He points out that if matter should be given a mental connotation, then the term matter would no longer retain its usual meaning. He sketches the development of the brain through the lower animal series and describes a functional complexity of action from the reflex level to instinct. The increasing importance of consciousness, as such, is then traced through to the highest type of brain-structure which makes possible the framing of general concepts. This gives man the sole power of permanently continued growth in so far as he can anticipate the future and adapt himself to what Lloyd Morgan calls "prospective reference."

A lecture by McDowall follows with a searching discussion of the relationship between mind and body. He starts out by showing the dependence of mind on the brain and its various subdivisions. There is a very simple exposition of such topics as the human nervous system, speech, personality, voluntary movement, attention, originality and deduction, consciousness and the higher psychical activities including religion and ideals. The main contention throughout is that not all so-called 'thoughts' are consciously developed, and that consciousness, although an arbitrary term, is absolutely dependent upon the oxygen supply of the brain. Man is not only a creator of his environment in terms of the body through which he receives outside stimuli; but he becomes master of the environment through the type of nervous system, including the

brain, which he has inherited and which thus brings to him stimuli from earlier environments.

The psychological contribution comes from Aveling. Here we have a conservative and concise presentation largely from the point of view of introspectively analyzed conscious experience. The chief stress is cognitive. This phase of mind is traced from the lower organisms where is found a beginning of consciousness termed 'sciousness.' "*The method* of psychology is, in the first place, introspection, pure and simple. . . . the *subject matter* of psychology is for you your known experience, and for me mine." Much reliance is placed upon Cartesian dualism and on the general idealism represented by Anaxagoras, who posited a mental order throughout the universe. This brings us to three fundamental principles: (1) "The immediate awareness of the active self cognizing mental objects; and (2) the relatedness of it to its objects and of these to one another." Particularly, the causal and purposive relations in the mental world are stressed. (3) "We tend to evoke immediately from any mental item thought, together with any relation applied to it, the thought correlate item." This is called the creative aspect of mind. He rejects, therefore, the notion of mind as a group of conscious experiences and offers us a definition which includes the *experiencer* plus his *experiences.*

This active aspect of personalized mind carries us over to the lecture on psychotherapy given by Hadfield, who continues the dynamic and 'hormic' view. He illustrates this concept through abnormal situations where both the physiological and psychological factors are described especially in their causal relationship to the diseases aroused. His reaction to behaviorism is twofold: behaviorism is neither new to psychology nor does it tell the whole story. He concedes that "the psychologist may hope some day, by his histological methods, to observe consciousness under his microscope, and we may look down upon slides in which irascibility is stained purple, envy stained green, and love stained, let us say, pink." But he rightly asks, "Where in the organism can consciousness be found?" In his definition of mind, however, he goes beyond the scope of consciousness. Its influences, dynamic in nature, are not limited to the process present in any state of consciousness. This mind is governed by its own laws, and has developed at the present stage of evolution to a state of independence.

A remarkable interpretation of mind comes from Lindemann, the representative of physics. On the first page he strikes the key note when he says concerning physical laws, that "there is no certainty that the indefinables employed in their statement, or the particular relations they embody, have any fundamental significance beyond their appeal to the mental preferences, prejudices or infirmities of the physicist." It is very probably symptomatic of the present state in which physics finds itself when this scientist claims that all the various laws of physics are, of course, stated in terms convenient to the human mind. Even the law of causality is termed a mental habit. Throughout this excellent lecture the "almost pathological craving of the mind for constancy in time' is balanced by the more wholesome attitude found "in its mild fondness for constancy in space, as evidenced, for instance, by the concept of a regular solid."

The contribution from philosophy is offered by the Rev. Matthews, dean of King's College. Quite rightly he has taken his stand against the supposition that "consciousness is a kind of phosphorescence on the brain." Nor does he subscribe to the notion that experience is a change in the brain. He refers to the theory of emergent evolution and leans ultimately toward an idealistic conception, but he is not willing to agree either with Bradley or Gentile that there exists an impersonal Absolute apart from the collection of individual minds.

The essay on education by Wilson is enlightening in the way in which education is made a social lever for the continued program of mankind conditioned on the unusual plasticity of the nervous system in the human being and on its life-long receptivity. It closes with a very impressive metaphor in which the child is decreed to be the saint worshipped by all mankind. In the child's educational development truth, beauty, and holiness are emphasized and the necessary inclusion of an art that is practised rather than preached is recognized.

Collingwood of Oxford makes a strong showing in favor of the fine arts as a mental discipline which has no intrinsic utility but which nevertheless fosters a healthy growth of the imagination. This development finds its outlet not only in the fine arts themselves but in even the most prosaic work of the day or in the rigorous thinking of science. The chief function of aesthetic is to prevent imagination from running riot into abnormal conditions and to provide for a healthy outlook.

The anthropological aspect of mind is furnished by Seligman who studiously describes a great many types of primitive minds and some racial types of today. He seeks throughout to classify most of these types in accordance with the extravertive and introvertive manner of response. He makes out a distinct case for primitive peoples as predominantly extravertive while certain present day races show inclinations in either one or the other direction. Nordics and Chinese are definitely introvertive; Mediterraneans and Japanese extravertive.

The last lecture, given by Hobhouse, shows the influence of the human mind in the effects which it receives from social contacts. Society is progressively benefited by the continuous growth of mind. While he does not favor a 'social mind' as such, he thinks it is safe to generalize on certain needs or group characteristics. For this purpose he would assign to whole groups descriptive adjectives which would not apply to the individuals who appear as units in this group; but the social group also has a way of developing the minds of the individuals, so that the whole situation is really reciprocal in effect.

The reviewer has rarely experienced so much satisfaction in reading a collection of essays in book form as was occasioned by this volume. There is considerable evidence that many of the lecturers wrote their papers with preceding lectures in mind. The style in most cases is admirably clear and effective and with few exceptions the lecturers make original contributions to the subject. There is no need to say that without exception each lecturer is scholarly and shows that he has his finger on the pulse of his subject matter. The volume can be most heartily recommended to the American psychological public.

University of Iowa CHRISTIAN A. RUCKMICK

Progressive Relaxation. By EDMUND JACOBSON. The University of Chicago Monographs in Medicine. Chicago, University of Chicago Press, 1929. Pp. xiii, 429.

Dr. Jacobson's book should be of great interest and value to psychologists, as well as to psychiatrists and to medical practitioners generally. His papers on relaxation have been appearing in various medical, physiological, and psychological journals during the past decade. The present volume is a welcomed compendium of his experimental work, and of its important theoretical implications.

The author points out that the remedy "rest," although generally prescribed for numerous disorders, is not clearly understood. The fact that a person is told to rest does not assure that he will achieve this state, in the physiological sense. The patient may not know how to relax, and a state of neuro-muscular "hypertension" will persist, producing restlessness and increased "nervousness." This neuromuscular hypertension is the condition which Jacobson wishes to control, in order to bring quiet to the nervous system. This condition of hypertension may be produced by many and diverse causes, and may manifest itself in equally varied symptoms. Among the latter are: (1) increase of tendon reflexes; (2) increase of mechanical muscle excitability; (3) spastic conditions of smooth muscle; (4) abnormal excitability of the heart and respiratory apparatus; (5) tremor; (6) restlessness, volubility, and other apparent overactivity involving the skeletal muscles (pp. 13-14). Incidentally, the suggestion is made that neuromuscular hypertension be substituted for "neurasthenia," a term of misleading signification. So-called neurasthenic symptoms may not be indicators of "nerve weakness" or "exhaustion," but rather resultants of hypertension.

By "progressive relaxation" the author means an extreme degree of relaxation, in contrast to what ordinarily is termed relaxation. The latter fails to do away with "residual tension," which accomplishment is the essential feature of Jacobson's method. The subject will not be able to relax completely at once; in certain chronic cases relaxation is achieved only after a rather lengthy period of training (it may require several months). Two chapters are devoted to descriptions of this training technique. One of these describes the method of "general" relaxation, the other that of "differential" relaxation. The latter refers to a "minimum of tension of the muscles requisite for an act, along with the relaxation of other muscles." For example, an individual while reading or singing may be contracting systems of muscles entirely unessential to the performance of the activity. This produces fatigue and lowered efficiency; differential relaxation aims to enable the person to reduce this excess tension.

The author's contention that relaxation of this systematic sort reduces excitability is supported by experiments on the flexion reflex, the knee-jerk, and, less conclusively, perhaps, on "mental activities." The flexion reflex was elicited by an induction shock, the extent of the reaction being recorded on a revolving smoked drum. Two conditions, "normal" and "relaxed," were used in various sequences and a period of preparatory training was employed to teach subjects to recognize minute tensions and to relax them away. The results showed a

much greater percentage of failures to react when relaxed than when "normal." The extent of the movement was likewise much less, although statistical reliabilities of the differences under the two conditions are not calculated. (The probable errors of the difference may, however, be computed from the data given.) With one exception, the subjects reported, without being questioned, that the shock was less painful and apparently weaker during relaxation.

Similar results were secured with the knee-jerk. The reflex diminished with advanced relaxation; in some subjects it was entirely abolished. Experiments on differential relaxation while reading, writing, etc., yielded similar results. The "curve of relaxation," plotted from data secured on 13 untrained medical students, showed a gradual decline in the extent of the jerk during a 25-min. period, for stimuli presented at 30-sec. intervals. These subjects received no training, in contrast with those used for general relaxation; no mention was made of "relaxation" and the results are interpreted as indicating that adaptation to the conditions occurred "whereby the organism, at first using neuromuscular energies not essential to a task, soon omits them, so that more approximately only those energies which are required come to be in use." The author points out the essential similarity, when reciprocals of the values representing extent of reaction are plotted, between the curve which he obtains with normal subjects and curves representing the learning process (Bryan and Harter, etc.). With untrained neurotic patients no such decline in extent of the knee-jerk was noted, indicating habitual hypertension. In further experiments he shows that decrease of the knee-jerk tended to be prevented by stimuli having an exciting effect.

The experiments on the effects of relaxation on mental activities are not supported by quantitative data of the sort available for the flexion and patellar reflexes. The subjects used were 15 patients and 15 students in psychology and physiology at the University of Chicago. All of these received prolonged training in relaxation, in order to enable them to report even minute and fleeting tenseness in whatever muscle group it might occur. When they were well-trained, they were asked to "imagine" various situations (visual, auditory, verbal) and to report any tenseness noted during the process. The following conclusions merit quotation in detail: "With visual imagery there is a sense as from tenseness in the muscles of the ocular region. Without such faint tenseness the image fails to appear. . . . Motor or kinaesthetic imagery likewise may be relaxed away. 'Inner speech,' for instance, ceases with progressive relaxation of the muscles of the lips, tongue, larynx and throat. Auditory imagery also is attended by a sense of tenseness, sometimes perhaps felt in the auditory apparatus, but characteristically in the ocular muscles. The individual tends to look toward the imagined source of sound. . . . With progressive muscular relaxation, not alone imagery, but also attention, recollection, thought-processes and emotion gradually diminish."

A chapter is devoted to emotion; and various experimental studies of physiological changes present during emotional excitement are reviewed, as are also theories of emotion. Jacobson reports that his patients and subjects "agree that the emotions subside as the individual *completely* relaxes the striated muscles, particularly those which he seems to find concerned in the

emotions at hand: the esophagus in one instance of fear; the forehead and brow as a rule in worry or anxiety." The subjects found it impossible to be both emotional and relaxed at the same time, even when they were specifically instructed to do so. These results are, of course, in accord with the principle of the James-Lange theory of emotion. Jacobson is inclined to emphasize proprioceptive factors as being largely responsible for emotion, although he says that the search for any single uniform physiological measure of emotion will probably prove futile. "The aim should be to find many quantitative tests for emotions in a particular test situation and to seek to correlate them where possible."

Voluntary relaxation of striped muscles tends to produce relaxation of smooth muscles. Cases are discussed in which spasticity of the esophagus and of the colon was relieved by relaxation. The evidence here was quite direct, since both organs were observed under the fluoroscope when barium was present.

In general, the facts presented in the book indicate that neuromuscular tension in any part of the body tends to enhance the excitability of all other parts. Relaxation, when actually secured, tends to lower excitability and so to reduce excess activity and "nervousness." The voluntary reduction of muscular contractions, which can be cultivated as any other habit can be acquired, lowers emotivity and promotes normal physiological functioning. These are facts which impress the reviewer as being very important. In contrast with various highly imaginative brands of "psychotherapy" which flourish in our gullible age, the methods and principles advocated here grow out of scientific soil. They are developed in the light of approved physiological and psychological conceptions and they are put to experimental test. Dr. Jacobson is to be congratulated on his achievement.

Vanderbilt University LYLE H. LANIER

Die Entwicklung der Musikalität in den Reifenjahren. By MARIA VAN BRIESSEN. Friedrick Mann's Pädagogisches Magazin, Heft 1243. Langensalza, Herman Beyer & Söhne, 1929. Pp. 127.

This monograph from the Psychological Laboratory of the University of Graz, with an introduction by Otto Tumlirz, editor of the *Jugendkundliche Arbeite* series of which it is a number, seeks to show by a series of experiments that the development of the complex of musical aptitudes follows, like that of other mental endowments, an orderly course of maturation. A description of the experiments used follows a brief examination of previous work along related lines, carried out mainly in Germany. Though this work lacks the degree of objectivity desirable in experimental investigations, it is very suggestive, especially to those who are inclined now to think of musical tests only in terms of the Seashore phonograph-record tests. The tests carried out in the present investigation, in many cases carried out with poor technique and involving personal elements which make verification by other investigators impracticable if not impossible, are the following. (1) 'Absolute' and relative hearing of pitch: singing a given tone, a^1, after it has been sounded on the piano: naming a note (f^1) that has been played on the piano: singing again from memory, or

recognizing it when given, the first tone. (2) Interval perception. (A) Successive Intervals. Saying which is the higher when C and $C^{\sharp}$ are sounded; comparing the size of the intervals $E{:}A$ and $g^2{:}d^3$ and of $A{:}c^{\sharp}$ and $e^3{:}g^{\sharp 3}$ (Are they alike? If not which is the larger?); recognizing and naming these and the following intervals: $e^1{:}a^1$, $c^2{:}a^1$, $f^{1\sharp}{:}a^1$, $d^2{:}e^1$, and $c^1{:}a^1$. (B) Simultaneous Intervals. One or more tones given, S asked to say how many (S was started with $D{:}A$); also the comparisons and recognition called for under *successive intervals* were here repeated as *simultaneous*, and, in addition, naming of interval $e{:}g$ successive and of $e{:}g^{\sharp}$ simultaneous. (3) Chords: Consonant or Dissonant ($e^1{:}g^{1\sharp}{:}c^2$, and $e^1{:}g^1{:}b^1{:}c^{2\sharp}$). The chord $a{:}e^{1\flat}{:}f^1{:}c^2$ is reacted to as to completeness or incompleteness and S is to judge whither c^2 tends; he then is to answer the same question regarding the lowest tone, a. Same for the chord $a{:}d^1{:}e^1{:}g^{1\sharp}$. S is then requested to say whether $a{:}d^1{:}f^1$ is a major or a minor triad. The same procedure is followed when the triad $f^{1\sharp}{:}a^{1\sharp}{:}c^{2\sharp}$ is played. E sounds on the piano $a{:}e^1{:}g^{1\sharp}{:}d^2{:}f^2$ and S is to say how many tones there are. The major triad $a{:}d^1{:}f^{1\sharp}$ is sounded on the piano and S is to sing the individual tones and is to answer questions as to the intervals sung. (4) Melody perception and memory: S sings, in the syllable 'la,' certain unfamiliar melodies played on the piano; reproduces from memory; and recognizes certain alterations in a second playing. (5) Harmony perception: S is to recognize and speak out "wrong" whenever discords or mistakes occur in an erronously harmonized melody played on the piano. (6) Rhythm: A melody is played in different rhythms and S is afterwards to indicate the rhythm in each case, with movements of the hand. (7) Empathy and Imagination: A series of different selections is so played in succession as to constitute a sort of complex whole; observations are made and questions asked to ascertain S's affective and ideational responses to each variation. Questions are asked concerning any special imaginative experiences of S in hearing music.

The entire series of tests lasts about 35 min. for each S. It is acknowledged by the author to be a qualitative test, but it probably has considerable value, though the reviewer doubts that this is effectively brought out in the methods of treating results chosen. The intention was to have 10 youths of each sex for each age of 12 through 19 years (160 Ss) but this was not fully carried out. Scores were assigned for the various parts of the tests as follows: 1 for right, 0 for wrong; half right 0.5, three-fourths right 0.75, etc. This weighting was arbitrarily assigned so that the various parts of each of the main tests, and finally these main test scores themselves, could be averaged. Thus all results of individual exercises, of the separate tests, and of the entire test series combined are given in a single table in hundredths, varying from 0.00 to 1.00. Graphical representations are then given of the average score made on each of the 7 tests by each sex-group, under the six ages 12-14 (combined), 15, 16, 17, 18, and 19 years. These results then receive extensive discussions. No reliability measures of differences between sex- or age-groups are offered, and one feels that the discussions are therefore often of little value and largely speculative. In general the curves rise in the early years (females as a rule higher at first than males) and fall later, the females most often ending lower than the males. The averages of all the tests for the age-groups just indicated, males

first in each pair of scores, are as follows: (12-14) 0.30, 0.40; (15) 0.47, 0.46; (16) 0.54, 0.52; (17) 0.52, 0.52; (18) 0.60, 0.57; and (19) 0.58, 0.52.

The author admits that several factors other than native musical endowment contribute in different measures, and variously at different ages, to the general results. For instance, at certain ages specific school training enters; and the popular music heard outside of school probably affects the sexes differently in the later age-groups; the girls, according to the author, being exposed to a greater extent and affected more than the boys.

The author expresses the view that not the possession of a number of separate musical aptitudes as such, but rather the total organization of these aptitudes (instead of the mere sum of them) characterizes the "musicality" of the individual. Yet it must not be assumed that all the aptitudes in the individual develop *pari passu*; rather the development seems to follow roughly the order *rhythm, melody, harmony*. Van Briessen holds that instruction in the playing of any musical instrument must first of all be real education *in music*.

The contributions seem to the reviewer not to be either the results obtained or the methods of handling them, nor the conclusions and explanations offered by the author, but the suggestiveness of several of the experiments to those musical psychologists who desire to extend researches on musical abilities into fields not yet possible merely by the more quantitative tests now in use in America. J. P.

A Study of Educational Achievement of Problem Children. By RICHARD H. PAYNTER and PHYLLIS BLANCHARD. New York, The Commonwealth Fund, Division of Publications, 1929. Pp. x, 72.

Two methods have been generally employed in investigating the nature of the relationship between academic success and behavior disorders in children. Research workers who have made use of the first of these methods, namely, the case study, have usually agreed in reporting a significant relationship between personality and conduct deviations on the one hand and poor scholastic standing on the other. No such consistency characterizes the results of investigations made by means of the second method, namely, statistical treatment of data. An important contribution of the latter type has recently appeared under the title announced above.

The subjects used in this investigation consisted of one group of 167 children who were examined at the child guidance clinic of the National Council for Mental Hygiene in Los Angeles in 1924 and of another group of 163 children who were studied at the corresponding clinic in Philadelphia in 1925. Approximately one half the children in both cities were of American stock, and none had an I.Q. of less than 0.80. All school grades from grade 2 to grade 9 were represented in the two cities, the smallest numbers being 17 in grade 9 and 24 in grade 2, and the largest numbers, 56 in grade 3 and 57 in grade 4. Each of the 330 children received a thorough clinical examination (including the taking of social and medical histories), a complete physical examination, various types of psychological examination (the Stanford-Binet and one recognized group intelligence test, the Woodworth-Matthews questionary, the

Pintner-Paterson Performance tests, the Stenquist Mechanical Assembly tests, etc.), educational achievement tests (the Stanford Achievement and the Otis Classification tests), and a careful psychiatric examination. The data are represented in 22 tables which utilize various methods of classification: percent distributions on the basis of I.Q., E.Q., A.R., kinds of physical defects and behavior difficulties, grade placement, and the like; comparisons with corresponding data for large unselected school populations; comparisons with smaller groups selected for purposes similar to that of this study; comparisons within the group of 330 children, of those whose I.Q., A.R., etc. were above 100 with those whose corresponding measures were below 1.00, and also of the ten children in the Los Angeles group with the highest A.R. and the ten with the lowest. Wherever important differences seemed to have been found, their reliability was measured by appropriate statistical procedures.

Only the most important conclusions can be quoted here. "In so far as shown by educational quotients and accomplishment ratios, the data would indicate that educational achievement remains comparatively unaffected, in general, by any specific behavior difficulties" (p. 38). "Comparisons between types of personality and behavior difficulties with respect to grade placement for life, mental, and educational ages bring out almost no significant inter-relations" (p. 38). "Physical defects or social conditions of children did not lower their educational achievement as measured either by tests or by school progress" (p. 41). "There has, perhaps, been too much of a temptation to generalize on the basis of . . . individual studies, and to imply that the existence of personality and behavior deviations will necessarily impair educational achievement. All that we wish to point out . . . is the absence of any such general trend. It still remains true that in certain pupils the failure to rise to normal levels of achievement is the result of emotional maladjustments; but it would appear that the number of these cases is smaller than one might think after reading some of the literature" (p. 62).

The authors are fully aware of the weaknesses in their data and of the shortcomings in their statistical methods. A very excellent chapter (Chapter VI) is devoted to a critical discussion of their measures and their technique. Among others, the following points are made: they were unable to secure analogous information from a control group of ordinary unselected school children; their cases were somewhat too small in number to justify wide generalizations; there was much undesirable overlapping between personality ratings for traits and behavior diagnoses; the measures of intelligence, school achievement, school progress and the like were lacking in precision; the derived measures, especially A.R., were therefore untrustworthy; classifications for statistical treatment were made on purely arbitrary bases without due regard for possibly important distinctions. Such frank admissions of the limitations to which their investigation was subject are convincing evidence of the impartiality and expertness of the authors and should be of large value in showing to others who may contemplate further studies in this field the many variables which are involved in applying statistical techniques to clinical data.

George Peabody College — W. A. Brownell

Fundamentals of Objective Psychology. By J. F. DASHIELL. New York, Houghton Mifflin Co., 1928. Pp xviii, 583.

The point of view taken in this book is that psychology is a biological science, distinguished from the other biological sciences in the emphasis which it places upon "man (or animal) in his interaction with environmental conditions."

The author disposes of the introspective method in a short paragraph very early in the book. The method, he holds, is in general untrustworthy although it may with justification sometimes be used. When instruments fail, for example, to reveal what the subject is doing, the subject may be questioned regarding his experience. (A procedure against which careful users of the methods have repeatedly warned!) The argument against introspection is superficial and betrays little first-hand acquaintance. The author's objections arise from a confusion of the aims of pure and applied science, and rest upon the wholly unwarranted assumption that objective performance is a 'check' upon conscious experience.

This methodological discussion occupies a position of pivotal importance, not only in the introductory chapter, but in the book as a whole. The preceding sections lead up to it, and the succeeding chapters illustrated the applications which follow from it. It is, therefore, absolutely essential that the reader obtain a clear understanding of the author's methodology before proceeding further into the book.

Other topics treated in the introductory chapter are the motives to the study of psychology, the origins of the natural sciences, and the major divisions of psychology. Of special interest is the author's assertion that the desire to get control of human nature is the motive to the study of psychology. There is no desire, according to him, for knowledge for its own sake. The desire for pure knowledge "springs from the human being's practical demand for regulation and control of things."

The author is concerned in Chapter II with the concept of trial-and-error behavior; in Chapter III with the analysis of human behavior; in Chapters IV-VI with the physiological bases of behavior; and in the twelve remaining chapters (VII-XVIII) with the fundamental problems of psychology and the specific facts revealed by behavioral methods.

The first problem considered is that of integrating the 'elementary action-units.' The treatment follows that of Sherrington and Pavlov. The chapter on "Native Reaction-Patterns" includes a description of the experimental procedures used in determining innate responses and in recording the expressions of emotions. An entire chapter is devoted to "Motivation." *Drives* are distinguished from *motives* proper. The former are the original sources of energy that activate the human organism; the latter are the acquired sources, the directions of activity. The primary drives are tissue conditions or needs, such as hunger, sex, and unfavorable temperature liberation through the skin, *i.e.* the need of maintaining a constant body-temperature. One section of this chapter deals with Freudianism.

A form of behavior much neglected is brought to the fore in the chapter on "Postural Responses." Sherrington's distinction between phasic and tonic

contractions is applied here to behavior in general. The concept is illustrated by reference to the reaction-time experiments, attentive behavior, and moods. There is also a section on the determining factors of attention and on the classical problems of attention, *e.g.* span, fluctuation, and distraction.

The chapter entitled "Intelligent Behavior" is, in the opinion of the reviewer, the least satisfactory in the book. Here the author accepts the judgments of such authors as Terman without attempting a critical evaluation of their opinions in the light of recent findings. For example, the much questioned constancy of the I.Q. is, without qualification, baldly asserted. One searches also in vain for mention of the recent advances in the study of animal intelligence.

Learning is treated in orthodox stimulus-response fashion—it is merely a matter of changes in the operation of elementary action-units. Learning by 'insight' is simply a matter of learning to react to some highly specific element in a situation—something very different from the meaning of the authors who introduced the term into animal psychology.

Dashiell attempts, in his treatment of perception, to adapt the doctrine of *Gestalt* to his concept of the stimulus-response. He defines perceiving, for example, as "adjusting to objects and situations, of which the actual stimuli are only a part or a sign," *i.e.* the organism responds, not merely to objects, but also to relations between them. The author, however, does not show how an organism that learns only by modifying 'elementary action-units,' is able to transcend those limitations and respond to structures that are merely the sum of the parts. The introduction of the concept of *Gestalt* makes his account of human behavior more plausible, but it produces a rift in his system.

Language and thinking are both dealt with according to the demands of the stimulus-response concept. In thinking, the reactions of the striped muscles serve as symbolic substitutes for concrete stimuli. The speech mechanisms are the thinking mechanisms *par excellence* but other effectors may also take up the rôle through a process of short circuiting. A departure from the usual treatment occurs in the introduction of a directive factor—postural reactions. These general bodily responses or attitudes serve to direct thought; they function as determining tendencies.

In spite of the instances in which the author manifestly has to strain to force his facts into the stimulus-response formula, his book as a whole impresses one as an excellent compendium of experimental data at the textbook level.

Bryn Mawr College E. FRANCES WELLS

The Mind At Mischief: Tricks and Deceptions of the Subconscious and How to Cope with Them. By WILLIAM S. SADLER. New York, Funk & Wagnalls Co., 1929. Pp. xv, 400.

The book contains an introduction from the psychologist's point of view by Robert H. Gault, and one from the neurologist's point of view by Meyer Soloman, both of Northwestern University.

The first part of the book contains a discussion of the nature and working of the "subconscious mind," following the traditional lines of the psychoanalysts and presenting no essentially new point of view. Following this is a great

wealth of illustrative material on the various complexes, but nothing new is offered concerning their real mechanisms or their genesis. This part of the work might have been written by Freud, or any other of the psychoanalysts, although the author does not class himself as a disciple of Freud, and the term psychoanalysis is used very few times in the whole work. Sadler disagrees with Freud on the point that there is a tendency to suppress only the bad or ugly, but agrees that it is the unpleasant which is suppressed. He recognizes, instead of the single Freudian *libido*, five "master urges," viz.: the life, sex, worship, power, and social urges. These are described at length, but little real scientific support is given the classification.

A good description is given of the attempt (sometimes unconscious) on the part of children and neurotics to escape from reality, together with a considerable amount of speculation on the genesis of subconscious control over conscious behavior.

The writer gives a very plausible explanation of the common telepathic and spiritualistic phenomena, without resort to supernatural powers. He expresses his own belief in "spiritual forces" (p. 359) and attributes to "if not a majority, certainly a very respectable minority" of scientists the belief in the presence of a spirit as a part of man's equipment as a moral being (p. 363). He does not state what group of scientists is meant. Surely he cannot, in the light of Leuba's work, mean psychologists. These forces are, however, conceived as operating in a spiritual realm, and "their time is not occupied with trivial intrusion into the materialistic realm." He seems unable to free himself from a dualistic concept of mind and brain. Here is a good sample: "The human mind is conceived as a very intricate organization or grouping of cells, a grouping which holds the patterns of memory and thought, and which undoubtedly conforms to certain laws after the fashion of physical systems and constellations. It is known that certain groups of mind-cells or systems, called complexes, may be cut off, as it were, from active connection with the major mental powers, and may behave in an insubordinate manner" (p. 14). And again: " 'Physical brain processes of experiences are correlated with corresponding mind processes or experiences, and vice versa.' We come to see, then, that every mental experience leaves behind a residue—some actual change in the neurones of the brain. This actual change becomes 'the physical register of mental experience.' Psychologists believe that this physical register is very largely preserved in the subconscious mind." (p. 364)

The author has evidently attempted the laudable task of eliminating the mystical, "spiritual," and occult from several generally observed phenomena of self-deception, such as spirit mediums, as well as from the ordinary processes in neurotic sufferers. Unfortunately, however, he has stopped far short of a real explanation in unambiguous terms. While external supernatural powers are entirely repudiated, we find the explanations in terms of "tricks of the subconscious," "workings of the subconscious mind," etc., for an adequate description of the mechanisms of which we are still as badly in need as before.

The reviewer looked for the "real service to science" mentioned by Professor Gault in his introduction, but failed to find it. The description and analysis of the various neurotic symptoms is indeed interesting, though not

essentially new (see above). All too frequently at points where real evidence was needed the author substituted such statements as "It is well known that . . . .," "Psychologists agree . . . .," etc. This does not look like science, and there is little evidence in the book of the use of scientific methods.

Peabody College HOWARD EASLEY

Academic Progress: A Follow-up Study of the Freshmen Entering the University in 1923. By HAROLD A. EDGERTON and HERBERT A. TOOPS. Columbus, Ohio State Univ. Press, 1929. Pp. x, 150.

The purpose of this study is "to find out what happened, academically, to a class during the first four years after their entrance to college." The endeavor has been to obtain information about these students as regards (1) rate of elimination, (2) rate of retardation and acceleration, (3) rate of graduation, (4) transfers from one college to another within the University, (5) the relation of conditional entrance to later success, and (6) the prognosis of elimination and grades by the use of tests and estimates.

The subjects of the study were the 1,958 freshmen entering the Ohio State University in the fall of 1923. As might be expected, more elimination occurs at the end of each college year than between the quarters, and the rate of elimination decreases rather uniformly with the academic age of the class. Eighty-six percent of the students completed the work of the first year, and only 28.7% completed 12 quarters in the 15 quarters of possible attendance (including summer sessions).

The percentage of those registered who were retarded one or more quarters was greatest at the end of the junior year, amounting to 21%. Women were less retarded than the men. Eighteen percent of the entrants received their diplomas by the end of the fourth year. The percentage of survival for the women nearly doubled that of the men. It was estimated that 34% of the original class would ultimately graduate.

Of the 23% of the entrants who transferred from one college to another within the University, over one-half made the change during the first year. Conditional entrance, based on an apparently nominal deficiency in geometry credit, seemed to be justified, since the conditional students were significantly inferior in later attainments.

Attempts at prognosis of academic persistence (in terms of credits attained) were not every successful. Intelligence scores correlated only 0.19 with persistence, while grades for the first quarter gave a coefficient of 0.40 with persistence. Intelligence scores correlated 0.45 with grades.

It is significant that, for the women at least, moderate superiority in intelligence is more productive of credits than very marked superiority. The maximum number of credits were earned by those in the eighth intelligence decile. It is also noteworthy that students who were very superior in intelligence did not excel the average in general attainment as much as equally inferior students fell below this average. The validity of such a comparison is questionable, of course, since units on the two ends of the scales are probably not of equal value.

The authors conclude that, since only 34% of the entrants can graduate, "the University and the students are not properly adjusted to each other." Granting the truth of this statement, the reviewer does not feel that it constitutes a sufficient basis for criticism of the University, unless it is agreed that a college is in some way obligated to graduate all those who choose to enroll as freshmen. The university is not properly the *one* school for "all the children of all the people." While the handicapped individual deserves the very best training that society can afford, he nevertheless has no inalienable right to a college diploma.

University of Colorado THOS. H. HOWELLS

Psychophysiology, I. Problems of Psychology and Perception. By LEONARD T. TROLAND. New York, D. Van Nostrand Co., 1929. Pp. 404.

This first of a series of four contemplated volumes on psychophysiology, is devoted to a historical critique of the various theories which have appeared under the title of psychology. The author rejects behaviorism, *Gestalt* psychology, and introspection, yet finds something of value in each. Consciousness exists in some sense and seems to be in evidence (through introspection?) when some part of the environment is eliciting a response from an individual. In this relational situation psychophysiology finds its problem. The subject matter of the science therefore is not limited to consciousness, to the behavior of the individual, nor to the pattern of the response, but to an analytic effort to particularize the items of the environment, the mechanisms of response, and all the conditions which make up the precise causal relations between the two, taking due note of the temporal relations of the situation.

Consciousness is determined. The response arc (or all the parts of the mechanism which can be identified between the object and the action) may be set into activity by direct or by indirect determinants, or by a combination. A formula is given which conveys a definite theory of this relationship. The principal object of attack by psychophysiology is to determine the correlation between the two. This requires subtile, analytic methods which involve experiment. The results are handed on to cosmology without prejudice, for it is the business of cosmology to discover the nature of the processes and their mechanisms of causality.

A study of the theory of perception reveals that the material world is not taken for granted nor established as "real." A ship on the horizon perceived by an observer may be identified with his experience. Closer analysis, however, is necessary; as a result of this it becomes evident that the perception contains elements which could not be sensed directly, but which are merely controlled by the physical object. From this it is clear that the fixed point of reference is the consciousness concerned. The reality of consciousness is taken for granted, and the problem of psychophysiology is to discover those physical and physiological processes which determine the experience precisely as it appears.

Introspection as sometimes used by psychologists and philosophers is rejected. Instead, introspection is identified with simple direct description.

The acquisition of the necessary vocabulary in which to describe is the object of training in introspection. But given the words which symbolize the items of consciousness and their relations to the world, the psychophysiologist may proceed. How to check the accuracy of the application of the appropriate word to the product of observational analysis, however, is not clear.

So far as the first volume is concerned, it is a compendium of quotations from other authors, not altogether predigested. A wide range of detailed problems is presented, for the most part treated in the conventional mode.

New York City ARTHUR H. SUTHERLAND

Human Behavior: A First Book in Psychology for Teachers. By STEPHEN S. COLVIN and W. C. BAGLEY. Second Edition, revised by the junior author and Marion E. Macdonald. New York, The Macmillan Co., 1929. Pp. xi, 334.

The early chapters have been changed but slightly and still represent largely the work of the deceased senior author. Consciousness or mind—the two are treated as synonymous—with such of its principle manifestations or aspects as attention, sensation, meaning, perception, still functions as a "guide to behavior," but we are not told *how* it thus controls activity. Explanations often seem somewhat muddled, as in the first edition, as when we read that "habit is basal to all learning and weaves itself into all our acts" (p. 8). When based on subjective faculties they are just as bad, little attention being given to the actual stimulating conditions which effect the adjustments and learning. Few psychologists today would subscribe to the statement that "psychology is concerned with consciousness and its function as a director of behavior" (p. 15). The injections of this 'director of behavior' hardly clarify the cases of animal learning used as illustrations in Chapter II. The effect of the recent discussions of 'instinct' in the psychological literature are noticeable in several parts of the new edition. Certain topics and definitions dealing with instincts in the first edition are omitted or modified. For instance 'unlearned behavior' has been changed in the new edition to 'spontaneous behavior' with special reference to play. 'Spontaneous' here is hardly taken in the sense that Jennings has used it. Spontaneous behavior like play is performed "for no other reason than the mere satisfaction that these activities yield," an 'explanation' which is, of course, entirely wrong. One wonders if sneezing is also 'for the satisfaction it gives' rather than simply because some structure is stimulated. The authors would have done well to read James's criticism of the hedonists in which he charges them with confounding a pleasurable act with one seeking pleasure.

The most important changes are in the later chapters where tests and other measurement methods are considered, together with individual differences in intelligence and their significance. The well-known views of Bagley as to the influences of environmental factors on individual and racial scores in intelligence tests are emphasized as well as the importance of gains in the 'intelligence' of pupils by training, even though such gains are small. What genius achieves is significant only "in the degree that it can be brought into the service of the great masses." This is doubtless true in a general way, but there are no qualifying statements. It is often damaging to productiveness to demand of genius immediate and direct results. Among psychologists experienced in

taking different measurements of individuals there would be little tendency to confound I.Q. as determined by some single test with degree of intelligence as determined by extensive and careful determinations, and there would be a correspondingly diminished tendency to interpret effects of training as "changes in I.Q." Native endowment which determines in general one's relative standing among one's fellows seems unduly confounded in this new book with any particular I.Q. measurement. Even Binet, to say nothing of many careful testers in recent years, clearly recognized the great susceptibility of certain tests to environmental and specific school effects.

The present revision, as was true of the first edition, is very clear and simple in its statements from the point of view taken. Exercises and references are modified, and the latter are brought up to date in a general way and are given at the end of each chapter rather than at the end of the book as formerly. The last two chapters consider in a practical way factors like fatigue, which affect efficiency, and also the foundations of moral character.

J. P.

What is the Mind? By G. T. W. Patrick. New York, The Macmillan Co., 1929. Pp. viii, 185.

Perhaps a more descriptive title would be "What *does* the mind?" This would be more in accordance with the material discussed and with the decidedly functional viewpoint of the author.

Professor Patrick takes a very optimistic view of the capacities and accomplishments of the human mind, phrases of glorification appearing frequently. Such an optimistic view is perhaps natural for one following the theory of emergent evolution. The psychologist will readily grant the step upward from trial and error behavior to intelligent action, but when this passes on to morals and values it becomes rather distasteful to one behavioristically inclined.

In describing what the mind is, or rather does, the author always starts with behavior, but does not stop there. A definitely dynamic or functional point of view is taken; interests, impulses, and strivings are always added to complete the picture. These latter are the real sources of action, being flexible, adaptive, and end-seeking, in contrast to the invariability of the stimulus-response hypothesis. Living beings are not driven; they furnish their own motive power.

A definite contribution of the emergence theory is the simplification of mind-body relationships. Patrick feels that the major share of past difficulties arose through the tacit assumption that a duality or opposition existed between the two. With this taken for granted, trouble in relating the two naturally arose. The emergent view regards mind as being a new principle achieved (from the body) at some stage in development. The functional aspect furthers the argument; instead of treating the mind as made up of various elements Patrick says "the mind is not a kind of *being* but a kind of *doing*." "Mind is not antithetical to body, but superior to it."

In many points Patrick's views coincide with the *Gestalt* interpretation, but he fails to take this school into consideration except in one or two minor

points. Things are related because of belonging together; it is useless to try to analyze apart the various units. "The human organism is no aggregate of elements but an organization."

He mentions the *Gestalt* theory, the Freudian school, relativity, and other recent ideas as showing trends away from a mechanistic view and toward a dynamic aspect of life and nature, or the world in general. The views of Bergson and McDougall are favorably received.

The main contributions of this book, as the reviewer sees them, are in the functional and dynamic viewpoints taken, and in the proposed solution of the mind-body problem. The main thesis "What *is* the mind?" is not settled. Whether it ever can be or not is a question.

University of Illinois R. W. Husband

Traité de psychologie. By George Dwelshauvers. Paris, Payot, 1928. Pp. 672.

Psychological textbooks are primarily American productions. It is, therefore, a noteworthy event when a treatise of psychology which does not clearly exceed the dimensions of a textbook appears abroad, especially in France. Professor Dwelshauvers, who has an appointment at the Catholic Institute of Paris and is director of the psychological laboratory at Barcelona, has undoubtedly done the French speaking world a service in compiling so comprehensive and thorough a treatise in the field of psychology.

The style of the book is simple, readable and yet scientifically accurate. There is a wealth of illustration both from individual human experiences and from scholarly sources. While the writer bases his material chiefly on experimental material from many directions he does not omit a free rendering of theory where explanation is called for.

As for subject matter the writer is both orthodox and eclectic. With regard to method he steers a neutral course. Both introspection and the procedures generally termed behavioristic are given sanction. It is a pity that so many writers, however, put introspection on the coördinate plane with experimentation and saturate it with the historical connotation of intuition. The statistical procedures are also briefly described. And a book coming from France would not be complete without a discussion of pathological procedures, but there are references also to sociological and comparative treatments and the field of application receives attention. The bibliography appended to the first part is not as comprehensive or as authoritative as one could wish.

The striking feature of the second part is the treatment of mental synthesis or integration. An excellent discussion of habit follows which naturally leads to the dynamic aspect of instinctive life. The third part treats the customary material of elementary psychology, including the affective life, movement, and sensation. In the fourth part the next level of mental development, that of imagery, the association of ideas, the perception of space and time, are competently discussed. The fifth part provides us with a most thorough-going discussion of memory together with its abnormal aspects. Invention and verbalized processes together with their disturbances form the chief subject-matter of the section. The sixth part occupies itself with thought, voluntary action

of the will, the durable affective states, personality, the self, character, and intelligence.

In a book of this length there is usually much to criticize but the reviewer's failure to do so is conditioned by the author's skilful maneuvering. Rarely is a definite opinion stated or a particular theory advanced. Such matters are left to the discretion of the reader. The treatise is well illustrated and the science is enthusiastically sponsored. There is a plausible omission of much physiological material. At the present stage of French psychology this treatise is certainly worth inclusion in any professional or academic library which aims to be international in scope.

University of Iowa CHRISTIAN A. RUCKMICK

The Theory of Identical Elements. By PEDRO TAMESIS ORATA. Columbus, Ohio State Univ. Press, 1928 Pp. 204.

Though presenting no original experimental data, this work is valuable as an excellent analysis of the literature on the transfer of training. Its purpose is to show that Thorndike's view generally known as the theory of identical elements, is unwarranted and leads to false educational corollaries. It concludes with an outline of an ambitious and original 'non-mechanistic' theory, one which identifies transference with intelligence and regards both as consisting essentially in the application of meanings acquired from past experience to new situations.

It is conceded that Thorndike is correct in saying that there must be some sort of identity between the old and the new situation if there is to be any transference. The objection is urged, however, that Thorndike does not rightly conceive the nature of this identity and consequently fails to offer a correct explanation of how this identity functions to bring about transfer. Regarding transfer as essentially a process of identifying and applying meanings, the author criticizes the theory of identical elements because it does not give a satisfactory account of this process. The argumentation is rather indecisive because it is carried on from the standpoint of the author's own unsubstantiated theory, a theory which in the reviewer's opinion not only needs further experimental support but also suffers from a certain vagueness which is not lessened by insistence upon a non-mechanistic viewpoint and extensive reference to the *Gestalt* psychology.

Of more value than the argumentation of the first two chapters is the review of the experimental evidence in the next five chapters, which compose the main portion of the monograph. It is shown that the amount of transfer is not as restricted as Thorndike claims and that the majority of the very numerous investigators of the subject believe that training may be generalized if appropriate methods are used. More in particular the writer claims that the existing data contradict two common assumptions: (1) that the amount of transfer varies mathematically with the degree of identity or similarity between the activities or performances in question; and (2) that a complex situation is the simple sum of its component elements, and that a response to it is merely a sum of responses rather than an integrated whole (the *Gestalt* argument).

In Chapters VI and VII it is brought out that the amount of transfer rather than being a function of the degree of similarity between the performances in

question is primarily a function of the methods used to bring about transfer. Methods which lead to the formulation of general principles regarding techniques of procedure are shown to be particularly favorable.

The last seventeen pages of the monograph are devoted to a bibliography conveniently arranged under eleven subheadings.

University of Illinois. HERBERT WOODROW

La quatrième dimension. By H. J. J. BUYSE. Brussels, La Rénovation, 1928. Pp. 128.

In "La vie de l'espace," the poet Maeterlinck discussed "the possibility of finding a solution for the problem of the fourth dimension, not in external space, but in man himself, starting with what man imagines to be his ego; the necessity for considering time and space as married; and the correspondence of the elements constituting the unity of space-time with the elements constituting the unity of man: space representing the time in his body, and time the space of his mind." The present book has for "its fundamental thesis the coördination of these three propositions," the author being of the opinion that Maeterlinck has discovered aspects of the problem "which have not been perceived by any of his predecessors" in "the marvelous wedding celebrated by space and time and to which men of good will are invited."

Observing that any attempt to transcend three-dimensional experience in extension toward an objective fourth dimension is an attempt to know the unknowable, the author reverses the hypothesis, making the problem one of *intention* or *immension,* namely that of shuffling off the restriction of the third dimension, while retaining memory of third dimensional experience. The problem of the fourth dimension thus becomes that of the second dimension, that is of the psychology of a post-human or supra-human two dimensional being, who differs from the usual resident of hypothetical "Flatlands" in being able to penetrate with consciousness the third dimension, having been there before. With the aid of a "mysterious sixth sense" and on the basis of questionable and little understood psychic phenomena such as telepathy, which in agreement with Maeterlinck he regards as "today scientifically acknowledged and classified," dream phenomena, hypnotism, and "others like the communication of disembodied spirits which are still matters of debate," the author attempts to determine "not perhaps what states intermediate between the second and third dimension are but what they might be (hence what we might conceive them as being)." In brief, the problem of the fourth dimension becomes here as usual the problem of an extra-spatial somewhat. It becomes, that is, the problem of ignored and unsolved problems left on our hands by any method we may employ, and in that sense is no new mystery. Even the marriage to which all good relativists have recently accepted invitations, results, in the opinion of the reviewer, in designating as space-time a somewhat which has no more right to the designation than has the formulation of subjective colors in terms of ether vibrations (intuited, subjective, Euclidian space-time) to be called "red," "blue," etc. Unquestionably there is something not in the world of mathematics which like "infinity," really belongs to the world of dynamics

and which involves us in peculiar paradoxes when we try to handle it in the language of another realm; but nobody familiar with the work of Kant and his successors will find anything in this really new. Moreover, the mundane psychology of three-dimensional beings has interpretations of the abnormal phenomena, discussed in the present work, in which it thinks at least that there is more truth if less poetry.

Vanderbilt University HERBERT SANBORN

Psychology (For Reviews). By J. BAAR. Introduction by C. J. Warden. New York, Globe Book Co., 1928. Pp. vi, 126.

This book is merely a collection of classified notes taken from the readings of a number of current textbooks in psychology. Textbooks themselves are usually too brief to do justice to the facts and especially to the theories of different investigators, and this condensation of several textbooks is doubly unsatisfactory as being too brief to be accurate or useful for anything but to pass examinations on the sort of stuff that one has uncritically committed to memory. Theories and views of different authorities seem to be over stressed. Most of those stated are seriously inadequate and some of them are positively wrong, giving a too great emphasis on points that are not of great importance. Moreover, they are not approached from the standpoint of explaining a group of facts or of harmonizing them into a consistent whole; they are given just as bare 'views' evidently to be memorized for examinations. The notes may be helpful for one who has read the sources thoroughly, but they are not valuable to others as substitutes for reading; on the contrary, they are among the many present-day substitutes for real work, which encourage a mere cramming in order to *appear* to be master of what one does not really know. Such 'aids' only take the student from the valuable sources, or even from the less satisfactory texts, and put him into a condition that is worse than if he simply wasted his time. The person who has read the sources will doubtless prefer his own notes, or will desire to refresh his memory on points that only the sources can supply satisfactorily.

The 'reviewers' of the present volume will read under "How We See," that the light rays pass through the cornea, the aqueous humor, the pupil, the lens, the vitreous humor and the nine layers of the retina, to stimulate the rods and cones; then that impulses are "sent to the brain" by means of the optic nerve whose course is said to be "from the eyeball, through the blind spot, into the occipital cortex, the nerve from the right eye going to the left hemisphere and the nerve from the left eye going to the right hemisphere [sic!]." The eyes, to aid vision, make two main adjustments—accommodation and eye-movements (conjugate and convergent). Apparently the visual cortex passively receives the impulse and "sees." Under "What We See", students will read about qualities of visual sensations, complementary colors, laws of color mixing, Purkinje phenomena, after-images, and simultaneous-contrast. *Objects* are not mentioned at all! Obviously this is neither good introspective nor objective psychology. Then after a few notes on color vision considerable space is given to theories. That is about the typical treatment, and by no means the worst. Some sections are better. But how will study of such books make psychologists of students?

J. P.

The Standard System of Music Notation. By HARLOW E. LAING and AROLD W. BROWN. Ypsilanti, Mich., Standard Printing Co., 1928. Pp. 19.

This pamphlet describes what is called "the standard system of music-notation," and argues for its superiority in the simplification which it presents to the learner. The conventional system of music-notation places a heavy burden on the student in that he must learn to respond quickly to two different clefs—the treble, or *G*, and the bass, or *F*, clef—whose differences are rather confusing. It is argued, very reasonably from the standpoint of psychology, that this is a great obstacle to musical progress and that with only a slight change in the clefs the Standard System of Music Notation is obtained, which results in very little loss but much gain in simplicity and ease of acquirement. This system involves a *twelve-line staff* which results from the use of two ledger-lines, *c* and *a*, instead of one, *c*, so that the bass and the treble staffs become exactly alike, being two octaves apart, and each note thus occupies the same place on both staffs. It is pointed out that the change to this standard notation will bring about very little inconvenience to present musicians, since most of them already know the treble clef; that the new twelve-line staff will not only permit of more rapid progress, but that it automatically does away with the troublesome problem of instrumentation and transposition. It may be objected that this change is unnecessary, so far as the learner is concerned, since children are today taught both clefs simultaneously and immediately, rather than as formerly only the treble clef first and then later the bass in terms of the first as a transposition of it. But learning is usually the acquirement of reactions to patterns, and certainly the simpler the pattern the easier the learning, other things being equal. This recent reform in teaching the eleven-line staff as a single unit is after all only an effort to overcome in a measure the difficulties inherent in it. That a simplification like that offered by the Standard System should not have received serious consideration and adequate trial earlier is an interesting commentary on the slavishness of musicians to their system as they find it.

If the Standard System overcame at once all the difficulties in the present system of music notation there could be little doubt of its favorable reception by musicians, publishers, and instrument manufacturers. This desirable reform is, however, only partial, as all musicians know. It will nevertheless be interesting to see how this extremely valuable suggestion and beginning toward a needed simplification will be received, and to note what opposition will be brought against it by the elements of conservatism. J. P.

Conflicting Psychologies of Learning. By BOYD H. BODE. New York, D. C. Heath & Co., 1929. Pp. v, 305.

The thesis of this book is that philosophical theory is more important than experimental fact for an understanding of the psychology of learning. Theories concerning the relationship between mind, or soul, and body are ably and critically considered. The conclusion is reached that the substantive mind is not a necessary assumption for psychology. The traditional materialistic view is also held to be inadequate to the solution of the problem of psychology. Watson and Thorndike are considered and certain inadequacies in their views

in regard to learning are pointed out. A view of psychology which does not, in the opinion of the author, involve the errors of the mind theories on the one hand, nor of the crude mechanical theories on the other, is proposed. This position is called by the author *pragmatic psychology*. From the standpoint of this psychology education is considered as the development of a dynamic individual and not as the imposition of certain habits or forms of response upon an essentially passive individual.

The reviewer recognizes the value of this book, but he fundamentally disagrees with its major contention. The author says, "When considered as part of a teacher's professional equipment, psychology is of significance for the light that it sheds on the nature of the learning process. To the teacher it is all-important whether the learning process centers in habit-formation, or the cultivation of 'insight,' or the untrammelled development of original tendencies. Unfortunately, the choice among such alternative views cannot be decided by appeal to experiment. In the end it must rest on a theory of mind, and the considerations which determine our theory of mind extend far beyond the data of experimentation." The reviewer believes that by appeal to experiment and by appeal to experiment alone are the problems outlined in the above statement to be solved. Indeed, enough experimental facts are at present available to make a decision on some points in regard to the learning process possible. The reviewer is at a loss to understand what the "considerations" are, other than the facts of observation upon which the decision in this important question is to be based.

Professor Bode alleges that from the standpoint of educational practice there is no such thing as psychology, there are only psychologies. The reviewer would exactly reverse this statement. He holds that for one who is interested in educational practice there should be no such things as psychologies, there should only be psychology. Psychology is a body of observational fact. Such facts must for convenience' sake, it is true, be organized in some systematic form. The system, however, as Titchener has suggested, is only an individual's blundering effort to give temporary coherence to the facts of observation which transcend the individual. The facts of psychology go on; systems of psychology wax and wane. To take but a single example: The facts of learning that have been developed by Ebbinghaus and his successors are more important to one who is faced by the concrete problem of teaching than is all the speculation on the body-mind problem in the works of all the philosophers. As the scientific study of the learning process continues the number of established facts will be increased. The method that will lead to this advance will be quantitative experimentation and not philosophical speculation.

Brown University — LEONARD CARMICHAEL

The Pedagogical Value of the True-false Examination. By A. W. COCKS. Baltimore, Warwick & York, 1929. Pp. x, 131.

The major problem in this study is the pedagogical value of the true-false test as compared with the multiple-choice type. In the course of the investigation the author also became interested in two subsidiary aspects of the problem;

namely, the most valid method of scoring true-false tests and the possible dangers in presenting false statements to students.

Twenty-six boys in their first year in high school, 26 in their second year, and 24 in their third year were divided into equivalent groups on the basis of their I.Q. and their M.A. A test, consisting of 50 true-false statements and 50 multiple-choice questions, was prepared from material in the high-school course in physics. (The year in which this material was customarily taught in the high school is not stated.) All students took the test, 15 min. being allowed for each part. Then one group of the boys from each class was dismissed, and the other group corrected their papers under the direction of the investigator. In the case of the multiple-choice questions the correct one of the suggested answers was merely designated; in the case of the true-false statements an explanation was given for the correct answer. The same 100 items were given in the form of a recall test to all the students one week later. A comparison of the results, which are presented in detail for all 76 of the boys individually, showed that those who had corrected their papers had profited much from the experience, that the "pedagogical value of the true-false test is more than 50% greater than that of the multiple-choice when both tests are corrected by the pupils" (p. 49), and that "on the whole perhaps there is very slight indication that the false statements tend to be retained by the pupils as true statements to a greater extent than the true statements are believed by pupils to be false" (p. 52). The tests were repeated a month later and produced results consistent with those just described. The scoring of the papers seemed to benefit the brighter boys especially in the case of the multiple-choice questions, but not in the case of the true-false statements. An extension of the investigation to the subjects of spelling, algebra, and chemistry yielded data analogous to those in physics.

The investigation contributes little to the literature on the new-type test. At least part of the claimed superiority of the true-false test as an instructional device can be accounted for by differences in the methods employed in scoring the papers for the true-false and the multiple choice questions. The author's findings with respect to the relative harmlessness of false statements agrees fairly well with the conclusion of others on this point. No convincing new arguments are advanced for the use of the formula $S = R - W$ in scoring true-false tests. The previously published materials on the new-type test have been as thoroughly summarized and as critically reviewed elsewhere.

George Peabody College W. A. BROWNELL

The Process of Human Behavior. By MANDEL SHERMAN and IRENE CASE SHERMAN. New York, W. W. Norton & Co., 1929. Pp. 227.

The title of the book would lead one to expect a rather full and complete discussion of the entire field of human behavior, but as a matter of fact it deals almost entirely with infancy and early childhood. The experimental work upon which the book is based was carried out at Lying-In Hospital, Chicago; at the Wesley Memorial Hospital, Chicago; and in the Neuro-Psychiatric Clinic of Northwestern University. Some experimental work and clinical

experience at the Washington Child Research Center was used to compare the reactions of infants with those of older children.

The authors state in the introduction that, "an objective and biological approach to the study of human behavior is imperative." This is a very wholesome point of view to take and if persistently pursued should result in valuable data on the many problems of human conduct and behavior.

The first two chapters are devoted to the growth and significance of the nervous system. Nothing new or original is added in these sixty pages. In the third chapter the first human responses, of the sensori-motor type, are discussed. The authors seem to take the position that all coördinated activity grows out of early random activity as a result of experience and learning through constant stimulation. "The growth in adequacy of both the reflex and sensori-motor reactions, however, is related so closely to the amount of stimulation the infant receives that the existence of a developmental process similar to that noted in any act of learning seems evident." This is representative of the authors point of view throughout with respect to intelligence, emotions, and personality. In Chapter IV the authors say that "the first signs of intelligence in the newborn infant are manifested in sensori-motor responses, and the development of intelligence is directly related to the development of these reactions." Perhaps the most important part of the book is the chapter dealing with the observation of emotions. Through extensive experimentation with motion pictures of various emotional reactions of normal infants as well as through the observation of infants in the nursery, the authors demonstrate the inability of people to differentiate between the various emotions. The remainder of the book is given to a consideration of the nature of the emotions, the development of personality, and personality and social behavior. The proper kind of environment and training is made the sole bases for the explanation and control of these various processes of human behavior.

The book, in the opinion of the reviewer, is too dogmatic and uncritical to be of very great importance to the science of psychology. It seems to him that the experimental data are used merely to illustrate certain preconceived points of view.

Peabody College L. W. ALLISON

Youth and the Good. ANON. School Betterment Studies, Vol. 1, No. 2. Pittsburgh, Henry C. Frick Educ. Com., 1929. Pp. 76.

This bulletin is described as the report of an "experiment . . . to further test the soundness of the following hypothesis: Youth, of the high school age, is more susceptible to the influence of ideals than are persons at any other period of their lives. The higher the ideals the more strongly they grip boys and girls in their 'teens, and the more tenaciously are they held" (p. 9). The study is, however, no experiment at all in the scientific meaning of the term. It does not establish the special susceptibility of high-school pupils to moral education, nor does it prove their peculiar preference for the "higher" ideals.

Three speakers of national reputation addressed 10,000 students in ten Pittsburgh high schools. These students were then "asked to submit a brief account of their feelings and thoughts' during the addresses. Of the total

number of papers thus collected, 925 were analyzed. The results are reported first in a series of summary paragraphs. For example, 61 students frankly stated that they had expected to be bored, 93 were given credit for "an almost uncanny degree and correctness of perception of the personality of the various artists," 295 spoke of various technical excellences in the addresses, 90 wrote of the use of the "leaven of humor," and so on. The last 25 pages of the bulletin are devoted to excerpts quoted from individual papers to illustrate the nature of the students' comments. The general nature of the report can be inferred from the above quotations as well as from the liberal use of such terms and phrases as "little less than miraculous," "source of gratification and tremendous encouragement," "remove all ground of the fear for the intellectual and moral status," "hidden gold," etc. The complete absence of controls, of measures of reliability, and the like constitutes further evidence that the problem was approached in a popular rather than a scientific manner.

George Peabody College W. A. Brownell

Psychologie der Arbeitshand. By Fritz Giese. Berlin, Urban & Schwarzenberg, 1928. Pp. viii, 325, 166 illustrations.

The psychology of the hand at work is practically a new field so far as the behavior aspect of the processes involved are concerned. It has, of course, been studied somewhat in connection with other lines of investigation, like penmanship. The World War gave a great stimulus to scientific work in this field, particularly in as much as many disabled soldiers were obliged to take up new lines of activity whether with artificial hands or with hands that were disabled by cerebral injuries, and others had to regain normal controls that had been lost. The importance of investigations in this branch of psychology was forced on the author while he was serving as military vocational counselor in the war and also as director of the laboratory for persons with brain injuries in Halle (from 1919 to 1923). The present monograph seeks only to bring together the various methods that have been employed. It does not attempt to present a synthesis of results or a consideration of underlying theory. Norms, reliabilities, and intercorrelations of different performances are not given, the consequence being that a critical evaluation of any method used is not yet possible.

It is an interesting thing to consider that the lines of activity of the hand, and also the degrees of proficiency of such activity, are functions not only of the peculiar anatomy of the individual or group, but also of the "culture area" in which the individual operates. How much is the playing hand, as compared with the working hand, under the influence of the cultural background of the group? To what extent do differences in social strata affect the performances of the working and the playing hand? The hand activity is obviously a social not an isolated datum. The hand working alone is also quite a different problem from the two hands working in coöperation; in the latter case one hand usually has certain dominating tendencies over the other. Racial and genetic problems loom up at once as promising lines of study and as related more or less closely to the evolution of the hand from anthropoid and earlier background. There is also the ontogenetic development of manual activity and its decadence

in old age. Most of these problems are only suggested. It is necessary first to get at the more direct psychological study of the various activities of the hand, so as to develop methods and data for attacking the other problems. The investigation of the hand at work must and can be carried out in this fundamental way without any immediate application in view; but the phenomena to be investigated are in the actual activities of the hand at work not in artificially produced situations.

The large variety of topics covered range from the tying of knots, the use and operation of the various parts of artificial hands, etc., to such performances as violin playing, modification of handwriting in hypnotism, hand positions in massaging, and expressions of the hands in finger languages. Various performance tests of skill are included, together with numerous illustrations of apparatus and positions of the hands and arms.

A book of this sort is necessarily superficial in its handling of any particular subject included in its wide range, and it is to be noted that the hand cannot be treated without the inclusion of various postures, bodily activities and even the more abstract mental operations. J. P.

Human Behavior. By WALTER S. HUNTER. Chicago, Univ. of Chicago Press, 1928. Pp. x, 315.

The primary interest in the present volume lies in the fact that it represents a decided change in viewpoint on the part of the author since the first edition was printed under the title *General Psychology*. It therefore suggests a pertinent question concerning the general trend of psychology. In 1919 Hunter wrote, "From the theoretical standpoint our position is one of a combination of behaviorism and structuralism. I see no need for forcing the subject-matter into one or the other mold. Neither is large enough alone. Psychologists study both consciousness and behavior that does not involve consciousness." In the revised book he writes, "the subject matter of anthroponomy (the writer's new name for psychology) is behavior." "By behavior is meant the muscular and glandular activities of the organism." It is interesting to ask how far this change of attitude is spreading among psychologists.

This question implies a deeper problem. Supposing that this change of attitude is becoming general, does it bespeak a development of psychology in the direction of natural science? Does the description of psychological phenomena in terms of muscles and glands give us an adequate statement of them?

It is well known what often happens when an attempt at such a description is made. The nervous system is endowed with properties and functions that take the place of psychic states. What we have then is clearly a verbal transformation of something that appears objectionable into something more agreeable, though hardly more scientific. So the question arises whether the descriptive and explanatory demands of science are met by such a procedure.

Does there really exist a "connection of neurons, such as underlies fear or anger?" Just what is the nature of the fear or anger under which this connection lies? Although we may agree that psychology profits from turning away from "mysterious elements which cannot be verified by science," we wonder whether

the reduction of psychological phenomena to muscular and glandular activities does not create a demand for such scientifically inverifiable elements.

As we have already indicated, Hunter desires to replace the term psychology by anthroponomy. It is interesting to note that according to this terminology animal psychology becomes phyologenetic anthroponomy.

Indiana University J. R. KANTOR.

Die Psychologie der Leibesübungen. By R. W. SCHULTE. Berlin, Weidmannsche Buchhandlung, 1928. Pp. xiv, 120.

This monograph advances a theory of bodily exercise which departs from the usual concept of practicing only certain systems of muscles in the human organism. It stands in its position midway between the idea of athletic exercise whose aim is greater and more efficient control of the body and the various forms of eurhythmics which emphasize more adequate expression of thoughts and feelings. Starting from the hypothesis of a psychophysical monism akin to the double aspect theory of the mind-body relationship, it emerges with a discussion of exercises that are adapted to the psychophysical requirements of the individual as well as to the generalized forms that are more suitable to the race. There are certain rhythms which fit the mind as well as the body. The writer consequently deplores the practice of formulating the athletic training of women according to the standards adapted to the male body and mind. The discussions are very lucid and in most instances are clearly inspired by a mental attitude that is in keeping with modern psychology. Even such phases as introversion and extraversion are brought to bear on the problem of mind-body exercises. The chapters separately discuss such questions as the teleology, biological development, psychophysiology, and mental motivation of the bodily movements. Other topics are: phylogenetic ontogenetic and historical mental growth, *Struktur*, systematic and educational psychology as applied to and illustrated by athletic exercise. Appended to the book are a rather thoroughgoing bibliography and some forty pages of plates illustrating various forms of exercises, and many ingenious types of apparatus, not a few of which would be very serviceable in the general psychological laboratory.

University of Iowa CHRISTIAN A. RUCKMICK

Aids to Psychology. By JOHN H. EWEN. New York, William Wood & Co., 1929. Pp. vii, 166.

This book, by an assistant medical officer in a mental hospital, is based chiefly on a few standard English and American psychology texts (some of them rather old), and its aim is to offer students a means of rapid review. It attempts especially "to emphasize those points useful to students studying for a diploma in Psychological Medicine." The 24 short chapters are all on topics of traditional normal and abnormal psychology, beginning (after an introduction) with Body and Mind, except that the final chapter of 3 pages is on Intelligence and Intelligence Tests. The point of view of much current English psychology, with early emphasis on consciousness, cognition, conation, apper-

ception, etc. is emphasized. Each chapter consists usually of classifications and short uncritical summaries, definitions, distinctions, synopses in the form of tables, etc. "The Instincts" are thus tabulated as to name, related emotion, goal, etc. in a 4-page summary. Another 2-page table gives a similar synopsis of various forms of action. Here again instinctive action forms one of the regular classes, simple reaction, reflex action, automatic action, habit, and impulsive action being other classes. Imitation is treated in a whole chapter and is described as "a conation which seeks satisfaction in the imitative impulse itself" (p. 113). McDougall's view is prominent throughout. There are no references but usually the authority is given in parenthesis. The little volume is of convenient size, and is compact and very clear; but it will certainly conduce to cramming on old terms, classifications, etc. Its assumed purpose, however, is to aid in review, not to serve as a text.

J. P.

The New Citizenship. By SEBA ELDRIDGE. New York, Thomas Y. Crowell Co., 1929. Pp. 349.

The mind of the citizen is the product of numerous environmental forces not now under control. In consequence citizens are not sufficiently interested in politics and government. They suffer without knowing the mechanism which causes their own hardships and handicaps. Also democracy is failing. These forces surround the individual in his home, his childhood, his school and all his relationships during growing and adult years.

Adults are the important source of influence in making the minds of children, who will presently be the adults. To give to children a better direction of interests in the principles of good government, the author proposes an organization of the rank and file of citizens in community centers. Several experimental centers might first be established in order to test the question of the possibility of any democracy. But taking the answer for granted, the author proposes a wise selection of leaders who shall vivify the opportunities and obligations of citizenship to the mass of citizens so that they may develop in their children the proper interest in the principles of good citizenship and good government. We should thereby take additional steps toward the new citizenship.

There is much that is interesting in this discussion of ideals. The difficulty, which the author foresees but does not answer, however, is, how to secure an altruistic cooperation from any considerable number of citizens, and especially how to secure an agreement among them.

New York City — ARTHUR H. SUTHERLAND

Research in the Social Sciences: Its Fundamental Methods and Objectives. Edited by WILSON GEE. New York, The Macmillan Co., 1929. Pp. x, 305.

This is a series of lectures given in the University of Virginia under the auspices of the recently established Institute for Research in the Social Sciences. The chapter headings and the lectures in order are: Sociology, by Robert E. Park; Economics, by Allyn A. Young; Anthropology, by Clark Wissler;

Statistics, by R. E. Chaddock; Psychology, by R. S. Woodworth; Jurisprudence, by Roscoe Pound; History, by Arthur M. Schlesinger; Philosophy, by John Dewey; and Political Science, by Charles S. Beard. The sub-title indicates the general nature of the treatment of each subject. The lecturers on the several subjects were selected by the corresponding departments of the University. This book constitutes the fourth volume of its kind published in America since this series of lectures was projected in 1926, during which period several other very significant developments in this country have taken place in the way of coöperation and mutual inter-stimulation of the various social sciences. With two or three exceptions, very little is given in this volume in a definite way as to "fundamental methods," thus reflecting, probably, a chief defect in this group of scientific endeavors at the present time. Most of the lectures, however, afford valuable suggestions and insights as to the principal research leads in the fields which they cover. To specialists in any one line there is probably very little that is new in the book, but even the expert may be interested in the presentations by leaders in lines related to his own. Some of the lectures are verbose, indicating little specific preparation on the part of the lecturer, although at least five of the chapters indicate very definite lines of growth and problems in their respective fields.

In general, psychology is recognized as of fundamental importance in social science work, but, of course, its possible, specific contributions are not clearly seen. One lecturer, making the point that little is yet known in the general field of the social sciences, plays up rather facetiously the differences of mere opinion among psychologists as to the relative importance in behavior of factors like heredity and environment, neglecting the technical methods employed. He seems, also, to suppose that 'intelligence' is largely an arbitrary factor.

J. P.

Studies in the History of Statistical Method: with Special Reference to Certain Educational Problems. By HELEN M. WALKER. Baltimore. Williams & Wilkins Co., 1929. Pp. viii, 229.

A chapter is devoted to each of the following topics: The Normal Curve, Moments, Percentiles, Correlation, The Theory of Two Factors, Statistics as a Subject of Instruction in American Universities, and The Origin of Certain Technical Terms Used in Statistics. There is an appendix of a page and a half explaining that catalogues of the following universities were studied and stating the method used: Chicago, Clark, Columbia, Harvard, Illinois, Iowa, Michigan, Pennsylvania, and Yale. Data from this examination of catalogues were used mainly in the discussion of the sixth topic mentioned above.

In most chapters there is a brief discussion of the leading contributions to the development of the topic, with references both in footnotes and occasionally at the end of the chapter. The chapter on The Theory of Two Factors contains only some of the more important references, annotated. There is no other discussion. The tracing of statistics as a subject of instruction in America is admittedly neither complete nor entirely accurate in certain details, but a valuable nucleus around which further work may be done is afforded. Much

careful work has been done by the author, including extensive correspondence and conferences with numerous workers in statistics in various university departments from mathematics to agriculture. In the last chapter one finds references and notes on the origin of such terms as accomplishment quotient, coefficient of alienation, coefficient of variation, homoclisy, index of reliability, etc. A bibliography of 29 pages is given.

J. P.

L'intuition, la matière et la vie. By N. Lossky. Paris, Félix Alcan, 1928. Pp. 179.

This collection of three essays, translated from the Russian by M. Exempliarsky, contains the exposition of a dynamic theory of matter and a vitalistic theory of life developed from the platform of an intuitive theory of knowledge. "All knowledge is realized in the consciousness of somebody" and has for its distinctive characteristic the coördination of subject and object. Rejecting the views of correspondence and of construction, the author maintains that truth is attained when consciousness takes "possession" of an object, so that it is "present in consciousness," not as any sort of phenomenon, but literally. Truth and the object coincide or are identical, a tree, for example, being "immanent in the consciousness of the subject" but "transcendent with respect to the conscious subject." This relationship between subject and object is neither spatial nor temporal nor causal, but "purely theoretical," so that the object itself belongs to the trans-subjective world and may have a non-psychic or material existence. The pre-conscious coördination of subject and object is, however, not intuition, but only the preliminary condition for consciousness and knowledge, or the possibility of the "act of intuition." The objectivity of the judgment is guaranteed for the author by the fact that the objective side of knowledge consists of an object (a maple leaf), a content (yellow), and the relationship between them, that is, subject, predicate, and their necessary connection. On the basis of this view of knowledge there is developed an organic conception of the world not essentially different from that of Wundt and other voluntarists, although it is worked out with reference to more recent physical views. While agreeing with most of the results obtained, the reviewer holds that some problems, notably that of validity, are solved in advance by an "intuition" which is doubtless itself still somewhat problematical.

Vanderbilt University HERBERT SANBORN

The Questionnaire in Education: A Critique and Manual. By LEONARD V. KOOS. New York, The Macmillan Co., 1928. Pp. vii, 178.

This is a somewhat superficial and external analysis of the questionary in education, based upon investigations in seven educational journals and two monograph series. The book is an inquiry into the efficiency of this method of investigation, and is also planned to serve as a manual for investigators using the method. (Let us hope that it will not tend to increase the number of investigations by the questionary method!) There is little in the book of interest to psychologists, except the very high percentages of investigations still carried

out by questionaries in the different divisions of education. The author is somewhat, but not sufficiently, critical of the method as a means of getting data in education. While he advises that other methods be used in all cases to which they are applicable, he probably fails to impress the student in education sufficiently with the fact that many 'researches' by the questionary method are not worth carrying out. One reason why progress has been so slow in education, as compared with the experimental sciences (including present-day psychology in some of its branches, at least), is doubtless that investigators are too anxious to get results quickly and so resort to poor methods Fourteen recommendations relative to the use of the questionary method are given in the last chapter of the book, and the recommendations adopted in four large psychological laboratories, and published by Boring (this JOURNAL, 37, 1926, 632-633) for persons who receive questionaries, are quoted. No adequate inquiry into the selectiveness of respondents to questionaries is made, and the treatment of the validation of data obtained by this method is superficial. The reviewer believes that an article could have incorporated the main substance of the book.

J. P.

The Unique Status of Man. By HERBERT WILDON CARR. New York, Macmillan Co., 1928. Pp. 216.

A course of lectures delivered before the School of Religion of the University of Southern California: Chap. I, The theological form of the free-will problem; II, The metaphysical form of the free-will problem; III, Empiricism and the rise of the idea of a natural religion.

Professor Carr indicates at the end of each problem how the metaphysical and religious thought of the time is related to free-will. "We (from Hume) may see in it almost a prophetic anticipation of that complete divorce of science and religion which will distinguish the new materialism." The concept of free-will arose with Christianity. "It was through Christianity that the idea that the human being is actually free came into the world. According to Christianity, the individual *as such* has an infinite value as the object and aim of divine love, destined as mind to live in absolute relationship with God himself, and have God's mind dwelling in him." This book should interest those dealing with the psychology of religion.

Ohio State University A. P. WEISS

Fitness for Work. By T. H. PEAR. London, University of London Press, 1928. Pp. 187.

In this small volume of six chapters Professor Pear presents in a novel manner certain interesting phases of modern applied psychology.

In the first chapter he carefully distinguishes by definition between such concepts as capacities, abilities, skills, and motives. The relationship between each of these concepts and habit is considered. The term habit is not used by the writer in the inclusive sense so common in America. He says, indeed, (p. 48) "A first class automobile driver's adaptive behavior in traffic makes the

ordinary amateur look like a bundle of habits which some pessimists declare man to be." The author similarly develops a verbal distinction between habit and intelligence and between intellect and skill.

The rôle of motives in work and play is next considered. Motives are held to issue from a variety of human dispositions, many of which are considered at a verbal rather than at a physiological or psychological level. The nature of laziness and the psychological aspects of work are considered in turn. The final chapter considers the problem of whether or not industrial skill is worth while in English society as it is now organized. The nature of the problem raised in this chapter is one of value. Most of its pages are therefore out of the strict realm of psychology. In its own right, however, the political and social analysis is interesting.

The volume is written in a brilliant style. It is well indexed and of real value to all those who are interested in the broader aspects of applied psychology.

Brown University LEONARD CARMICHAEL

Des vérités éternelles chez Descartes. By ÉMILE BOUTROUX. Paris, Félix Alcan, 1927. Pp. 146.

The volume is made up of an introductory exposition of Boutroux's philosophy by Leon Brunschwicg and the French translation by M. Canguilhem of Boutroux's thesis in Latin on the eternal verities. This thesis "had a decisive influence on the destinies of French thought" in so far as it did away with "the a priori concepts of an illusory positivism and a systematic empiricism." The discussion of Boutroux's philosophy is taken up under the three captions: The Philosophy of History and History; The Philosophy of Science and Science; and Dogmatic Rationalism and Reason, in all of which Boutroux's emphasis on the individual and contingent, in opposition to an extreme Hegelian view, is clearly pointed out.

Just as the *esprit de corps* tends to prejudice the proper development of individuals, so the *esprit de système* tends to denature and pervert ideas, retaining or emphasizing, in given philosophies, merely those ideas which fit into some preconceived scheme. Thus Socrates is taken to be preëminently a logician instead of the ethical philosopher Boutroux considers him to be. In the second part of the volume Boutroux interprets the philosophy of Descartes from this same point of view, finding him to be much more than the philosophical exponent of mathematics and physiology he has sometimes been thought to be; but into the details of this the reviewer cannot go here.

Vanderbilt University HERBERT SANBORN

An Analysis of the Downey Will-Temperament Test. By RICHARD S. UHRBROCK. New York, Bureau of Publications, Teachers College, Columbia University, Contributions to Education, No. 296, 1929. Pp. 78.

This study is divided into two parts: an historical part in which Uhrbrock summarizes the work that preceded and led up to the formulation of the Downey Will-Temperament Tests; and an experimental part in which he reports the results of his own investigation with 151 junior high school boys.

Vassar College LOVISA C. WAGONER

The New Leaven: Progressive Education and its Effect upon the Child and Society. By STANWOOD COBB. New York, John Day Co., 1928. Pp. x, 340.

A rather detailed account of the principles and methods which are being worked out in certain private schools of America is given here. While the book includes much rather frank propaganda, it should be of some interest to educational administrators and to intelligent parents. It has but little direct bearing upon the field of psychology.

Institute of Child Welfare — FLORENCE L. GOODENOUGH
University of Minnesota

Exercise Manual in Statistics. By K. J. HOLZINGER and B. C. MITCHELL. Boston, Ginn & Co., 1929. Pp. v, 160.

The authors have prepared a manual to supplement the usual text in statistics in order to furnish the necessary materials for much more extended practice in calculations than the ordinary text affords. The problems, graduated in difficulty, are of many varieties, such as computation of measures of central tendency, variability, correlation, etc.; construction of graphs (including curve fitting); and problems involving the use of tables. The exercises deal almost entirely with data derived from mental, educational, and anthropometric measures.

Cornell University — FRANK S. FREEMAN

The Inferiority Feeling. By WILLIAM S. WALSH. New York, E. P. Dutton & Co., Inc., 1928. Pp. 381.

This book is written for the general, lay reader, and follows Alfred Adler in emphasizing the important ramifications of feelings of inferiority in the development of character and the determination of happiness or unhappiness in everyday life. It comprises sixteen chapters, dealing respectively with topics as follows: The Inferiority Feeling, Influence upon Life in General, Complex and Conflict, Mental Adjustments, Overcompensation and Undercompensation, Introverts and Extraverts, Sources of Inferiority Feelings, Other Sources, The Way Out, Combating Inferiority, Getting Along with Others, Decision, Sensitiveness, Personal Appearance, The Sense of Guilt, and Courage. Its bibliography contains references to only eighteen books and three magazines. If relative importance, for the author, is indicated by relative space, the sources of inferiority feelings and of complexes is his most important topic. The sources or causes range from inherited physical and mental defects, through acquired ailments, to the complexes of one's parents, social ridicule and unfavorable contrasts, favoritism, harsh and inconsistent and too indulgent methods of correction by parents, and over and under attention to children by parents and guardians. Like most books in abnormal and child psychology, it lays down no mathematical, or otherwise definitely determined, threshold, standard, or helpful dividing line between injurious and beneficial measures to be adopted by parents: the parents are condemned for too much and too indulgent [sic] love and attention; likewise, they are castigated for harshness, and for inattention and neglect. Where does the one leave off and the other begin?

The author, however, cautions us that the inferiority feeling is seldom, if ever, the sequel and development from merely one cause, and holds that it is the subject's attitude toward his real or imagined infirmities, rather than these infirmities themselves, which is in essence the complex or the more direct counterpart of it. Like most Freudians and psychoanalysts, and more lately *conditionists*, the writer attempts to trace inferiority feelings and complexes back to their inception in and development from incidents of childhood. While his book contains nothing essentially new in content or point of view, and is of little value to the psychologist, it may be read with profit by the laity.

University of Oregon — H. R. Crosland

The Psychology of Personality. By P. F. Valentine. New York, D. Appleton & Co., 1927. Pp. xi, 393.

The author discusses in this book methods of measuring and controlling personality, which he defines as "the sum total of one's habit dispositions." While granting that the present fund of knowledge regarding personality is meager and incomplete, he nevertheless maintains that a certain amount of control may be exercised over the development of personality.

The book is primarily for the layman. It is interestingly written and is very readable. The professional psychologist, however, will find little if anything that is new within it.

University of Iowa — W. G. Piersel

De la nature affective de la conscience. By D. Bertrand-Barraud. Paris, J. Vrin, 1927. Pp. 155.

The thesis of this book is that thought is fundamentally affective rather than rational. The book is well written, and includes an interesting discussion of dreams. Unfortunately the terminology is psychologically vague and the data adduced in support of the thesis are for the most part retrospective reports by the author himself or statements culled from the belles-lettres.

Harvard University — J. G. Beebe-Center

Progressive Scholasticism. By Gerardo Bruni. Translated by John S. Zybura. St. Louis, B. Herder Book Co., 1929. Pp. xxxviii, 185.

This is a translation of the author's *Riflessioni sulla Scholastica* (Rome, 1927) which has been revised and enlarged for the benefit of English readers. Despite the fact that the book deals with aspects of mental life of interest to psychologists, the general method of treatment adopted makes it of little value to them. — J. P.

The New Morality. By Durant Drake. New York, The Macmillan Co., 1929. Pp. 359.

This book is simply a popular treatment of the subject considered, and has little of real value for psychology.

Peabody College — S. C. Garrison

New Stanford Achievement Test. By T. L. KELLEY, G. M. RUCH, and L. M. TERMAN. Yonkers, World Book Co., 1929.

This is a revised form of the Stanford Achievement Test of 1925. The 1929 revision consists of two primary examination forms (grades 2-3), two advanced examination forms (grades 4-9); with directions for administering and scoring each, and a guide for interpreting.

Cornell University FRANK S. FREEMAN

Fortschritte der Sexualwissenschaft und Psychoanalyse. By WILHELM STEKEL, and thirteen collaborators. Leipzig, Franz Deuticke, 1928. Pp. 195.

This collection of sixteen papers dealing with psychoanalytic topics is published as a *Festshrift* in honor of the sixtieth birthday (March 18, 1928) of Dr. Wilhelm Stekel. The contributors, in addition to Dr. Stekel, who has three articles, including the opening one on Fortschritte der Traumdeutung, are the following: Anton Missriegler, E. Gutheil, Samuel Löwy, Max Friedemann, Hugo Sonnenschein, E. Bien, Ernst Rosenbaum, Sándor Feldmann, M. Lachtin, E. Tremmel, B. van Tricht, S. Plahner, K. G. Heimsoth.

University of Illinois HERBERT WOODROW

Education of Mentally Defective Children. By ALICE DESCOEUDRES. Translated by Ernest F. Row. New York, D. C. Heath & Co., 1928. Pp. 313.

After a brief discussion of the methods employed in distinguishing normal children from those requiring special class training, the author describes in a few pages the types of classes and schools established for defectives, the training of teachers of special classes, and the social consequences of special class training. By far the major portion of the book is devoted to an analysis of methods employed in teaching defective children handwork, drawing, speech, reading, spelling, and arithmetic. There are also chapters dealing with the training of the sense-modalities, attention, and physical and moral development. The volume is valuable chiefly for its detailed description of methods and materials employed with success by the author herself in her own classes of defective pupils. It should prove to be a suggestive and stimulating handbook

Cornell University FRANK S. FREEMAN

The Mechanics of the Mental Life; an Introduction to the Study of Physiological Psychology. (Russian). By N. A. KABANOV. Moscow, 1928. Pp. 160.

According to a comment made by the author the book seeks to establish a neural basis for social and moral conduct. The author believes that the method of conditioned reflexes here offers a useful approach to the functions of the cortex; a method capable, as he thinks, of showing that social and ethical conduct is as inevitable under certain conditions as is the secretion of saliva at the sight of tempting food. Although of different orders, these phenomena are, he contends, of the same nature. One of the chief topics in the book is the evolution of the more complex emotions, and especially of the social emotions.

M. B.

An intellectual lifework appealed strongly to Ebbinghaus at the time. He therefore planned to return to Berlin to prepare himself further for an academic position. These plans did not materialize, however, for we find him in England and France as a student and teacher from 1875 to 1878 and acting as French tutor to a Prince Waldemar during 1879.

In 1880 he habilitated himself at Berlin. While there, he founded the laboratory[6] and in 1885 published *Über das Gedächtnis*. He was made *ausserordentlicher Profesor* in the same year, probably in recognition of the latter accomplishment. In association with Arthur König he founded the *Zeitschrift für Psychologie und Physiologie des Sinnesorganes* in 1890. Breslau called him in 1894 to become a full professor in the chair left vacant by Theodor Lipps' going to Munich to replace Stumpf who was bound for Berlin. At Breslau Ebbinghaus again founded a laboratory. In 1905 he had a call—this time to Halle—to succeed Alois Riehls who was going to Berlin. He remained there as Professor of Philosophy until his sudden death from pneumonia on February 26, 1909, just a month after he had entered his sixtieth year.

Ebbinghaus found his own way to psychology and beat his own path. None of his instructors determined, in any marked way, the direction of his thinking, despite the fact that among them there were such eminent individuals as J. E. Erdmann, F. A. Trendelenburg, and J. B. Meyer. On the other hand, the com-

[6]Certainly before 1891; "in the late eighties," says J. McK. Cattell, Early psychological laboratories, *Science*, 67, 1928, 546. Wundt founded the Leipzig laboratory in 1879. The formal date for G. E. Müller's laboratory at Göttingen is 1888, and Müller has said that it was the first laboratory in Prussia. Münsterberg's experimental researches at Freiburg, which began in 1887-88, had to be done in rooms in Münsterberg's own house; cf. Margaret Münsterberg, *Hugo Münsterberg, His Life and Work*, 1922, 27. There was nothing at Halle, where Stumpf used the cathedral organ for his work on tonal fusion; Stumpf, however, dates the rudimentary laboratory at Munich from his advent there in 1889: cf. C. Stumpf, in R. Schmidt, *Die Philosophie der Gegenwart in Selbstdarstellungen*, 5, 1924, 217. Lipps had an unofficial laboratory at Bonn in space loaned by the physicists, and it probably dates from about 1887; cf. W. O. Krohn, this JOURNAL, 4, 1892, 592 f. This same article describes Ebbinghaus' laboratory at Berlin, pp. 589 f. There were no others in Germany. It is plain that Ebbinghaus was experimenting at Berlin before there were formal or informal laboratories elsewhere in Germany except at Leipzig. If Göttingen really antedated Berlin, then Ebbinghaus' laboratory was the third in Germany, except for Münsterberg's and Lipps' unofficial laboratories at Freiburg and Bonn. Stumpf's account of the founding of the Berlin laboratory with his advent in 1894 is therefore misleading; cf. Max Levy, *Geschichte der königlichen Friedrich-Wilhelms-Universität zu Berlin*, III, Halle, 1910, 202-207. (I am indebted to Professor Boring for these data.)

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HERMANN EBBINGHAUS

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Hermann Ebbinghaus[1] was born just about nine months (January 24th) before the famous October morning in 1850 when Fechner had his vision of the principle of Weber's law.[2] He was the son of a merchant, Carl Ebbinghaus, in the town of Barmen,[3] Germany. Of his infancy and childhood I have been able to discover nothing except that he was brought up in the Evangelical faith and attended the town *Gymnasium* until he was seventeen. In 1867 he went to the University of Bonn, and somewhat later to Berlin and Halle.[4] Although his initial interests were history and philology, he was gradually drawn into philosophy. The outbreak of the Franco-Prussian War in 1870 interrupted his university work. He joined the Prussian Army in which he remained until the spring of 1871, when he returned to Bonn to continue his philosophical studies. On August 16th, 1873, he obtained his Ph.D. with the dissertation, *Über die Hartmannsche Philosophie des Unbewussten*, after a "glanzend bestandenem Examen."[5]

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[1]This paper is a revision and condensation of a paper presented in Dr. E. G. Boring's Seminary on the History of Experimental Psychology, at Harvard University, April 2, 1928. I wish to acknowledge with thanks the interest and suggestions of Professor Boring.

[2]Cf. E. G. Boring, *A History of Experimental Psychology*, 1929, 270.

[3]Close neighbor of Elberfeld, famous in psychological circles for some of its equine inhabitants.

[4]An attempt to determine the exact time spent in each of these places has thus far been unsuccessful. An odd issue of the Berlin Register for 1867-1868, which I have unearthed, does not contain Ebbinghaus' name.

[5]Karl Marbe, *Frankfurter Zeitung*, Literaturblatt vom 7. Marz, 1909. This estimate is not based on personal observation as Marbe was only four years old at the time.

Ebbinghaus.

bination of philosophical and scientific points of view, which he found in Fechner, a copy of whose *Elemente* he picked up in a Parisian second-hand bookstall, influenced him greatly. This debt he acknowledged in his dedication of the *Grundzüge* to the memory of Fechner. Gracefully put in verse, it ends, "ich hab'es nur von Euch."

One *Leitmotif* runs through his work from his very first publication: psychology is *Naturwissenschaft*. The first thesis in his dissertation (1873) sets forth the proposition: "Psychology (in the broadest sense) belongs no more to philosophy than does natural science."[7] In *Über das Gedächtnis*, in a discussion of the possibility of an experimental approach to the memory problem, he says, "at the very worst we should prefer to see resignation arise from the failure of earnest investigation rather than from the persistent, helpless astonishment in the face of their difficulties."[8] Thus he spent his energies in an attempt to establish psychology on a quantitative and experimental basis.[9] His estimate of the value of experiment for psychology is well brought out in the short historical sketch which introduces his *Abriss der Psychologie*. "When Weber in 1828 had the seemingly petty curiosity to want to know at what distances apart two touches on the skin could be just perceived as two, and, later, with what accuracy he could distinguish between two weights laid on the hand, ... his curiosity resulted in more real progress in psychology than all the combined distinctions, definitions, and classifications of the time from Aristotle to Hobbes (inclusive)."[10] The desire to bring into psychology clear and exact methods resulted in unusual carefulness in experimental technique and considerable interest in apparatus.[11]

Ebbinghaus was an unusually good lecturer. His ever-youthful personality, his natural humor, and the unusual clarity and ease

[7]Ebbinghaus was merely making articulate the attitude finding expression in others at the same time (*e.g* Wundt, in the first edition of his *Physiologische Psychologie*). The seeds sown by E. H. Weber, Fechner, Johannes Müller, *et al.*, were sprouting fast.

[8]English trans., 6.

[9]In this connection it is interesting to note a criticism by him (in *Zsch. f. Psychol.* 3, 1892, 201) of an article by W. T. Harris on "Fruitful lines of investigation in psychology" published in the *Educational Review*, 1, 1891, 8-14. In this article Harris warned educators against the materialistic psychology, pointing out that only in a study of the soul is psychology of value to education. Ebbinghaus ends his short abstract with the caution, "Let us hope that people will be warned and stay away from such a wicked science."

[10]English trans., 17.

[11]Cf., *e.g.* Bibliography, No. 19, *infra*.

of his presentation assured him of an overflowing audience and of considerable influence on his auditors. At meetings of the *Psychologische Gesellschaft* these characteristics and the wide range of fields at his command made him a leading participant in discussions. Another outstanding trait, which was particularly valuable in his rôle of editor of a journal, was his Jamesian tolerance.[12] The special importance of permitting the expression of the most diverse opinions in a young science was taken for granted by him. He seemed even to encourage sincere opposition, especially in younger men. In personal contacts with his students, he invariably manifested the deepest interest and affection, especially in the discussion of their experimental problems.

In view of the unusual attractiveness of this man it is surprising that the number of his disciples is so small. Jaensch accounts for this by the fact that Ebbinghaus had no desire to develop disciples.[13] He never urged another to undertake an investigation; in fact, if one wanted to work with him one had to obtrude one's self upon him with firmness.[14] Some of his better known pupils are A. Wreschner, L. W. Stern, and O. Lipmann.

Besides his courses in various branches of psychology, Ebbinghaus continued throughout his pedagogical life to give lectures in philosophy and in aesthetics. These were, however, only of minor interest to him. Psychology was by far his major interest. He either founded or developed the laboratories at all of the universities at which he taught. He founded the laboratory at Berlin, but evidently he did not get very much support from the University, for Krohn reported in 1891 that Ebbinghaus had two rooms with little apparatus.[15] When he went to Breslau in 1894 he founded a laboratory there.[16] At Halle, to which he was called in 1905, he enlarged and developed the very modest laboratory which he found there. Stumpf had been there from 1884-1889, but he had evidently done nothing in the way of founding a laboratory.[17]

[12]Boring has called Ebbinghaus the 'James of Germany' (with the reservation: 'if there can be another James anywhere!') The similarity of these two men in a number of respects is indeed striking and worthy of special study. Cf. E. G. Boring, *op. cit.*, 383-385.

[13]E. R. Jaensch, Hermann Ebbinghaus, *Zsch. f. Psychol.*, 51, 1909, i-vii.

[14]Compare in this respect, Cattell's accounts of Wundt's method, *Psychol. Rev.*, 28, 1921, 156; and *Science*, 67, 1928, 545.

[15]Krohn, *op. cit.*, 589-590. Cf. also Stumpf's account in Max Levy, *loc. cit.*

[16]Note in this JOURNAL, 7, 1895-1896, 152.

[17]Krohn found none there in 1891; *op. cit.*, 592.

Ebbinghaus did psychology an important service in founding and editing the *Zeitschrift*. The fifty volumes published up to his death present a practically complete portrait of the psychology of the two decades from 1890 to 1910.[18] Since Ebbinghaus was entirely free of schoolishness the researches and opinions of every group found its way to his columns. A glance through the index of names and studies is enough to convince one that the *Zeitschrift* was the most important psychological organ in Germany, and for that reason, in the world.[19] Helmholtz, Stumpf, G. E. Müller, Schumann, Th. Lipps, Von Kries, are only a few of the names which strike one in a cursory glance at the contents of a few volumes.

Since this paper is concerned solely in tracing the intellectual and personal history of Ebbinghaus and not in expounding his work and theories, only his more fundamental attitudes and the more general nature of his contributions will be treated in the following discussion.

Compared to Wundt, Ebbinghaus was an amateur in the matter of publication.[20] His published work is comparatively small. His own point of view with regard to print is expressed in a passage quoted by Woodworth,[21] "the individual has to make innumerable studies for his own sake. He tests and rejects, tests once more and once more rejects. For certainly not every happy thought, bolstered up perhaps by a few rough and ready experiments, should be brought before the public. But sometimes the individual reaches a point where he is permanently clear and satisfied with his interpretation. Then the matter belongs to the scientific public for their further judgment." How serious Ebbinghaus was in this attitude is shown in the matter of his memory experiments. Although they were completed in 1880, he did not report the results until 1885 after having repeated them in their entirety in 1883.[22]

Von Hartmann's *Philosophie des Unbewussten* had come out in 1869 and had achieved numerous editions within a few years.

[18]With the possible exception of the Wundtian School products, which were being published almost entirely in *Philosophische Studien*.

[19]Cf. Karl Bühler, *Die Krise der Psychologie*, 1927, 1-2.

[20]May this be partially accounted for by the fact that Ebbinghaus probably had no typewriter? Cf. Cattell, *Science* (*loc. cit.*), and Dürr's Introduction to Ebbinghaus' *Grundzüge* in which he refers to the "manuscript" he found.

[21]R. S. Woodworth, Hermann Ebbinghaus, *J. Philos.*, 6, 1909, 255. I have been unable to trace the source.

[22]English trans., 33.

It was therefore entirely natural that it should be a popular subject for dissertations among doctoral candidates of the period. Ebbinghaus' treatment of it was very critical, as was to be expected from one who, in the same dissertation, defended theses setting forth the essential similarity of psychology and the natural sciences and the abstract and verbal nature of the then-existing psychology. "Wherever the structure is touched, it falls apart,"[23] and "What is true is alas not new; the new not true,"[24] are some of the conclusions at which he arrived.

Über das Gedächtnis: Untersuchungen zur experimentellen Psychologie was published in 1885. It is to be deplored that Ebbinghaus left no record[25] of the period preceding the memory work. In the introduction to the section on nonsense syllables he made the bare statement, "In order to test practically, although only for a limited field, a way of penetrating more deeply into the memory processes . . . . I have hit upon the following method,"[26] and he went on to discuss the nature and mechanics of nonsense syllables.

Let us, with the few facts and the considerable conjecture at our command, attempt to reconstruct in at least a sketchy way the background for this 'hit.' Accepting as authentic the facts given in the short biographical accounts available, we know that in 1873 he received his Ph.D. We have no record of what he did between 1873 and 1875, except that he had planned to go to Berlin for further work—which he may have done. From 1875 to 1878 he taught, travelled, and studied in England and in France. During part of 1879 he tutored a young prince in French.

According to a statement in *Über das Gedächtnis* the work was first done in 1879 and 1880, with a "long time" of preliminary experimentation.[27] We know, therefore, that Ebbinghaus must have 'hit' on his method *before* 1879. Now to get at the other limiting date! Jaensch tells of Ebbinghaus having picked up a copy of Fechner's *Elemente* in a Parisian second-hand bookstall, which, Jaensch says, inspired the memory work.[28] This seems likely to have been some time during the years 1875-1878; and it

[23] *Über die Hartmannsche Philosophie des Unbewussten*, 59.
[24] *Ibid.*, 67.
[25] At least no published record that I have been able to discover.
[26] English trans., 22.
[27] English trans., 33.
[28] Jaensch, *op. cit.*, v.

also seems reasonable to suppose that this was Ebbinghaus' first acquaintance with the *Elemente* and perhaps Fechner, for it does not seem likely that Jaensch would have mentioned this find if Ebbinghaus had not attached considerable importance to it. Ebbinghaus' telling of the fact was evidently more than the bibliophile's boast of a bargain picked up in a book, copies of which were probably fairly common in second-hand bookshops.

So it is likely that between 1875 and 1879, nonsense syllables, for use in memory work, first came into being.[29] That is about as near as we can safely come with the knowledge at our command. As to the exact process of genesis, we can only guess. Was it an invention in the commonly accepted sense of the term, that is to say, deliberate? Or was it largely a discovery? What part did the gurgle of an infant, a transient regression to infancy, the reading of Jabberwocky,[30] the expletives of the Paris coachman or the London cabby, play? Probably more than Ladd and Woodworth allow for in the hypothetical account they give in their discussion of the reasoning process.[31]

What of the significance of *Über das Gedächtnis*? Previously, exact work had been limited to problems of predominantly physiological affinities. It is not that there was no interest in more strictly psychological fields,[32] but now a fundamental central function had been *experimentally* investigated! It *must* have meant a good deal to the young science, although comparatively little of

[29] Ebbinghaus was probably *eingestellt* at that particular time to the problem of memory which he got from the British Associationists. Cf. Boring, *op. cit.*, 380.

[30] "Through the Looking Glass" had been published in 1872, and had achieved considerable popularity. Perhaps Ebbinghaus acquired proficiency in English by reading Lewis Carroll!

[31] G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 604 f.

[32] In this regard it is of interest to compare some statements made by Clemens Baeumker in his *Selbstdarstellung: Philosophie der Gegenwart in Selbstdarstellungen*, II, 38-39: "From the study, especially of Wundt's *Physiologische Psychologie*, the idea grew on me to apply the experimental method to higher psychic processes, at first to memory. For a long time I undertook experiments for this purpose without finding, to be sure, the best means for it, methodically built syllables. Instead of these I used digits. . . . . But when in 1885 the pioneer work of Ebbinghaus on memory appeared, which by means of a methodical technique brought superlative results, in discouragement I let my own plans fall."

the contemporary effect is to be discovered in print.[33] James[34] was impressed with the "heroic" nature of the experiment, as was Tanzi.[35] Later opinion is probably fairly expressed by Titchener:[36] "It is not too much to say that the recourse to nonsense syllables, as a means to the study of association, marks the most considerable advance, in this chapter of psychology, since the time of Aristotle." Nevertheless, Titchener also thought that,[37] "the introduction of nonsense syllables . . . . has nevertheless done psychology, a certain disservice. It has tended to place the emphasis rather upon organism than upon mind."[38]

Between 1887 and 1890 Ebbinghaus published three papers[39] discussing the revision of the Weber-Fechner law and offering a physiological interpretation[40] which would account for the failure of the law to hold at the extremes.

In 1892 he set forth his *Theorie des Farbensehens*,[41] which on the whole was not favorably received. In his *Grundzüge* he gave very little space to it, evidently not taking it very seriously.[42]

Although Ebbinghaus was loath to enter into controversy, it was natural for him to take up the cudgels for psychology as he understood it, when Dilthey's *Ideen über eine beschreibende und*

[33]This may in large part be accounted for, perhaps, by the dearth of psychological journals. *Mind* and *Philosophische Studien* were the only more strictly psychological journals in existence. The former contains a review by J. Jacobs (*Mind*, 10, 1885, 454-459), which is very favorable. The latter (founded in 1883) carried no reviews. Besides this I have, however, been able to discover the following reviews:

Joh. Volkelt, *Zsch. f. Philos. u. philos. Krit.*, 93, 1888, 126-137 (very favorable).

A. Höfler, *Vjsch. f. wiss. Philos.*, 2, 1887, 340-351 (very favorable).

Ed. Schmidt, *Rev. Philos.*, 19, 1885, 687-693 (favorable).

Otto Flügel, *Zsch. f. exacte Philos.*, 14, 1886, 166-172 (favorable).

A. Elsas, *Philos. Monatshefte*, 23, 1887, 84-88. Elsas was sorry not to be able to criticize more favorably this very elaborate work undertaken in the true service of science, but felt that "not the author, but the school from which he comes, has to answer for the book." Cf. in this relation, Elsas, *Über die Psychophysik*, 1886.

[34]*Principles*, I, 1890, 676.

[35]*Rivista di filosofia scientifica*, 4, 1885, 598-600. "Una serie di ricerche, veramente digne d'un Certosino [A series of researches, truly worthy of a Carthusian monk]." The review on the whole is very favorable.

[36]*Text-Book of Psychology*, 1910, 380-381.

[37]*Ibid.*, 414.

[38]A statement, which would at the present time make for more general concurrence in the former judgment!

[39]See Bibliography, Nos. 3, 4, and 6, *infra*.

[40]James called this the most 'real' hypothesis of Weber's law on the neural side (*Principles*, I, 548).

[41]Cf. Bibliography, Nos. 8 and 9, *infra*.

[42]Vol. I, 1902, 260 f.

zergliedernde Psychologie appeared.[43] The keystone of Ebbinghaus' faith was being attacked! In an article in the *Zeitschrift*, *Über erklärende und beschreibende Psychologie*,[44] he justified the use of hypothesis and causal explanation in psychology.

Dilthey held that the new psychology could never be more than descriptive and that attempts to make it explanatory and constructive were wrong in principle and lead to nothing but confusion of opinion and fact. "We explain nature, but we understand psychic life;" and any psychology which was modelled after atomistic physics—as was a psychology such as Ebbinghaus'—could never "understand," for in the final analysis the process of "understanding" had to be experienced (*erlebt*) and could not be inferred logically (*erschlossen*).

Ebbinghaus in his reply claimed that, in so far as Dilthey was attacking explanatory psychology, he was attacking the old associationists, as to whose failure he agreed. Their difficulty arose, he felt, because of a use of the fields of chemistry and physics as fields of analogy rather than that of biology. Dilthey was not actually discussing the modern psychology: he talked of "the" explanatory psychology but was mostly using Herbart as an example—and Herbart was dead, even in Germany. Outside of Germany he never had had very much influence[45] because of his "metaphysical sophistries, unfounded fictions, and mythology." With Dilthey's point that explanatory psychology works on the principle that cause equals effect in the physical sense, Ebbinghaus disagreed. All that it can at the present say, and does say, is, "the contiguity of two sensations is considered as causal relationship because later a representation of one sensation results in a *Vorstellung* of the other." The controversy is not over as yet. As Klüver reports,[46] it is one of the outstanding features of the contemporary German psychological map.

In 1895, the school authorities of Breslau were interested in the advisability of holding five-hour school sessions. Ebbinghaus was

[43]W. Dilthey, *Sitzber. d. Berl. Akad. d. Wiss.*, 1894, 99. My acquaintance with it is second-hand through Eisler's *Lexicon* and Gardner Murphy's *Historical Introduction to Modern Psychology*.

[44]Bibliography No. 10, *infra*. As H. Klüver points out (Gardner Murphy, *op. cit.*, 445) many of the issues of the present discussion of "*verstehende*" psychology were first formulated in the Dilthey-Ebbinghaus controversy.

[45]Ebbinghaus is evidently referring here to his influence on psychologists rather than on educators.

[46]*Op. cit.*, 455.

one of a commission appointed to investigate this problem. His contribution was the *Combinationsmethode*. Although this device was not very successful in measuring intellectual fatigue, it was found to be very valuable as a measure of general intellectual capacity, since it correlated highly with the rank and scholarship of the pupils. Current adaptations of the principle involved are found in two such psychometric standbys as the Healy Picture Completion Test and the Trabue Sentence Completion Test.[47]

When Ebbinghaus died in 1909, the systematic treatise which he had started at the beginning of the last decade of the previous century was only a little more than half completed. The first half of Volume I had come out in 1897. This volume was first published as a whole in 1902, and a second edition of it followed in 1905. In 1908, the first section of Volume II (96 pages) appeared. On Ebbinghaus' death Dürr took over the editing of his works. He found about forty pages of manuscript, which broke off in the midst of a discussion of convergence and accommodation, and nothing more in the way of notes.[48] Dürr completed the second volume on the basis of his own views and the current psychological literature.[49] In 1911, sections 2 and 3, and in 1912, sections 4 to 7 appeared. The second volume had its first complete appearance in 1913, and the first volume achieved a third edition in 1911.

The *Grundzüge* was enthusiastically received,[50] which is not at all surprising, for besides a masterly, concise and clear presentation of the existing psychology there were numerous suggestive criticisms and improvements of current theories. On the whole, its contribution was in its readableness and in its general format rather than in any radical approach to psychology.

Abriss der Psychologie is the title of a two-hundred-page sketch of psychology published by Ebbinghaus in 1908. Originally written for Hinneberg's *Kultur der Gegenwart* as the contribution

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[47]In 1909 Woodworth (*loc. cit.*) said of the Ebbinghaus Completion Test: "Probably it has greater claims to be regarded as a test of intelligence than any other single test that has been introduced."

[48]Karl Bühler in personal conversation has said that Ebbinghaus kept few notes.

[49]Cf. Preface to Vol. II.

[50]M. Foucault, in *Rev. Philos.*, 55, 1903, 329-341, in a generally favorable criticism, talks of the section on Sensation as "vraiment magistrale!" McDougall, in *Mind*, N.S. 11, 1902, 578 f., in a review of the second half of Volume I, says it "bids fair to rank as the best general text-book for all classes of students, beginners not excepted."

on Psychology,[51] it was found too long, and was cut in half for inclusion in the volume. It was published as a whole as the *Abriss*. After the first edition on the average of more than one new edition came out every two years until the last one,[52] the eighth, in 1922. Dürr acted as editor of it for a time, and then Karl Bühler. An English translation by Max Meyer appeared in 1908, and French editions in 1910 and 1912. Its value and appeal may be judged by these facts. An admirable short sketch of the history of psychology introduces the volume, which begins with the oft-quoted, "Psychology has a long past but only a short history."

What has Ebbinghaus done for psychology? Where does he belong? He has contributed much and had great influence, mostly in an indirect way. *Über das Gedächtnis* is undoubtedly his outstanding contribution. His memory work was not only the starting-point for practically all the work which has followed in this field, but probably also for the work in the acquisition of skill. His *Grundzüge* comes next, not for its new system (which is very much like that of other psychologists who come to mind) but for its clear and concise treatment of the literature and its experimental emphasis.[53] His *Combinationsmethode* has meant a good deal to the field of mental testing. His editing of the *Zeitschrift* did much to advance psychology in a very productive period. His emphasis on experiment and his faith in the laboratory approach resulted personally in the establishment of at least two laboratories and the development of a third. His qualities as a lecturer and writer did a great deal to spread a knowledge of orthodox psychology.

Despite an early training in philosophy, he was one of the leaders in the movement to emancipate psychology from philosophy. He belongs fundamentally in the tradition which leads from pre-psychological science, through physiology, Helmholtz, and Fechner, to Wundt and 'content psychology.' Dunlap would give him, together with Aristotle and Binet, the credit for making

[51] Among other contributors were Wundt, Eucken, Lipps, and Riehl.
[52] At least that is the last of which I can find a record.
[53] Cf. Titchener in his Clark address, The past decade in experimental psychology, this JOURNAL, 21, 1910, 405: "I have sometimes thought that with allowances made for changed conditions, it [Ebbinghaus' *Grundzüge*] might prove as important for Experimental Psychology even as Wundt's *Physiologische Psychologie* or Brentano's *Psychologie vom empirischen Standpunkte*."

psychology "behavioristic,"[54] but that is probably going too far. His psychology, however, does have a functional emphasis which may be seen in such things as his constant reference to the biological affinity of psychology, his nativism in the matter of general attributes of sensation, and the contribution he has made towards the problem of individual differences.

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THE DEPENDENCE OF TONAL ATTRIBUTES UPON PHASE

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The experiments here reported bear upon experiential changes which accompany modifications in the phase of a sound-wave delivered simultaneously to the two ears.[1] The main problem in the research was to ascertain whether the several attributes of a tone are affected when sound-energy is delivered in unlike phase at the two ears. The obvious effect of phase-difference in binaural hearing is to divert the apparent location of the sound-source right or left from the median plane of the head. We sought to discover whether a change in attributive character also occurred with such a change in spatial location.

Observers were first trained to report in terms of the pitch, volume, brightness and intensity of tones of various frequencies. The observers were then presented binaurally with successive pairs of tonal stimuli which differed only in their phase-relations. They were under instructions to compare the tones with respect to one or another attribute.

The Experimental Study of Auditory Localization

The history of the study of auditory localization may roughly be divided into three overlapping stages; (a) the measure of accuracy of localization (perimetry) together with an analysis of place-errors, (b) the more analytical studies which grew out of theoretical explanations and which attempted to isolate the factors lying at the basis of localization, and (c) the assimilation of the experimental facts regarding localization to current theories of hearing.

(a) In the experiments of the first class, a single or dual sound-source was moved about, often in the open air. With a single source, the direction of the sounding object could be directly estimated and the error of the estimation measured. With a dual sound-source the actual binaural ratio of intensities could be better controlled; but in these earlier experiments the intensive differ-

*Accepted for publication December 1, 1929.
[1]The experiments were carried out at the University of Illinois.

ences were still complicated by other factors introduced by actually changing the position of the sound sources.[2] Localization was generally attributed to binaural intensive differences.

(b) With the clear-cut formulation of the 'intensity' theory[3] there came at intervals the discovery of at least three other sets of conditions aiding or determining auditory localization; differences in the complexity of the sound, differences in the phase-relations of pure tones at the two ears, and slight differences in the time of arrival of a sound at the two ears. Mach is usually credited with first holding that localization may be due primarily to clang-color. Later writers seem to have overlooked his reasons. He pointed out that with low tones the head will throw no appreciable sound-shadow, and hence localization cannot be due to intensive differences.[4] Considerable evidence now points to the importance of the complexity of the sound for accurate localization. Noises and clangs are much more accurately localized than are semi-pure or pure tones.[5] With monaural stimulation, where intensive, phasic, and temporal ratios at the two ears are absent, there is fair accuracy in the localization of noises, but pure tones are localized with practically chance accuracy.[6] This evidence has nevertheless strongly supplemented the 'intensity' theory; while the phasic and time-

[2]Typical studies are: Lord Rayleigh, Acoustical observations, *Phil. Mag.*, (5) 3, 1877, 456-464, and Acoustical notes (viii), *Phil. Mag.*, (6) 16, 1908, 235-246; A. G. Bell, Experiments relating to binaural audition, *Amer. J. Otol.*, 2, 1880, 169-180; H. Münsterberg & A. H. Pierce, The localization of sound, *Psychol. Rev.*, 1, 1894, 461-476; M. Matsumoto, Researches in acoustic space, *Stud. Yale Psychol. Lab.*, 5, 1897, 1-75; C. E. Seashore, Localization of sound in the median plane, *Univ. of Iowa Stud. in Psychol.*, 2, 1899, 45-54; A. H. Pierce, *Studies in Auditory and Visual Space Perception*, Longmans Green, 1901; J. R. Angell, A preliminary study of the significance of partial tones in the localization of sound, *Psychol. Rev.*, 10, 1903, 1-14; E. A. McGamble, The perception of sound direction as a conscious process, *Psychol. Rev.*, 9, 1902, 357-373, and Intensity as a criterion in estimating the distance of sounds, *Psychol. Rev.*, 16, 1909, 416-426; D. Starch, Perimetry of the localization of sound (i), *Psychol. Monog.*, 6, (No. 28), 1905, 1-45, and (ii), *Psychol. Monog.*, 9, (No. 38), 1908, 1-55; D. Starch & A. L. Crawford, Perception of the distance of sound, *Psychol. Rev.*, 16, 1909, 427-430; K. Dunlap, The localization of sounds, *Psychol. Monog.*, 10, (No. 40), 1909, 1-16.

[3]One of the earlier references to this theory is found in A. Steinhauser, The theory of binaural audition; a contribution to the theory of sound, *Phil. Mag.*, (5) 7, 1879, 181-197, 261-274. See also E. W. Scripture, On binaural space, *Stud. Yale Psychol. Lab.*, 5, 1897, 76-80.

[4]E. Mach, Bemerkungen über die Function der Ohrmuschel, *Arch. f. Ohrenhk.*, 9, 1875, 72-76.

[5]This has been demonstrated by Lord Rayleigh, Acoustical observations, *Phil. Mag.*, (5) 3, 1877, 456-464; S. P. Thompson, The pseudophone, *Phil. Mag.*, (5) 8, 1879, 385-390, and On the function of the two ears in the perception of space, *Phil. Mag.*, (5) 13, 1882, 406-416; Angell, *op. cit.*; A. M. Hocart & W. McDougall, Some data for a theory of the auditory perception of direction, *Brit. J. Psychol.*, 2, 1908, 386-405; C. S. Myers, The influence of timbre and loudness on the localization of sound, *Proc. Roy. Soc.*, B, 88, 1915, 267-284.

[6]See Rayleigh, *op. cit.*, and Acoustical observations (iv), *Phil. Mag.*, (5) 13, 1882, 340-347; Bell, *op. cit.*; J. R. Angell & W. Fite, The monaural localization of sound, *Psychol. Rev.*, 8, 1901, 225-246, and Further observations on the monaural localization of sound, *ibid.*, 449-458; Hocart and McDougall, *op. cit.*; and O. Klemm, Untersuchungen über die Lokalization von Schallreizen, *Psychol. Stud.*, 8, 1913, 487-505.

difference theories have, in the views of many investigators, not so much supplemented as actually challenged the theory set in terms of physical intensity.

Although there were several earlier studies on binaural beats and attendant changes in the apparent position of the sound,[7] the theory of sound-localization as based upon binaural phase-differences, which integrated these and other facts, is usually attributed to Lord Rayleigh, who pointed out that the head-shadow is insufficient, at least with frequencies less than 256 cycles, to make an appreciable binaural difference in intensity.[8] It was further urged by More and Fry[9] that such animals as mules and rabbits, whose ears are placed above their heads, have no head-shadow and are nevertheless able to localize. The resulting experimental and theoretical work on localization of tones differing binaurally in phase was carried out in large part by physicists, most of whom concluded that the organism directly perceives differences in phase.[10] It was early shown, however, at least by Wilson and Myers[11] and by Ferree and Collins,[12] that phase is not an experiential term, and that, as a matter of fact, we do not experience the differences as such. Several methods of dealing with phase-differences have appeared. One method consists in reaffirming faith in, and

[7]This aspect of Mach's work (*op. cit.*) seems also to have been overlooked. See also S. P. Thompson, On binaural audition, *Phil. Mag.*, (5) 4, 1877, 274-276, Phenomena of binaural audition (ii), *Phil. Mag.*, (5) 6, 1878, 383-391, and Phenomena of binaural audition (iii), *Phil. Mag.*, (5) 12, 1881, 351-355; K. L. Schaefer, Ueber die Wahrnehmung und Lokalisation von Schwebungen und Differenztönen, *Zsch. f. Psychol.*, 1, 1890, 81-98, and Zur interaurealen Lokalisation diotischer Wahrnehmungen, *ibid.*, 300-309; P. Rostosky, Ueber binaurale Schwebungen, *Phil. Stud.*, 19, 1902, 557-598. (More and Fry delayed publishing their work.) L. T. More and H. S. Fry, On the appreciation of differences of phase of sound waves, *Phil. Mag.*, (6) 13, 1907, 452-459.

[8]Lord Rayleigh points out (On the perception of sound direction, *Phil. Mag.*, (6) 13, 1907, 214-232) that the maximal binaural difference at 256 cycles would be about 10% of the total intensity; at 128 cycles it would be about 1% of the total intensity. On the other hand it appears (H. Fletcher, *Speech and Hearing*, Van Nostrand, 1929, part 3, p. 147 ff) that a perceptible increase in loudness does not always involve simply a proportional increase in the sound energy; that, for instance, at a loudness level of 10 decibels the energy must be increased about 73% to be noticeably louder.

[9]*Op. cit.*

[10]Lord Rayleigh, Acoustical notes (vii), *Phil. Mag.*, (6) 13, 1907, 316-333; L. T. More, On the localization of the direction of sounds, *Phil. Mag.*, (6) 18, 1909, 308-319; G. W. Stewart, Phase relations in the acoustic shadow of a rigid sphere; phase difference at the ears, *Phys. Rev.*, 4, 1914, 252-258, The function of intensity and phase in the binaural location of pure tones, *Phys. Rev.*, 15, 1920, 425-445, and The intensity logarithmic law and the difference of phase effect in binaural audition, *Psychol. Monog.*, 31, (No. 140), 1922, 31-44; R. V. L. Hartley, The function of phase difference in the binaural location of pure tones, *Phys. Rev.*, 13, 1919, 373-385; R. V. L. Hartley and T. C. Fry, The binaural location of pure tones, *Phys. Rev.*, 18, 1921, 431-442. Fletcher holds that "when the phase is different there will be a corresponding time interval between the occurrence of the two maxima produced in the brain. It is undoubtedly [!] the recognition of this time interval that enables us to recognize phase difference," *op. cit.*, p. 191.

[11]H. A. Wilson & C. S. Myers, The influence of binaural phase differences on the localization of sound, *Brit. J. Psychol.*, 2, 1908, 363-385; C. S. Myers & H. A. Wilson, On the perception of the direction of sound, *Proc. Roy. Soc.*, 80, 1908, 260-266.

[12]C. E. Ferree & R. Collins, An experimental demonstration of the binaural ratio as a factor in auditory localization, this JOURNAL, 22, 1911, 250-297.

adherence to, the 'intensity' theory by demonstrating localizations based upon intensive differences, and discounting the rôle of phase, either directly[13] or indirectly by ignoring it.[14] A second way consisted in attempting to reduce phasic to intensive differences. The famous theory of Wilson and Myers, for instance, attempted to translate phase back into intensive differences by way of bone-conduction between the ears.[15] There is now little, if any, evidence in support of this theory, and very considerable against it. Binaural beats are now usually considered dependent upon central factors, and not upon effects in the peripheral organ.[16] No one has so far authenticated binaural difference-tones.[17] In experiments with cases having monaural deafness no evidence of sound-transmission has been reported, except with great intensities.[18] The masking effects of pure tones has been measured when the two tones were presented to separate ears. At one representative pair of frequencies (1200 and 1300 cycles) the intensity of the 1200 tone at one ear must be increased 1,000,000 times (60 threshold units) before the threshold value of the tone in the other ear is affected. In contrast to this dichotic masking, the masking effect upon the higher tone begins practically with the introduction of the tone of 1200 cycles at the same ear.[19] Again, if the Wilson and Myers hypothesis is true, a

[13]Ferree & Collins, *op. cit.*

[14]G. F. Arps & O. Klemm, Untersuchungen über die Lokalisation von Schallreizen, *Psychol. Stud.*, 8, 1913, 226-270; C. A. Ruckmick, Experiments in sound localization, *Psychol. Monog.*, 30, (No. 136), 1921, 77-83.

[15]Wilson & Myers, *op. cit.* Several investigations had earlier suggested bone-conduction as a possible theory for the localization of tones differing in phase. See *e.g.* S. P. Thompson, On binaural audition, *Phil. Mag.*, (5) 4, 1877, 274-276, and Phenomena of binaural audition (ii), *Phil. Mag.*, (5) 6, 1878, 383-391; Rostosky, *op. cit.;* T. J. Bowlker, On the factors serving to determine the direction of sound, *Phil. Mag.*, (6) 15, 1908, 318-332. A careful history of the general problem may be found in H. Banister, The transmission of sound through the head, *Phil. Mag.*, (7) 2, 1926, 144-161, and Phase effect and the localization of sound; an examination of the Myers-Wilson hypothesis, *Phil. Mag.*, (7) 2, 1926, 402-431.

[16]J. Peterson, The nature and probable origin of binaural beats, *Psychol. Rev.*, 23, 1916, 333-351; G. W. Stewart, The theory of binaural beats, *Phys. Rev.*, 9, 1917, 514-528; E. M. von Hornbostel, Beobachtungen über ein- und zweiohriges Hören, *Psychol. Forsch.*, 4, 1923, 64-114.

[17]S. P. Thompson, On binaural audition, *Phil. Mag.*, (5) 4, 1877, 274-276; K. L. Schaefer, Ueber die Wahrnehmung und Lokalisation von Schwebungen und Differenztönen, *Zsch. f. Psychol.*, 1, 1890, 81-98; C. R. Cross & H. M. Goodwin, Some considerations regarding Helmholtz's theory of consonance, *Proc. Amer. Acad. Arts & Sci.*, 27, 1891, 1-12; Peterson, *op. cit.*, and H. Banister, The transmission of sound through the head, *Phil. Mag.*, (7) 2, 1926, 144-161.

[18]L. T. More, *op. cit.*, Peterson, *op. cit.*, Banister, *op. cit.* R. L. Wegel & C. E. Lane (The auditory masking of one pure tone by another and its probable relation to the dynamics of the inner ear, *Phys. Rev.*, 23, 1924, 266-285) got the tone across when the intensity was very great. They attribute this to bone-conduction.

[19]H. Fletcher reports this in practically identical language in Physical measurements of audition and their bearing on the theory of hearing, *J. Frank. Inst.*, 196, 1923, 289-326, and *Bell Sys. Tech. Jour.*, 2, 1923, 145-180, and *Speech and Hearing*, part 3. See also Wegel and Lane, *op. cit.* The effect is again attributed to bone-conduction. Banister (Phase effect and the localization of sound; an examination of the Myers-Wilson hypothesis, *Phil. Mag.*, (7) 2, 1926, 402-431) points out, however, that they have overlooked several other possible conduction sources, such as the sinuses.

perceptible intensive difference should be observable between phase-differences of 0 and $\lambda/2$; yet such differences are not obtained.[20] Finally, those studies where intensity alone has been varied at the two ears indicate that the intensive difference must be many times greater than naturally occurs, in order to obtain lateral localizations.[21] It was this type of evidence that drove certain physicists to the theory that we perceive phase directly.[22] We may conclude that localizations can be made upon the basis of binaural intensive differences alone and upon binaural phasic differences alone, but that the one is not reducible to the other.

The third way of dealing with phase comes from those recent experiments which show that dichotic clicks slightly differing in the time of arrival at the two ears are perceived as a single sound localized laterally in proportion to the time-difference.[23] This result is used to support the suggestion that phase-differences can be reduced to temporal differences,[24] as opposed to the view that the ratio of the phase-difference at the ears to the angular displacement of the fused sound is proportional to the frequency of the pure tone employed.[25] But this is a facile solution which merely pushes back the problem another step, since the temporal difference is not perceived but instead a single tone or

[20] H. Banister, A further note on the phase effect in the localization of sound, *Brit. J. Psychol.*, 15, 1924, 30-31, Three experiments on the localization of tones, *Brit. J. Psychol.*, 16, 1926, 266-292, and Phase effect and the localization of sound. See also G. W. Stewart, The function of intensity and phase in the binaural location of pure tones, *Phys. Rev.*, 15, 1920, 425-445, and The intensity logarithmic law and the difference of phase effect in binaural audition, *Psychol. Monog.*, 31, (No. 140), 1922, 31-44; Hartley & Fry, *op. cit.*

[21] G. W. Stewart & O. Hovda, The intensity factor in binaural localization; an extension of Weber's Law *Psychol. Rev.*, 25, 1918, 242-251; Stewart, *opp. citt.*; H. M. Halverson, Binaural localization of tones as dependent upon differences of phase and intensity, this JOURNAL, 33, 1922, 178-212; H. Banister, Three experiments on the localization of tones, *Brit. J. Psychol.*, 16, 1926, 266-292.

[22] See the references cited in footnote 10 above.

[23] O. Klemm, Untersuchungen über die Lokalisation von Schallreizen, *Arch. f.d. ges. Psychol.*, 38, 1918, 71-114, and Untersuchungen über die Lokalisation von Schallreizen, *Arch. f. d. ges. Psychol.*, 40, 1920, 117-146; E. M. von Hornbostel & M. Wertheimer, Ueber die Wahrnehmung der Schallrichtung, *Sitzber. Akad. preuss. Wiss.*, 1920, 388-396; J. Whittmann, Beiträge zur Analyse des Hörens bei dichotischer Reizaufnahme, *Arch.f.d. ges. Psychol.*, 51, 1925, 21-122; A. L. Bennett, A measurement of the efficiency of the ears as a means of detecting short time intervals, *J. Opt. Soc. Amer.*, 14, 1927, 342-345.

[24] In addition to those just cited, see also Stewart *op. cit.*; E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this JOURNAL, 37, 1926, 157-188; H. M. Halverson, The upper limit of auditory localization, *ibid.*, 38, 1927, 97-106; O. C. Trimble, The theory of sound localization: a restatement, *Psychol. Rev.*, 35, 1928, 515-523; L. T. Troland, The psychophysiology of auditory qualities and attributes, *J. Gen. Psychol.*, 2, 1929, 28-58. Boring (*op. cit.*) proposes further to reduce the temporal to an intensive difference. Trimble's more recent evidence stands against this possibility. O. C. Trimble, The relative rôles of the temporal and the intensive factors in sound localization, this JOURNAL, 41, 1929, 564-576.

[25] See, *e.g.*, M. Simpson, Experiments in binuaral phase difference effect with pure tones, *Phys. Rev.*, 15, 1920, 421-424; H. Banister, The effect of binaural phase differences on the localization of tones at various frequencies, *Brit. J. Psychol.*, 15, 1925, 280-307.

click as from a given direction. There finally remains the direct psychological attack upon the experiential changes which may accompany changes in phase, or changes in temporal relationships, in binaural hearing. Von Hornbostel[26] and Whittmann[27] have written extensive accounts of qualitative changes under these circumstances. Halverson[28] has attacked the problem quantitatively for the attribute of volume, while Banister[29] has worked with intensity. Since our own problem deals with these two and also with brightness and pitch, the results of these researches will be examined later.

(c) As the factors involved in auditory localization became better known, the evidence required some sort of systematization in terms of auditory theory. Much of the theorizing with localization has been done by recent advocates of non-resonance theories of hearing. The resonance theories were developed primarily to explain the facts of analysis.[30] Localization is for them entirely a secondary problem, the facts of which should be subsumed under the general theory with no more than a few minor supplements.[31] Theories starting from a different point of view may consider localization as of more importance.[32] A notable contribution in this direction is Boring's recent paper, which approaches theory from the facts of intensity, volume and localization rather than from pitch. Boring has shown that by envisaging audition from this aspect several equivocal interpretations of accepted experimental work could be offered, and a number of crucial experimental problems could be formulated. With respect to localization, Boring postulates (as did Watt) some sort of overlapping of the cortical areas of the two ears; and he then attempts to reduce the intensive, phasic, and temporal differences to a partial inhibition of the area of one ear by that of the previously or more intensively excited ear. Further details of these theories, which are related to the present problem, will be introduced and discussed after our facts have been presented.

The Attributive Character of Tones

A primary difficulty to be faced in any problem involving the experiential basis of sound is the lack of agreement among psychologists upon the auditory attributes. Within the last twenty-

[26] *Op. cit.*

[27] *Op. cit.*

[28] H. M. Halverson, Diotic tonal volumes as a function of difference of phase, this JOURNAL. 33, 1922, 526-534.

[29] H. Banister, Phase effect and the localization of sound.

[30] *E.g.*, Helmholtz, *Sensations of Tone*, 1885; H. Hartridge, A vindication of the resonance hypothesis of audition, *Brit. J. Psychol.*, 11, 1921, 277-288; 12, 1921, 142-146, 362-382; C. A. Cosens & H. Hartridge, A vindication of the resonance hypothesis of audition (iv), *Brit. J. Psychol.*, 13, 1922, 48-51; (v), *ibid.*, 185-194; G. Wilkinson & A. A. Gray, *The Mechanism of the Cochlea*, Macmillan, 1924; H. Fletcher, *Speech and Hearing;* L. T. Troland, *op. cit.*

[31] See H. Banister, A suggestion towards a new hypothesis regarding the localization of sound, *Brit. J. Psychol.*, 17, 1926, 142-153; Fletcher, *op. cit.*, Troland, *op. cit.*

[32] See H. J. Watt, *The Psychology of Sound*, Cambridge Univ. Press, 1917, and A theory of binaural hearing, *Brit. J. Psychol.*, 11, 1920, 163-171; Boring, *op. cit.*

five years intensity seems to be the only attribute which has not been questioned.

Pitch, when considered as 'quality', received a blow from Watt[33] who looked upon it as an 'ordinal' attribute. Rich attempted to eradicate differences of opinion by studying the attributes experimentally, testing them for consistency and independent variability on the basis of their several limens.[34] He regarded volume as an attribute, and thought that it followed Weber's Law. Halverson later worked with volume, getting similar results.[35] Rich's observers were unable to differentiate brightness and pitch, but Abraham, employing the siren disk, found that he could vary brightness independently of pitch, and identified brightness with vocality.[36] Rich concluded that vocality was not an attribute. The theory that pure vowel sounds lie an octave apart[37] was not confirmed either by Rich[38] or by Weiss.[39]

Summing up the experimental literature to 1924, Ogden lists the attributes of sound as pitch, volume, brightness and intensity.[40] We shall provisionally accept this classification of the tonal attributes. Our specific problem is to determine what phenomenally represents a change in phase. A description in psychological language would be a description in terms of tonal attributes. The fact of disagreement among experimental psychologists regarding the auditory attributes suggests inherent difficulties in observation. Experimental reports dealing with one of the attributes of tone[41] may tend to confuse that one with others, unless the observers painstakingly acquire fixed standards of discrimination. In reviewing the studies on the tonal attributes the question often arises whether diverse terms do not sometimes mean the

[33] H. J. Watt, *The Psychology of Sound*, Chap. 2.

[34] Especially G. J. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 121-164. See also Rich, A preliminary study of tonal volume, *J. Exper. Psychol.*, 1, 1916, 13-22.

[35] H. M. Halverson, Diotic tonal volumes as a function of difference of phase, this JOURNAL, 13, 1922, 526-534, and Tonal volume as a function of intensity, *ibid.*, 35, 1924, 360-367.

[36] O. Abraham, Töne und Vocale der Mundhöhle, *Zsch. f. Psychol.*, 74, 1915, 220-231, and Zur physiologischen Akustik von Wellenlänge und Schwingungszahl, *Zsch. f. Sinnesphysiol.*, 51, 1920, 121-152.

[37] W. Köhler, Akustische Untersuchungen (ii), *Zsch. f. Psychol.*, 58, 1911, 59-140.

[38] G. J. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 121-164.

[39] A. P. Weiss, The vowel character of fork tones, *ibid.*, 31, 1920, 166-193.

[40] R. M. Ogden, *Hearing*, Harcourt Brace, 1924. Ruckmick's recent classification ignores brightness (C. A. Ruckmick, A new classification of tonal attributes, *Psychol. Rev.*, 36, 1929, 172-180).

[41] *E.g.* Abraham's work with brightness, Halverson's with volume, Banister and other's with intensity. The physicists working with hearing usually handicap their careful technical control of the stimulating conditions by neglecting the training and instruction of their observers. They usually assume that pitch and intensity exhaust the character of pure tones.

same thing, and also whether the same term may not, at various times and for various observers, refer to different characteristics. It is therefore advisable, indeed necessary, to train observers accurately to discriminate the several aspects of tone, and carefully to describe as well as to label them.

PROCEDURE, APPARATUS AND PRELIMINARY TRAINING

Our general course of procedure was first to train our *Os* in attributive description, secondly to confirm this description by obtaining liminal values, and thirdly to present in pairs successive tones produced by unlike phase at the two ears, instructing the *Os* to compare the two with regard to one of the attributes, pitch (P), volume (V), brightness (B), or intensity (I). Several vibrational frequencies were used in order that we might distinguish absolute temporal distance from relative phase-difference as a condition of localization and of attributive change.

The sound-source was an electric oscillating circuit which actuated a pair of Baldwin receiving phones (Fig. 1).[42] The phones were placed in wooden boxes and heavily padded with harness-felt. A small hole bored in one end of the box permitted the entrance of 6-mm. glass tubes. These tubes, which were five feet in length, fitted in turn into 9-mm. tubes of the same length. A thick coating of vaseline about the inner tube made the fit practically air-tight. From the ends of the large tubes rubber and glass connections led to two pairs (for two observers) of binaural caps. The phase of the wave-train was changed by shifting one or the other box together with its connecting tube in either direction along a steel centimeter-tape. By inserting an additional length of tubing between one phone and its fixed tubing the extent of a possible difference in phase was increased from about 130 cm. to twice that amount.[43] The boxes and tubes for changing phase were put in this parallel situation to effect greater differences of phase, and to afford fewer turns in the connections which might resonate or interfere with the transmission of the tones. As a matter of fact, the tones at the end of the binaurals were simpler and purer than the tones heard directly from the ear phones.[44]

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[42] For the assembling and standardization of the apparatus the writers are heavily indebted to the technical knowledge, skill and good-will of Professor Elmer Culler.

[43] For a particular series the phones could be equated in intensity by changing their respective resistances. A test-series, however, with two observers instructed to report on intensity, in which both phones were moved in phase nearer to, and away from, the observer, showed that the differences in distance did not appreciably affect the intensity of the sound.

[44] Although oscillograms of the electric wave produced by the oscillator showed approximate sine curves, the vibrations at the telephone (especially with frequencies below 60 cycles) were not perfectly pendular. The Baldwin phone gives a much better tone, however, than the stock phone sold by the Western Electric Company. It was the best that we could command.

This method of changing phase was selected, despite Banister's criticisms,[45] because it allows a precise measurement of the extent of change in phase. Banister's results show that the angle of displacement for various observers was constant irrespective of pitch.[46] These results, which he himself discounts,[47]

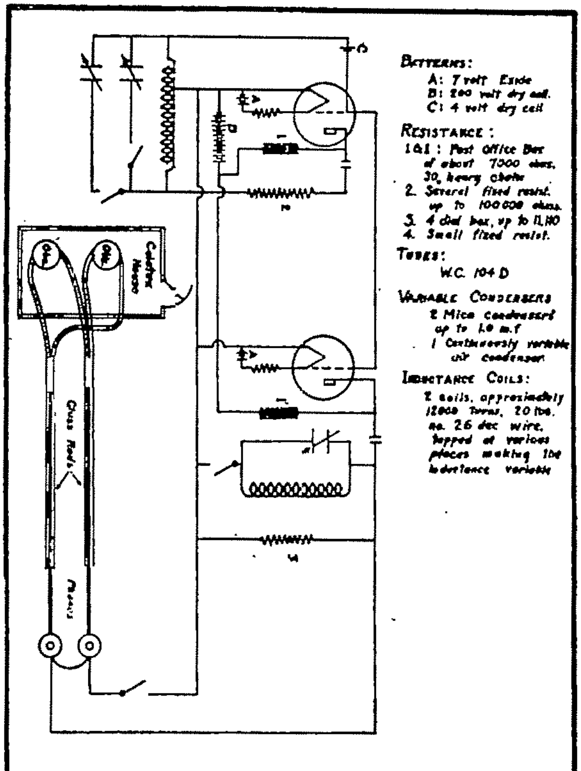

Fig. 1
Oscillator Circuit and Observation Booth

were doubtless due to the fact that his observers compared only 180° differences with the standard of 0°. The dual tones which Whittmann observed, like the buzz in Wilson and Myers' report, may be due to the resonance or filter effects of his U-shaped pipes.[48]

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[45] H. Banister, The effect of binaural phase differences on the localization of tones at various frequencies, *Brit. J. Psychol.*, 15, 1925, 280-307.
[46] *Op. cit.*
[47] H. Banister, A suggestion towards a new hypothesis regarding the localization of sound, *Brit. J. Psychol.*, 17, 1926, 142-153.
[48] Whittmann, *op. cit.*

The observers sat in a 'Celotex' house, 6 ft x 6 ft x 3 ft, which allowed the simultaneous observation of two subjects and served to cut off visual distractions. It also permitted the experimenter to work in the same room. The reports were made on mimeographed blank-sheets. The phone-circuit was cut with a knife switch, and the duration of the stimulus was controlled by a silent pendulum. The compared tones were about 1½ sec. long and separated by a 1-sec. interval.

The method of constant stimuli was used with five (or sometimes seven) comparison-positions. Half the judgments in each series were based on pairs in which the standard was first, and in half the comparison stimulus was first. The phones were alternated from left to right side in order to factor out whatever differences in quality they might condition. The rubber and glass tubing was so arranged that a given phase was presented to opposed ears of the two observers, who sat alternately on left and right within the observation-house. No observations were made on this apparatus with changes in phase, however, until after extensive preliminary training had been carried out under the various instructions for P, V, B and I.

Most of the training consisted in making reports on pairs of tones produced by Stern variators. The University air-supply, with a pressure of 5 atmospheres, was controlled by a system of needle-valves. Minor unevennesses in the supply were smoothed by inserting a metal air-reservoir in the line. A water-manometer graduated in centimeters was kept constantly in the circuit of the variator.

These preliminary observations were made by several observers at once and in the room with the variators. The judgments took the form of written reports on mimeographed blanks. The tone from variator No. 1 was interrupted by releasing an escape-valve (thumb over open end of Y tube in circuit), which dropped the pressure below that necessary to produce a tone; the other variators were interrupted by turning the air-jet suddenly away from the mouth of the bottle. Frequencies of 150 (variator No. 1, pressure 7.5 cm.), 550 (variator No. 4, pressure 2 cm.), and 900 (variator No. 6, pressure 3.5 cm.) were used as standards. The comparison-intervals were first made very large and then cut down step by step to suit the discrimination of the observers. On the variators there were in all for each observer about 600 reports on P, 600 on B, and about 850 on V and 850 on I. A device for changing B without changing P was made[49] by cutting 8 concentric rows of 20 circular holes of like diameter in a cardboard disk. A blast of air on the various rows of the rotating disk gave tones of like P but different B. A demonstration from this apparatus preceded every hour's work on B throughout the course of the experiment. For demonstrating to the Os intensive differences, the energy was changed by a slight variation in the air-pressure upon the variator. A few verbal reports were taken on pairs of such variator intensities (at various frequencies) prior to each set of observations upon intensity.

The Liminal Determinations

The limens for the various attributes were not determined immediately upon the conclusion of the preliminary series but

[49] O. Abraham, Zur physiologischen Akustik von Wellenlänge und Schwingungszahl, *Zsch. f. Sinnesphysiol.*, 51, 1920, 121-152.

just before the reports were taken upon a given attribute with variation of phase (see Table I). The limens were determined at the frequencies of 150 and 500 by the constant method, using 5 stimuli from the variators. Again an hour or two of practice was required before the limens were determined.

TABLE I

LIMENS (IN VIBRATIONS) FOR P, V, AND B, RECKONED FROM XI (SUBJECTIVE EQUALITY)

		Pitch Limens				Volume Limens				Brightness Limens			
		150 dv		550 dv		150 dv		550 dv		150 dv		550 dv	
Obs.		High	Low	High	Low	Less	More	Less	More	Brt.	Dull	Brt.	Dull
B	DL	.31	.24	.89	.89	2.19	.80	1.43	.62	1.18	1.64	3.32	3.73
	h	.55	.72	.56	.56	.15	.37	.12	.27	.43	.31	.09	.08
C	DL	.36	.30	.47	.53	2.06	1.45	5.04	5.10	1.32	1.53	3.91	3.09
	h	.74	.88	.46	.40	.27	.38	.15	.15	.87	.75	.11	.24
F	DL	.40	.26	.29	.29	.62	.34	.63	.13	.28	.37	1.10	1.35
	h	.79	1.22	.30	.30	.83	1.58	.32	1.64	.97	.73	.36	.29
G	DL	.37	.48	.36	.54	1.51	.27	.86	.78	.96	.62	1.44	1.17
	h	1.27	1.06	.65	.43	.71	3.95	.25	.27	.39	.61	.16	.19
H	DL	.26	.20	.54	.54	1.11	2.01	2.62	2.86	.54	.42	1.68	1.20
	h	.90	1.20	.25	.25	.56	.31	.08	.08	.25	.33	.11	.16
I	DL	1.17	.32	.96	1.16	.74	1.11	3.02	3.45	.90	.88	2.26	3.94
	h	.11	.40	.30	.25	.35	.25	.08	.07	.44	.45	.26	.15
Mc	DL	1.23	.65	1.49	1.98	3.28	2.18	9.34	3.84	1.44	1.74	3.56	3.56
	h	.59	1.12	.73	.53	.12	.18	.10	.24	.70	.58	.22	.22
M	DL	.07	.06	.29	.59	.13	.11	.33	.57	.78	.64	2.20	1.14
	h	3.06	3.81	1.21	.60	1.00	1.22	.92	.53	.59	.71	.13	.25
S	DL	.48	.52	1.60	1.22	1.22	.96	2.44	3.41	1.08	1.52	2.64	3.08
	h	1.01	.94	.36	.47	.42	.47	.18	.13	.44	.31	.27	.23
Z	DL	.40	.41	.53	.41	.62	.83	1.47	.89	no limen computable			
	h	1.21	1.20	.48	.63	.72	.85	1.05	1.64				
Av. Limens		0.505	0.344	0.742	0.815	1.348	1.006	2.718	2.165	0.94	1.04	2.46	2.47

The instructions during the entire experiment followed this general form (given in an example for V).

"After a 'ready' signal there will be two tones. Compare these with respect solely to VOLUME. If they are the same in VOLUME record "S." If they are different ("D") indicate, if you can, whether the second tone is greater or less than the first. A statement of the criteria will be requested at the end of the series."[50]

The intervals used for the comparison-stimuli at frequencies of 150 and 550 were; for Pitch 0.5 dv (at 150) and 1.0 dv (at 550); for Volume, 2.0 dv and 3.0 dv (except for the group G, F, M, and Z, for whom the intervals were gradually

[50]The observers, to whom the writers are indebted for many long hours of service, are as follows. (1) Those with musical training and with experience in related auditory researches: Dr. Sullivan (S) (Observer for Bishop); Dr. Guilford (G) (Observer for Beck); Dr. Möller (M) (Observer for Bishop, Halverson and Möller); and Mr. Macdonald (Mc) Observer for Pratt, Möller, Halverson and Macdonald). (2) Observers with musical training only; Mr. Zuschke (Z) and Mr. Fox (F). (3) Observers with limited musical training: Dr. Culler (C), Dr. Higginson (H), Mr. Bidwell (B) and Miss Ireland (I). The last two were graduate students. The others were members of the departmental staff.

reduced on 3 successive days to 0.5 and 1.0); for Brightness, 1 dv and 3 dv; and for Intensity, 2.0 dv and 3.0 dv. The limens are based on 20 reports for each comparison. The time-orders were half sv and half vs.

The limens and measures of precision were computed by the use of Urban's tables. The figures (Table I) give the limens (DL) from the point of subjective equality, Xi,[51] together with the measures of precision (h) for P, V and B.

It may be observed that the limens for volume are lower as a group than those obtained by Rich[52] or Halverson;[53] although the determinations for one of Rich's observers, Professor Ogden, fall well within the range of our figures. Furthermore, there is little difference between the average limens for V and B with these ten observers.

Although the P, V, and B series served as training, the I series had a theoretical bearing. According to Boring's theorizing, V does not necessarily change with P, nor do V and I change simply with the energy of the stimulus; both vary with the amplitude of the vibrations.[54] He thus suggests that the V-limens obtained by Rich are artifacts of the experiment, due, not to the ostensible change in frequency, but to fortuitous changes in amplitude which would occur if the energy remained constant. Boring points to Halverson's work on V and I[55] as showing the more fundamental relationship. Thus, if we work out Boring's hypothesis,[56] we find it possible to determine a limen for I of about 20 dv at 400 cycles, and, since the V-limen is about twice as great as the intensive limen, it should be considerably higher at this

[51]E. Culler, Studies in psychometric theory, *Psychol. Monog.*, 35. (No. 163), 1926, 71-75.

[52]Rich, *opp. citt.*

[53]Halverson, *opp. citt.*

[54]Boring, *op. cit.*

[55]H. M. Halverson, Tonal volume as a function of intensity, this JOURNAL, 35, 1924, 360-367.

[56]We are told that, pitch constant, the limen for V is considerably more than that for I (Halverson, *op. cit.*); and that, energy constant, the limen for V at a frequency about 500 dv is approximately 12 dv (Rich). It follows that when energy is constant an I-limen should likewise be found, less than the V-limen. The extent of such a limen may be computed from the results of Knudsen, who has determined over a wide range of frequencies the proportional increment in pressure which is perceived as being just different (V. O. Knudsen, Sensibility of the ear to small differences in intensity and frequency, *Phys. Rev.*, 21, 1923, 84-102). At 400 dv the increment is 0.115. Now, since energy is proportional to the product of the squares of the amplitude and frequency, the increase in the amplitude would be .056. With energy remaining constant, a change in the amplitude by the amount 0.056 would increase the frequency about 22 dv (it would decrease it about 17 dv).

frequency. As can be observed from Table II, no such I-limen could be found for the 9 *O*s in this experiment; and no V-limen has been found much larger than 15 dv at this frequency.

TABLE II

REPORTS ON INTENSITY WITH CHANGE OF PITCH (IN PERCENTAGES). METHOD OF CONSTANT STIMULI

	Obs. in Group 1						Obs. in Group 2			
	B		Mc		C		H		S	
Freq.	+	−	+	−	+	−	+	−	+	−
154	70	20	5	80	100	0	80	0	55	5
152	45	30	0	25	80	0	35	0	0	55
Stand	10	30	10	0	15	10	10	10	30	10
148	30	50	30	5	0	95	0	60	25	35
146	85	15	45	0	0	95	0	75	50	15
556	20	75	70	20	90	0	55	20	70	0
553	30	60	35	5	75	15	30	35	15	10
Stand	25	5	5	0	10	0	0	10	5	15
547	50	40	20	30	20	70	15	60	15	25
544	15	80	10	65	0	90	5	90	15	15

	Obs. in Group 3							
	F		G		M		Z	
Freq.	+	−	+	−	+	−	+	−
154	90	0	65	0	35	40	5	90
152	70	10	65	20	30	40	0	70
Stand	20	30	40	35	35	15	30	5
148	0	100	30	70	10	40	15	20
146	50	25	100	0	10	85	0	45
556	20	55	20	25	5	70	30	10
553	65	15	10	60	35	50	10	0
Stand	0	45	30	45	20	25	5	0
547	100	0	80	0	45	35	90	10
544	90	0	15	35	60	10	15	0

THE RESULTS WITH PHASE DIFFERENCE

The frequencies used in all the determinations with phase-difference were 100, 400 and 1000 cycles, produced by the oscillator. Most of the results from the four attributive instructions (P, V, B and I) are based upon the same settings of comparison and standard. The speed of sound for the normal room-temperature was estimated to be around 34482 cm. per sec. in the open air and somewhat less in the tubes. Half the wave length ($\lambda/2$) for a frequency of 1000 cycles would then be slightly greater than 17 cm.

Three positions of the phones were used as comparison-values for 1000 cycles, the distances between them being 0, 8.5 and 17 cm. The standard, C, occurred first in half the determinations.

and second in half. At a frequency of 400 cycles, $\lambda/2$ would exceed 40 cm, a distance which was divided into five positions, the comparison distances between the phones being 0, 10, 20, 30 and 40 cm., respectively. Two different positions were used as standards, so that the area would be explored twice, the 0 and 20 cm. settings being thus used. The sequence of standard and variable was again, as in the other series, alternated at random so that the observer would never be reporting only upon differences of phase in the same direction. For the frequency of 100, $\lambda/2$ would approximate 172 cm. It would take sound approximately .005 sec. to travel that distance, a duration well over the upper critical time for localization based on time-differences.[57] For the purpose of a direct comparison, the first 40 cm. of this distance was divided, as was the 40 cm. of the 400-cycle frequency, into 10-cm. steps with standards at the 0 and 20 cm. settings. The next 120-cm. difference was divided into 15 cm. steps, the mid-points of each successive five (70 cm. and 130 cm.) being chosen as standards. Thus only a 160-cm. phase-difference ($\lambda/2=172$) was used.

After a long series of preliminary trials upon *volume*, a main series of observations was obtained at 1000 dv. with three constant stimuli, and at 400 dv. and 100 dv. with five constant stimuli. The results of several thousand observations (Table III) gave considerable evidence of change of volume with variation of phase. But the character of the volumic change ('greater' or 'less') was not consistent with the direction (increase or decrease). A possible total of 10 or 11 different limens can be computed for each of the 6 individuals who made the final observations There are actually only 38 cases where the percentage of judgments of greater or less equal or exceed 50% of the judgments at that comparison value. These are distributed as follows among the several observers.

	Less volume	More volume	Neither
B	4	2	4
C	8	1	2
F	2	5	4
G	1	7	3
M	5	3	3
Mc	0	0	10
Totals	20	18	26

[57] O. Klemm, Untersuchungen über die Lokalisation von Schallreizen, *Arch. f.d. ges. Psychol.*, 40, 1920, 117-146; E. M. von Hornbostel & M. Wertheimer, Ueber die Wahrnehmung der Schallrichtung, *Sitzber. Akad. preuss. Wiss.*, 1920, 388-396.

The true phenomenal basis for this result is not clear. We can only append the following comments. *Os* complained of the uncertainty of 'volume' in this curious and baffling situation

TABLE III

REPORTS ON VOLUME WITH CHANGE OF PHASE
(S = Standard stimulus)

	Phase Diffce	Obs C Great	Obs C Less	Obs G Great	Obs G Less	Obs Mc Great	Obs Mc Less	Obs M Great	Obs M Less
	Tone of 400 dv								
S	0	0	0	5	20	0	5	3	15
	10	30	10	55	5	5	15	33	45
	20	35	30	80	0	25	10	58	38
	30	20	50	70	15	15	15	40	53
	40	35	30	45	30	5	30	33	63
	0	55	5	20	50	25	20	50	45
	10	30	0	20	35	10	10	50	40
S	20	5	0	20	25	0	0	10	20
	30	0	65	30	25	10	10	13	55
	40	15	50	45	30	10	10	20	55
	Tone of 100 dv								
S	0	10	15	10	5	5	0	10	10
	10	20	50	55	0	5	0	30	25
	20	25	40	85	5	15	10	60	15
	30	30	45	90	0	20	30	65	25
	40	35	40	90	0	30	20	70	25
	0	40	25	60	40	40	15	35	50
	10	45	15	25	35	15	0	40	30
S	20	15	10	35	35	5	0	10	15
	30	10	35	40	40	5	5	25	30
	40	25	30	40	30	35	15	50	25
	40	50	20	50	40	35	10	75	10
	55	25	35	15	55	0	5	20	20
S	70	5	10	10	25	5	0	5	10
	85	35	30	65	25	5	10	30	65
	100	55	15	55	10	20	35	25	45
	100	80	5	25	50	5	10	35	45
	115	50	30	20	50	0	5	40	25
S	130	15	15	20	10	10	0	20	15
	145	0	55	35	35	20	0	25	40
	160	0	95	60	15	30	0	30	35

where the tone was constantly moving from ear to ear in its apparent course from side to side and back toward the median plane of the head. The criteria for 'volume' seemed shifting and uncertain. The general characterization of the attribute was an aspect of 'boundary' or 'extent' of the tone; but B and M thought volume not 'simple,' nearly all of the *Os* declaring at the end of the ex-

periments that volume and brightness were intimately related (6 of them said 'inversely related'), and several commented on volume's subtle dependence upon both pitch and intensity, attributes which were subjected, in our experiments, to wide variability in binaural tuning and acuteness. All *Os* found it extremely difficult to abstract from other changes induced by the experimental setting.

TABLE IV

REPORTS ON PITCH WITH CHANGE OF PHASE

(S = Standard stimulus)

	Phase Diffce	Obs. F Great	Obs. F Less	Obs. C Great	Obs. C Less	Obs. G Great	Obs. G Less	Obs. M Great	Obs. M Less
				Tone of 400 dv					
S	0	10	40	20	35	0	5	5	5
	10	30	50	25	40	85	0	35	50
	20	5	80	45	20	95	0	35	55
	30	10	70	15	35	85	0	75	25
	40	50	30	40	25	90	0	50	40
	0	60	25	70	10	60	10	65	25
	10	45	40	55	20	55	15	45	45
S	20	15	40	10	15	5	10	20	10
	30	60	20	45	35	10	25	20	70
	40	65	15	55	25	65	10	25	65
				Tone of 100 dv					
S	0	35	30	5	10	0	0	5	20
	10	35	35	15	25	60	0	30	50
	20	45	35	20	65	80	0	30	55
	30	45	50	5	70	90	0	35	55
	40	60	35	0	80	95	0	45	40
	0	25	65	50	10	75	5	45	55
	10	20	55	65	10	75	10	40	50
S	20	45	20	10	45	5	15	25	5
	30	70	20	15	60	15	35	20	35
	40	55	30	15	65	5	85	50	30

The reports upon *pitch* take a course similar to that for volume. The 'different' reports under phase-difference run even higher than in the case of volume. But again the direction and the amount of pitch-change do not run with the direction and the amount of phase-difference, and, as before, any standard when compared with itself gives most 'equal' reports of all. The reports for F, C, G and M at four of the seven settings are given in Table IV.

A complicating factor here is the 'pitch difference' of the ears. G, for example, reported 'lower' when the localization was on the right, and 'higher' on the left. When allowance is made for this difference of tuning, our results give no clear evidence of a direc-

tional change in pitch with a phasic modification of the wave-train. The difficulties of isolating a pitch-change are illustrated by the following remarks from the commentaries of this set.

C. "The two tones usually differ in some respect (the second being localized to one side); but I am not sure wherein the difference consists. It seems P sometimes, but usually not. Sometimes a change in B apparently."
S. "Localization distracts; judgments often based on P-B."
H. (preliminary) "I begin to doubt P-differences, and think perhaps it is V."
M. "I am not at all sure of my P-judgments."
G. "I don't believe there are many P-differences. Perhaps they are all equal!"

We had expected the reports upon *intensity* to be simple and straightforward; but they were the most confused and inconsistent of all. Here it was obvious to the Os that whatever change the sounds did undergo with phasic modification made a comparison of intensities extremely difficult. Here are relevant comments:

C. "There is always a tendency to establish a correlation between localization and the attributes in question, and to judge by inference when the criteria fail." "I is the product of density and volume, and the total weight of the tone." "This set seems to be entirely guesswork, there are so many differences in P, V and B involved."
M. "I think I am really judging a combination of other things. The B-difference has a lot to do with it."
Z. "Changes in B seem to veil the intensity aspect." "The second tone always seems larger, and the longer it lasts the more intense it seems." "Judgments are best when the series is a, b, a." "When I observe intensity, I can get brightness nicely!"

The pairs of unlike phase returned from 15% to 45% of 'greater' or 'less' reports: but the standard when compared with itself also gave a surprising number (up to 27%) of the same kind. While there was a distinct tendency to increase the 'different' reports with increase of interval reckoned from the standard, that increase was as apparent when the interval was directed toward like-phase (*e.g.* the phase-series 0, 10, 20, 30 and 40 cm. with the standard at 20 cm.) as when the series of comparisons moved out from like-phase toward a great difference (*e.g.* the same series, 0, 10, 20, 30 and 40 cm, with the standard at 0). This fact is apparent in the averages for all observers in these series at 400 dv.

Average of Reports on Intensity (400 dv)

Phase Diffce		Greater	Less	Total		Greater	Less	Total
0	Stand = 0	14	14	28		25	48	73
10		30	23	53		18	35	53
20		43	25	68	Stand = 20	16	27	43
30		31	40	71		23	41	64
40		47	28	75		42	29	71

With all these attributive comparisons, then, for volume, pitch, and intensity, we find virtually the same thing, *i.e.* a general increase of 'different' reports in comparisons moving away in both directions from the standard, but with no consistent difference in the apprehended direction of 'greater' and 'less,' 'higher' and 'lower,' to accord with the modification of phase.

The series for *brightness* wears a different complexion. Individual results for 5 *Os* and average calculated from all *Os* are given below (in percentages). Here appear (1) a small number

TABLE V

REPORTS ON BRIGHTNESS WITH A CHANGE IN PHASE
(S = Standard stimulus)

	Phase Diffce	Obs. B		Obs. C		Obs. F		Obs. G		Obs. M		Av. 10 *Os*	
		Gr.	Less	Gr.	Less	Gr.	Less	Gr.	Less	Gr.	Less	Gr.	Less
	Tone of 400 dv												
S	0	10	10	5	0	15	10	20	35	0	0	7	9
	10	25	55	10	55	10	65	10	65	15	70	14	44
	20	45	45	5	80	15	85	0	85	15	85	19	56
	30	20	80	0	75	20	65	5	90	25	80	14	70
	40	35	60	0	100	5	90	20	75	10	90	18	68
	0	50	30	73	15	70	15	70	15	65	25	60	17
	10	50	20	53	13	20	45	50	30	60	15	44	22
S	20	10	20	13	23	20	35	40	10	25	30	17	18
	30	30	30	5	33	40	50	35	50	20	55	23	40
	40	10	80	23	55	25	70	25	70	25	70	20	58
	Tone of 100 dv												
S	0	20	30	0	10	0	5	5	25	0	0	5	10
	10	0	50	5	35	0	70	0	95	0	80	8	46
	20	0	85	5	50	10	80	0	100	5	90	10	64
	30	0	100	10	65	0	90	0	85	0	100	5	71
	40	5	95	0	75	20	80	0	90	0	95	7	74
	0	55	30	85	0	70	10	53	25	50	25	55	23
	10	50	25	55	15	40	30	43	18	35	25	34	22
S	20	30	25	15	10	0	35	15	20	15	10	15	17
	30	30	35	20	30	30	15	18	43	15	55	24	31
	40	20	65	25	50	25	50	18	48	15	80	21	48
	40	85	0	50	20	70	5	55	5	55	35	54	16
	55	45	25	40	25	40	20	35	15	50	25	34	20
S	70	40	15	30	40	5	30	25	5	20	5	20	15
	85	30	45	10	60	5	30	30	40	25	50	19	33
	100	10	75	5	75	10	45	5	60	20	60	14	50
	100	30	50	55	30	45	5	43	28	50	15	47	16
	115	10	50	45	35	20	40	28	38	15	40	23	35
S	130	30	30	35	20	10	30	30	23	25	20	19	17
	145	30	40	35	45	35	35	48	13	35	30	32	25
	160	35	35	75	15	60	15	68	15	65	10	51	20
	Tone of 1000 dv												
S	0	35	10	0	40	0	10	25	10	10	10	12	12
	8	40	35	3	63	48	45	20	28	33	40	26	39
	16	33	60	5	68	50	50	10	63	28	60	23	56

of 'different' reports upon the standard pairs, equally divided between + and −, (2) an increasing number of 'different' reports as the phase-difference moves in opposed directions from the standard pair, (3) a decided increase in 'brighter' reports as like phase is approached and of 'duller' (less bright) reports as phase difference is increased. The commentaries showed that the Os H, I, Mc, S and Z were uncertain (through a part of the experiments) of their criteria for brightness; though all came in time to recognize it from instruction with the siren-disk. While their results bear out the above rules, the figures are clouded by a shifting of the basis of report. But the other 5 Os (B, C, F, G, and M), all of whom carried fixed criteria for brightness, exemplify the three rules throughout their reports, except for a reversal at the greatest phase differences (130 to 160 cm) of the tone 100 dv. The region around $\lambda/2$ needs further exploration.

Resuming the observations upon attributive character, then, it appears that, under our conditions and in the tonal regions which we examined, brightness alone (of the four attributes) undergoes a consistent change with change of phase and that the rule is for tones to grow duller and duller in dichotic hearing as their right and left components depart farther and farther from like phase.

After the above experimental program (which included about 7000 reports from each O) had been carried out, each O had placed before him for answer the following questions. Before his answers were required, he was presented over and over with the tones 400 dv and 100 dv, given dichotically in successive pairs as in the regular experiments. The pairs contained one member at like phase and the other at a large phase-difference (40 cm. or more upon one tube).

The questions were as follows:

1. Describe P, V ,B, and I in terms of auditory experience.
2. What, descriptively, are their inter-relations and dependences?
3. Do you think that these attributes fairly exhaust the description of a simple tone?
4. Do you think that they constitute distinct aspects or ways of regarding the tone?
5. How certainly can you distinguish changes in them with a change in phase?
6. Which of them change most prominently with a change in phase?
7. What is the direction of this change?
8. In which series were the differences in localization most disturbing?
9. Describe the experiential differences in localisation with a change in phase.

The first question was designed to orient the *O*. The first four strike directly at the attributes. Questions 5, 6 and 7 refer to changes accompanying phase-difference. Question 8 was put in to determine whether any of the observers might inadvertently be reporting localization rather than attributive difference; and the last question directly bears upon the main problem of the entire experiment.

O was instructed to search in turn for one attribute after the other at each new setting of the oscillator and to ask for a repetition of the pairs until he was thoroughly satisfied as to his answer for each attribute. The answers, which were written out, are given below in abstract form.

1. Description of tonal attributes
   A. Pitch
      i. the aspect which varies up and down (B, F, I, Mc, M, S, Z)
      ii. the distinguishing qualitative attribute (G)
         pitch comes as size (H)
         high tones are smoother, clearer, thinner, denser, compacter (C)
   B. Volume
      i. an aspect of the boundaries or extent (C, F, G, H, I, Mc, M, S, Z)
         seems a composite; dependent upon brightness (B)
      ii. It is also a matter of filling (M, S)
   C. Brightness
      i. an aspect of the filling, or clearness of emergence (B, C, F, G, H, I, Mc)
      ii. the glint or glitter which attaches to V of tones (M, S)
         antithesis between veiled and non-veiled, tubercular and healthy, etc. (Z)
   D. Intensity
      i. the aspect of force or strength (all but G)
      ii. the total mass or impressiveness of the tone (G)
2. Interrelations and dependences of the attributes
   i. B and V are inversely related (B, C, F, G, I, M)
      (P and I may be involved)
   ii. P changes at least B and V (all Os)
   iii. I is greater with high tones (C, H)
   iv. I varies with V (G, I, M, B? F?)
3. They exhaust the description of a simple tone
   i. Yes (F, Mc, S) Yet they are not simple (M)
   ii. They overlap, V being complex (B, I)
   iii. They leave out that total impression, the *simple* tone (H, M, Z)
   iv. Should add "density" (C, G)
4. Are they distinct aspects or ways of regarding tones?
   i. Yes; but cannot be separated (B, F, G, H, Mc, M, Z)
   ii. Brightness is not (S)
   iii. Not sure about V and B (I)
   iv. P and I seem fundamental, B dependent upon the purity of the tone, while V depends on some optical feature (C)
5. How certain are judgments with phase changes?
   i. Not certain (G, H, I, Mc, M)
   ii. Certain in the order B V P I (B)
      I B P V (F)
      I P V B (S)
      P B I V (Z)

6 & 7. How do the attributes change with increase of phase-difference?
- i. B decreases (all but S)
- ii. V increases (B, G, Mc, F?), decreases (H), direction doubtful (C, Z)
- iii. P increases (F), decreases (C, H) Low on right; high on left (G)
- iv. I increases (F?), decreases (C, H, Mc) More on right, less on left (G, M)

8. In which series did localization hinder?
- i. In P (H, I, S)
- ii. In V (C, F, G, I)
- iii. In B (C, F, I, Mc)
- iv. In I (Mc)
- v. Assisted in V and B (B)

9. What experiential differences in localization accompany a change in phase?
- i. hear "tones in one or the other ear" (B, F, H, Mc, M, Z)
- ii. some tones inside head (B, C, F, G, H, M, Z)
- iii. shifts from one side to the other (I, M, S)

These answers we take into account in our following discussion and interpretation of the experimental results.

Discussion

The Tonal Attributes. P and I remain unquestioned as attributes. The reports on B, both in the training and liminal series and with changes in phase, indicate that, as Köhler,[58] Abraham,[59] Weiss,[60] Ogden,[61] and others maintain, B is an attribute. We fail to get quantitative results confirming the previous work on V.[62] Perhaps it would be well if we could throw V out, as Banister has desired.[63] He contends that it is sounding objects which we perceive, and not sounds; that the conception of V has arisen (a) from our tendency to objectify our experiences, and (b) from a confusion of multiple locations of the sounding source. This assumption seems scarcely to accord with the phenomenal facts. Moreover, V has been identified as an attribute in at least three situations not involving localization: as a co-variant of P and of I[64] and (in the present experiment) of B. The relation of V

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[58] W. Köhler, *op. cit.*

[59] O. Abraham, Töne und Vocale der Mundhöhle, *Zsch. f. Psychol.*, 74, 1915, 220-231; and Zur physiologischen Akustik von Wellenlänge und Schwingungszahl, *Zsch. f. Sinnesphysiol.*, 51, 1920, 121-152.

[60] A. P. Weiss, The vowel character of fork tones, this JOURNAL, 31, 1920, 166-193.

[61] R. M. Ogden, *Hearing*

[62] G. J. Rich, *opp. citt.*; H. M. Halverson, Diotic tonal volumes as a function of difference of phase, this JOURNAL, 33, 1922, 526-534, and Tonal volume as a function of intensity, *ibid.*, 35, 1924, 360-367.

[63] H. Banister, Auditory theory; a criticism of Professor Boring's hypothesis, this JOURNAL, 38, 1927, 436-440.

[64] See Rich, *opp. citt.* and Halverson, *opp. citt.*

to B has appeared in our experiments to be especially intimate. V has been reported both as identical with B and as varying inversely to it.[65]

The Attributes and Phase. As we have said, B is the only property of the tone to vary consistently under change of phase. No one has (so far as we know) found a directional change of P with modification of phase and we have certainly found no positive evidence for such a correlation. Fixed differences in binaural tuning should, however, be taken more strictly into account before a final decision is reached. Intensive differences with changes in phase have been several times reported. Thompson[66] reported intensity as increasing with lateral shifts and phasic displacements. According to Bowlker and to the Wilson-Myers hypothesis[67] the intensity of the lateral tones should be greater than median tones. Rostosky[68] and Stewart[69] report the median tone as most intense, but discovered a pair of secondary maxima just short of $\lambda/2$. Von Hornbostel maintains that lateral tones, though fuller, are less intense than medial.[70] Halverson concluded that there were intensive differences, but was uncertain as to their direction.[71] More and Fry, however, with 14 observers looking especially for intensive differences, concluded that there were none.[72] Most systematic and careful was Banister's recent study.[73] He presented at random to his Os both intensive and phasic differences (comparing binaurally equal phase with $\lambda/2$). At first the Os reported the phase change as louder. With practice, however, they decided that the change was not one of intensity, but one of quality (brightness?). Our own negative

[65]See also R. H. Gundlach, Tonal attributes and frequency theories of hearing, *J. Exper. Psychol.*, 12, 1929, 187-196.

[66]S. P. Thompson, On binaural audition, *Phil. Mag.*, (5) 4, 1877, 274-276, and Phenomena of binaural audition (ii), *Phil. Mag.*, (5) 6, 1878, 383-391.

[67]T. J. Bowlker, *op. cit.;* Wilson & Myers, *opp. citt.*

[68]P. Rostosky, Ueber binaurale Schwebungen, *Phil. Stud.*, 19, 1902, 557-598.

[69]G. W. Stewart, Binaural beats, *Phys. Rev.*, 9, 1917, 502-508, The secondary intensity maxima in binaural beats, *Phys. Rev.*, 9, 1917, 509-513, The theory of binaural beats, *Phys. Rev.*, 9, 1917, 514-528, and Binaural beats, *Psychol. Monog.*, 25, (No. 108), 1918, 31-46.

[70]E. M. von Hornbostel, Beobachtungen über ein- und zweiohriges Hören, *Psychol. Forsch.*, 4, 1923, 64-114.

[71]H. M. Halverson, Diotic tonal volumes as a function of difference of phase, this JOURNAL, 33, 1922, 526-534.

[72]L. T. More & H. S. Fry, On the appreciation of differences of phase of sound waves, *Phil. Mag.*, (6) 13, 1907, 452-459.

[73]H. Banister, Phase effect and the localisation of sound; an examination of the Myers-Wilson hypothesis, *Phil. Mag.*, (7) 2, 1926, 402-431.

results seem to suggest that observed differences may be due to a confusion of the attributes and to unlike sensitivity of the two ears.

The relations of V and phase have been explicitly studied elsewhere only by Halverson. His own accounts conflict. In one report he notes that lateral tones are more diffuse, voluminous and intense.[74] In a later quantitative study of V and phase, however, he obtained that curious curve which caused Boring some difficulty in explaining, where V decreased for a time and then again gradually increased. But for a lateral tone, contrary to his previous report, V was less than for a medial tone.[75] It may be noted that one of Halverson's *Os* (Mc) sat in our experiments. Mc wrote in his final questionary that V increases with a change in phase; but neither his reports nor the reports of our other Os lend statistical support to this statement; unless it should turn out that V may be identified, as an attribute, with B.

There exists a certain plausibility for the attachment of B to phase. The striking B-differences upon the siren disk with a single frequency suggest that the deformation of wave-form from temporally varying interruptions upon the disk is here responsible.[76] The phase-difference in our experiments would seem to produce a similar deformation. If the wave-trains were wholly dichotic (as we have assumed), then the deformation would have to be centrally represented (*e.g.* by the hypothetical 'overlapping' of cortical fields which we have discussed, or, possibly, at subcortical levels).

Just what relation obtains between the brightness variable with the siren disk and with increase in frequency (the higher tones are commonly regarded as brighter) no one, as far as we know, has ascertained. It may be deformation again. Whether brightness-change, as such, acquires in experience a spatial (right-left) coefficient which is read off as localization is worth

[74] H. M. Halverson, Binaural localization of tones as dependent upon differences of phase and intensity, this JOURNAL, 33, 1922, 187.

[75] H. M. Halverson, Diotic tonal volumes as a function of difference of phase, *ibid.*, 33, 1922, 526-534.

[76] Clear brightness differences also appear when episkotister disks with radial sectors are substituted for the concentric holes. The only physical differences at various radial distances upon the episkotister would seem to be the absolute time-gradient at onset and cut-off and air-displacement under changing velocity (see Gundlach, *op. cit.*). This matter, as well as the 'brightness' of high tones and the 'dullness' of low, is being further investigated.

considering.[77] The obvious brightness-difference of vocal sounds also suggest that both the (brighter) overtones of the higher ranges and the several formants which appear to rest upon deformed wave-trains may be involved. The sharp localization of these configurations of tone, formant and noise may be intimately related to these facts of brightness.

The phenomenal similarity between a tone laterally localized and the same tone produced through one ear was exhibited when we slipped off the tube carrying the leading phase and *O* reported only a more lateral localization. The loss and subsequent reentry of this tone (when the tube was again connected) were not observed by *O*, who experienced only a change in lateral placement of the sound. One observer was surprised to hear that all tones in the regular series had had both right and left components. He had supposed that large lateral displacements came from a monaural source. These facts tend to sustain the theory that the prior entry of the sound at one ear (leading in phase) inhibits the full cortical effect of the other.[78]

Summary and Conclusions

This article reports preliminary experiments upon the descriptive character of tones and upon certain conditions under which this descriptive character varies. The special condition here considered is alteration of phase under binaural stimulation. The study seemed appropriate here and now both because it was a logical step in auditory research and because we were able to command several observers who had taken part in similar experimental studies conducted elsewhere, studies which have made

[77]We hope that subsequent experiments comparing the relative rates of brightness-change and of local place may throw some light here. We also hold to the possibility that tactual impressions upon the tympanic membrane may play a part.

[78]The fact that phase-distance could be increased, in our experiments, above 170 cm (=.005 sec. for the rate 100 dv) without producing a dual sound, with a temporal order, confirms Whittmann's criticism (*op. cit.*) of Klemm (*Arch. f.d. ges. Psychol.*, 38, 1918, 71-114) and of von Hornbostel and Wertheimer (*op. cit.*), who set the greatest limit for the perception of a single click at about .002 sec. Whittmann finds the optimal to be .0026 sec, and the greatest difference near .016 sec. In our experiments no significant changes occur around the critical times set by any of these researches; the only significant change being the uncertainty of B-change beyond 100 cm. The discrepancies may be due to errors in the procedure by the various experimenters or possibly to an inherent difference between the localization of tones and of clicks. The reduction of localization based on phase-differences to time-differences, however, seems justified.

distinct contributions toward our present conception of tonal character. We first assumed that the current descriptions in terms of tonal attributes, based upon several careful researches, were sufficiently seasoned for our case; but the outcome of the experiments casts doubt upon this assumption. An exhaustive description of tones in attributive terms is exceedingly difficult. In the comparison of unlike tones in terms of a single attribute, pitch and intensity are most unequivocal and most easily reported upon. Brightness and volume are more equivocal and more difficult of independent variation and report. They appear to be related, in a complicated way, to pitch and intensity. The best means that we could find to define and to identify brightness was to refer it to that attributive variation which prominently appears when tones of like rate but of variable interruption (as from concentric openings) are sounded upon the siren disk.

The difficulties of attributive comparison of tones are greatly increased when phase-difference and consequent shifts in localization are introduced. Here all the attributes are compared with difficulty and uncertainty by observers carefully instructed and serving throughout a long period of practised observation. It appears, however, from a very large mass of such observations that wave-trains of like rate and energy, when introduced separately into the two ears with difference of phase, give rise to a tone whose *brightness decreases* as the phase-difference rises from zero and approaches one-half a wave length and as the localization of the tone migrates right or left from the median plane of the head. This change in brightness may be related to a central deformation of neural impulses which represents two phases of a wave-train at a single rate. No regular and consistent change in pitch, intensity, or volume was observed under these experimental conditions.

AN EXPERIMENTAL STUDY OF VISUAL AND AUDITORY 'THICKNESS'[1]

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Historical setting. Experiments upon visual depth or 'thickness' have centered upon the control of physical and physiological conditions under which the perception is aroused. Hering recognized the psychological aspects of the problem, noting that under identical physical conditions we may see now a plane and again a distance.[2] Katz carried on the work begun by Hering, providing an account of the depth-like modes of phenomenal colors.[3] After Katz, interest in conditions was somewhat obscured by Schumann's alleged discovery that depth was a sensory phenomenon, which Schumann called the 'glassy sensation.'[4] In more recent studies, investigation of the influence of such factors as size, brightness, form, and fixation has kept an even pace with a parallel interest in description.[5]

Auditory depth, magnitude, or volume has a long controversial history and continues to occupy a prominent place in theoretical and experimental contexts. Discussion centers around two issues, the first its attributive status and the second its neurological substrate. Stumpf posited extension or volume (*Ausdehnung*) as an attribute of tones, observing that low tones are intrinsically massive while high ones are small.[6] Because it seemed to be inherent in the tone itself, he recognized volume as an attribute to be added to pitch and intensity.

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[1]From the Psychological Laboratory of Cornell University. The research was directed by Professor Bentley.

[2]E. Hering, in Hermann's *Handbuch der Physiologie*, Bd. III, Teil 1, 1879, 573.

[3]D. Katz, Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung, *Zsch. f. Psychol.*, *Ergbd.* 7, 1911, 1-31.

[4]F. Schumann, Die Repräsentation des leeren Raumes im Bewusstsein, *Zsch. f. Psychol.*, 85, 1920, 224. Cf. also E. F. Möller, this JOURNAL, 36, 1925, 249-285.

[5]H. Henning, Ein neuartige Tiefeneindruck, *Zsch. f. Psychol.*, 92, 1923, 161 f.; H. Schole, Beiträge zur Psychologie der Raumwahrnehmung und räumlichen Vorstellung, *Zsch. f. Psychol.*, 107, 1928, 314-365.

[6]C. Stumpf, *Tonpsychologie*, II, 1890, 56-60.

Like Stumpf, James and others (McDougall, Titchener, Watt, Köhler and Ogden)[7] make volume attributive. James applies 'voluminousness' or 'vastness' to experience of all modalities.[8] Rich has attempted by psychophysical methods to determine whether pitch and volume are co-variable.[9] His results, though meager, seem to indicate that the DL for tonal volume is different from that for pitch and that the two follow different courses. He concludes that they are independent attributes. Observational reports confirm the results of Rich's psychophysical methods. With practice his observers made volumic judgments as readily as reports of pitch. Rich repeated his work on volume under more adequately controlled conditions and verified his earlier results.[10] Halverson, again using a psychophysical method and finding the DL for volume larger than that for intensity, concluded that these are separate attributes.[11] Gundlach, however, attempting to equate the volume of pairs of unlike frequencies, is led to question its attributive status.[12]

In a recent discussion of auditory theory, Boring,[13] who regards tonal volume as a pre-spatial attribute, suggests as a neurological correlate the dispersion of neural impulses in the cortex, dispersion presumably varying with the number of basilar fibers stimulated. Troland further describes a hypothetical mechanism by which neural impulses from the basilar fibers are conducted to the cortex.[14]

Although there has been much research into the outside and the organic conditions of volume, the volumic aspects of visual and auditory experiences themselves have not been adequately described and compared. In the present investigation we have sought to discover by direct means the nature of such experiences in the two modes and to look for significant likenesses and differences be-

[7]G. Rich, A study of tonal attributes this JOURNAL, 30, 1919, 126.

[8]W. James, *The Principles of Psychology*, II, 1890, 134 f.

[9]G. Rich, A preliminary study of tonal volume, *J. Exper. Psychol.*, 1, 1916, 13 ff.

[10]G. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 127-164.

[11]H. Halverson, Tonal volume as a function of intensity, *ibid.*, 35, 1924, 360 ff.

[12]R. Gundlach, Tonal attributes and frequency theories of hearing, *J. Exper. Psychol.*, 12, 1929, 195.

[13]E. G. Boring, Auditory theory with special reference to intensity, volume and localization, this JOURNAL, 37, 1926, 157-188.

[14]L. Troland, The psychophysiology of auditory qualities, *J. Gen. Psychol.*, 2, 1929, 28-58.

tween visual and auditory perceptions of this sort. In order to rid our observations of trite terms and rigid concepts, we have provisionally used the word 'thickness' in place of 'depth' or 'volume.'

THE EXPERIMENTS

General method. The visual and auditory stimuli chosen were of such a nature as to call out to a marked degree the characteristic sought. Therefore it was reasonable to expect that, under a general instruction, observers would give accounts of it in their reports. The *Os* were instructed to give a complete phenomenological description of their experiences, without information upon the purpose of the experiment. From the reports under general instruction, terms were selected that were most representative of the experience of 'thickness.' The next step was to present the same stimuli under more specific instructions. *O* was then directed to note and to describe that aspect which he had called depth, density, or thickness. Finally, in an effort to obtain a solution to the second part of our problem, an attempt at direct comparison of thickness in sight and hearing was made by giving a visual and an auditory stimulus in immediate succession and asking for reports upon likenesses or differences in this aspect.

Apparatus and procedure. Because of its pre-dimensionality and its comparative freedom from object-meaning, film-color was introduced in an early series, utilizing Martin's apparatus.[15] Illumination was furnished by two Corning daylight lamps placed slightly above and facing the color on the back screen. The intensity of the light could be varied and controlled by means of a rheostat and a voltmeter placed in the circuit. All stimuli were presented in a dark room. The *Os* looked through a lightless black hood at the outer gray screen where the film-color appeared.[16] The energy of illumination was represented by 70 or 110 volts and the exposure-time was 3 or 10 sec. As the experiment progressed, it became apparent from the reports that there were secondary cues to thickness in the film-color; the blackness of the hood through which *O* looked could be differentiated from the grey screen which bounded the film-color and the film color appeared on still another plane. In a later series we thereore undertook to provide a visual experience like film but one which would present a uniform field. Visual conditions were so arranged that *O* was

[15] M. Martin, Film, surface, and bulky colors and their intermediates, this JOURNAL, 33, 1922, 459.

[16] The observers were Mr. D. T. Griffin (*G*), assistant in psychology; Miss M. Davidson (*D*); Mr. O. D. Anderson (*A*), and Mr. W. T. James (*J*), graduate students. All had at least three years experience in psychological observation. With the exception of *D*, all *Os* served throughout the experiment. The following Hering papers were used; gray 16, yellow 42, blue 89, blue 86, purple 14, green 9, green 66, red 19, orange 3, red 61, blue 12, green 74, red 1, and yellow 5.

confronted with a nearly homogeneous retinal field. Four inches before the eyes was a vertical plate of mud-ground glass 76 cm. wide by 61 cm. high. At a distance 20 cm. behind it another similar glass plate was set up. The plates were held upright by a grooved wooden frame. At 41 cm. behind the second a colored-glass sheet of the same size was placed in a frame. It was 1/8-in. thick and body-colored. Behind the colored glass, at 35 cm., was a cone-shaped galvanized tin reflector painted white inside and with its flaring base facing the colored glass. The reflector was 25 cm. deep and 75 cm. in diameter. Two daylight lamps of 200-w. each were focussed upon the center of it. The light-intensity was varied by a rheostat. A large black curtain closed off the view of the front screen and served to keep O from tactual cues as to the nearness of the front glass plate. After coming blindfold into the dark room, O was informally instructed to place his head in the headrest, to keep his eyes closed except when instructed to open them, to avoid exploratory movements with his hands, and to maintain a staring fixation straight ahead. He was given at least one minute for partial dark adaptation. Upon the presentation of the stimulus he looked into a visual field which was approximately uniform except for the extreme boundaries at the periphery. The light energies, expressed in volts, were 30, 60 and 95. It had been empirically determined with persons not observing in the experiment that the subjective differences of intensity as between these three energies were roughly the same. Colors were also presented whose intensity of illumination was gradually varied from 30 to 95 and from 95 to 30 volts within a single exposure.

For auditory stimuli, sounds were produced by an audio-oscillator and by tuning forks. The oscillator, which produces a practically pure tone of 1000 d.v. at three variable intensities, is Type 213, manufactured by the General Radio Company, Cambridge, Mass.[17] It was driven by four dry cells of 1½ volts each in a sound-proof box in a distant room and connected with a telephone receiver in the experimental room. The receiver was placed behind and slightly above O's head and the sound set at a known intensity. Later in the experiment, in order to secure a more diffused and less localizable tone, which should be more obviously comparable to the diffused visual experience just described, the receiver was placed 5 ft. before O. A large phonograph horn above the receiver diffused the sound.

It was thought possible that tuning forks of various frequencies might yield a greater variety of descriptions and characterizations of auditory thickness. Forks of low frequency, *e.g.* might result in a different sort of thickness, either in kind or amount, from the constant frequency of the oscillator. Forks of 700, 600, 400, 420, and 256 d.v. were therefore struck by a falling pendulum. A modification of Volkmann's sound-pendulum controlled the intensity.[18] The angle of fall (degrees of arc) was 10, 20 and 30. Again, five Max Kohl forks with properly tuned König resonators produced a sound which gave no localizing

[17]For a description see H. Halverson, The audio-oscillator, this JOURNAL, 38, 1927, 295.

[18]See E. B. Titchener, *Experimental Psychology, Instructor's Manual, Quantitative*, 1905, 195.

cue from manual excitation, since the tone was inaudible until the fork was placed before the resonator. The energy here was uncontrolled. The forks were 128, 192, 256, 288, and 384 d.v.[19]

A short series of noises completed the auditory stimuli in an attempt to determine whether noises afford any additional characterizations of thickness not reported with tones. The Volkmann pendulum was released and the hard rubber end struck against various substances, producing a brief sudden noise. For the production of continuous noises the surface of a rotating drum covered with sandpaper was scraped against a sheet of tin or glass. The speed of the drum was regulated by a system of pulleys and weights. All apparatus and the manipulations of *E* were concealed from *O*.

Throughout Part I (general instructions) the exposure-time was varied with a view to ascertaining whether such a variation had any effect on the volumic aspect of the experience. When no such effect was evident, the variations were discontinued and all stimuli were presented for 10 sec.

The formal instructions in Part I with both visual and auditory stimulation were:

"Observe attentively the exposed area (or the sound experience). After the experience disappears, report. The report should include a complete phenomenological description."

Since the purpose of the study was to obtain all possible characterizations and descriptions of thickness, it seemed best to have *O* report in any terms that were adequate to the experience. For this reason he was instructed to give a 'phenomenological' description. To be certain that the *O*s understood that the term meant simply to take experience as it comes, each *O* was asked, at the end of Part I, for a brief statement of what he had taken 'phenomenological description' to mean. The quotations show that the *O*s understood the instructions in the sense in which they had been intended.

A: "I consider a phenomenological description to be a description in meaningful terms of experience as it appears, as it is given."

D: "I understand a phenomenological description to mean a description of the appearance as a phenomenon, or a directly observable appearance. It is the description of the object or the thing in itself as it appears to the mind."

G: "When I am instructed to give a phenomenological description, I take it that my description need not necessarily be limited to dimensional or attributive terms. I try to tell how an experience is patterned and how it varies as it goes on, using any convenient terms which seem to indicate likenesses, differences, or variations."

J: "The sense in which I have used 'phenomenological' in the reports is to deal with and take experience in its own right, noting the most striking features, describing these in such terms as I can, without going back necessarily to attributes."

The stimuli in Part II (specific instructions) were the same as in Part I. *O* was formally instructed as follows:

"Observe and describe that aspect of the experience which you have called thickness or thinness, compactness, depth, volume, density, and the like. Also report temporal or spatial changes in the course of the experience."

[19] In *Quelques expériences d'acoustique*, 1882, 10-11, R. König gives the vibration-rates of the resonators.

In Part III visual and auditory phenomena were presented in immediate succession and directly compared. The colored glass was used for visual stimulation and the oscillator for auditory. Each of the three intensities of the sound was combined with each of the three intensities of the colors. The time order was so governed that neither invariably came first. Each visual and each auditory stimulus was given for 10 sec. The instructions read:

"You will be presented with a visual experience followed immediately by an auditory, or with an auditory followed by a visual. Observe and report any similarities or differences between the visual and auditory experiences with respect to thickness."

From the reports in Part II it was apparent that the *O*s had been taking thickness in two ways. It was, therefore, deemed advisable to make specific the sense in which they were to compare the visual and the auditory experiences. We first added to the instruction the qualification "thickness in the sense of size, voluminousness, spread in all directions." Then the whole series was repeated under the instruction "thickness in the sense of compactness, density, degree of concentration."

RESULTS

(*1*) *Existence of a multi-directional thickness.* Experience sometimes possesses a characteristic of thickness when directly apprehended under the most general instructions. It appears under visual and under auditory stimulation and is referred to as tridimensional or as extending in several or in all directions. Similar and identical terms are used to describe the visual and the auditory experiences; and they appear in both noises and tones. Under our conditions of stimulation and instruction, thickness is never definitely bounded. It comes as a simple and immediate aspect of the experience. Its size is frequently relative to some previous experience, but only when *O* attempts to report the 'amount' of it. The following representative reports obtained under general instructions show the presence of the characteristic of depth, size, spread, or volume.[20]

Film-color.

A: [orange-110v.] Phenomenologically it is deep and goes back of the form which contains it. The distance or thickness I cannot say, simply because it isn't that sort of deepness, not a rigidly contoured, perfectly definite extension out there; but extended as a mist is extended.

D: [green-110v.] The impression was very strong that the mass of color extended indefinitely beyond the aperture, *i.e.* up and down and on the sides.

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[20]The first letter refers to the observer; next is the quality of the stimulus; next the intensity (for the colors, in voltage of the illuminating circuit; for the tones, either the force of the oscillator-setting—high, medium, low—or the force with which the tuning fork was actuated—loud, medium, soft). Where no exposure-time is given, the time was 10 sec.

G: [yellow-100v.] The fulness or richness or thickness is as if I were looking into more or less dense fluids of the color of the disk. No front or back bounding plane.

J: [orange-110v.] It had considerable spread in every direction.

Tones.

A: [1000 d.v.-high: 3 sec.] This is more space-filling, *i.e.* it extends out there in several directions, up and down. The endings of up-down, back-forward, left-right, are extremely indefinite.

G: [1000 d.v.-high] This was more limited in extent and more definitely bounded.

J: [400 d.v.-loud] It was spread out. It did not strike one as being in a small stream, but as radiating everywhere. The bigness of the thing was one of its most outstanding characteristics.

Colored glass.

A: [yellow-95v.] There is no cue to specific thickness or distance. There is no such thing as direction in the depth at all. It seems to go out and back in all directions. It just isn't deep on a line straight out from my eyes. There is never any hint in any of these that I could take a yardstick and say there's where it ought to stop; and yet somehow the thing begins right out before my eyes.

D: [red-60v.] It gave a very pronounced idea of great mass. It was as though a thick red cloud cut off this part of the world from all the rest.

G: [green-95v.] This had as its most outstanding character that I could keep looking into it. There was no definite stopping point to my gaze. It had considerable volume.

Noise.[11]

A: [7] It isn't soft in the sense that the tones are soft. They have an almost glowlike spread in three dimensions.

D: [6, 7, 8, 9] All these four sounds seem localized a few feet on the other side of the curtain. They did not give the impression of indefinable distance nor of diffuseness which the tones gave.

G: [1] Although it did not have a specific volume of its own, it was voluminous.

(2) *Description of the experience of thickness.* The psychological description of thickness is not an easy task. Under general instructions *O*s gave detailed accounts of widely different aspects of their experience. They reported on the quality, duration, and texture of colors and sounds, the beauty of the colors or the roughness of the noises as well as on thickness. In finding expressions that adequately characterize the experience, the *O*s frequently resort to some context, simile, or meaning upon which the perception appears to depend. These settings are so varied that they are almost indeterminate. The characterizations, however, do fall roughly into three groups.

First, there are the descriptions in which there is little or no dependence upon contexts, as when the experience is described as 'concave,' 'soft,' or 'penetrable.' These are comparatively simple

[11]The noise stimuli and their numbers were: 1 wood, 2 corrugated paper, 3 felt, 4 rubber, 5 leather, 6 tin (one weight), 7 glass (one weight), 8 tin (three weights), 9 glass (three weights).

descriptions in the sense that the characterization fits the experience directly, without recourse to a more complex situation. We shall call this type *unsupplemented descriptions.*

Secondly, the perception may be supplemented by certain contexts, *e.g.* it may be described as bound-up-with, and dependent-upon, a particular grained aspect of a color. In this type of context thickness and the contextual material are of the same modality, both visual. In apprehending the organism uses the context presented by visual stimulation. This type of descriptions will be referred to as *intra-modal supplementary contexts.*

In the third group, the organism uses resources from other modalities, *e.g.* a tone is apprehended as 'thick' or 'thin' on the basis of the size of an accompanying visual image. These we shall call *extra-modal supplementary contexts.*

Group 1. Unsupplemented descriptions. Experiences possessing depth, volume, or thickness are described as soft, yielding, penetrable, compact, smooth, or globular. These characters are co-existent with, and somehow imply or carry the meaning of, thickness. It is significant that the list of unsupplemented descriptions just given appears both in vision and in audition. The inference is that the organism perceives thickness in the same way under various modes of stimulation; and that visual depth and auditory volume are not totally different perceptive moments. It will be remembered that these descriptions were obtained in separate series and under general instructions. The observers were working without knowledge of the problem. No suggestion as regards a comparison was given until the last series, in which *O* was asked directly to observe similarities and differences.

Film-color.

A: [yellow-70v.] The color experience is somehow inherently soft and penetrable. Its penetrable nature, I think, constitutes its depth.

D: [orange-110v.] It seemed to possess somewhat greater depth or density than the preceding; although this depth had no definite limits.

G: [blue-110v.] The disk was somewhat 'look-into-able.' The blue was rich, deep and penetrable.

J: [blue-70v.] It has a sort of substantiality about it. It doesn't quite fit to call it thick; but I suppose that is what I mean, a substantial consistency with a body to it.

Tones.

A: [700 d.v.-soft] It was extremely smooth, rather mellow, penetrable. I wish I knew what penetrability means in this instance. It was soft. Softness seems to be penetrability. The sound somehow carries the meaning of 'giveyness', of penetrability, of a certain looseness, a certain softness in the

sense that a thick vapor is soft. This softness, this penetrability, is very closely bound up with the meaning of spread, of tridimensional extensity. It means on its very face that it goes in all directions.

G: [1000 d.v.-medium] It was thick, dense, compact.

A: [600 d.v.-medium] Qualitatively the tone is mellow, soft, yielding and smooth-textured, like a film-color. I get the same sort of experience from a film color; —an indefinite thickness, a smooth texture, a lack of hard and fast borders. The only difference is that one is visual and one auditory.

Colored glass.

A: [red-30v.] The depth is undoubtedly a filmy depth, filmy in the sense that it is not definitely localized as to position away from me and in the sense of its being soft and penetrable. Its extent is extremely indefinite. All that I know is that the total thing is a globular mass, fairly luminous.

D: [yellow-30v.] Its most prominent characteristic was its thickness and opaqueness. There was no feeling that one could have seen through it.

G: [lavender-95v.] Everything was pinkness. It was penetrable as if the pinkness were a substance in which I was immersed.

J: [lavender-95v.] It was more like a film than anything else; it had no beginning or end. I seemed to be engulfed within it.

Group 2. Intra-modal supplementary contexts. Our stimuli had been carefully chosen so that secondary cues to depth and volume would be eliminated as far as possible. But under the difficult task of observation the *O*s sometimes apprehended the thickness aspect in terms of other constituents which they found in the visual or auditory content. Such constituents are color, pitch, and intensity. These contexts and the particular way they appear may be grouped under three heads.

Quality. The color or the pitch bears a relation to thickness.

Intensity. The degree of brightness of illumination or the degree of loudness may furnish a cue. Quality and intensity frequently overlap. The *O*s are uncertain about the possibility of separating them, particularly in audition. In an experiment on tonal volume, Rich at first found similar inability on the part of his *O*s to be certain what volume was.[22] After a period of training, however, judgments of volume were made as readily as those of other attributes. Our results in no way conflict with Rich's. His problem was one of analysis. We have attempted to describe volume as it comes, unstripped of experiential accompaniments. Whether the description can be made in terms of simples or whether it is sometimes bound up with other aspects, the results are equally valuable for our purpose.

Texture. Textural variations, such as the granular appearance of a color or the 'wiry' aspect of a tone, are reported as determining factors.

[22] G. Rich, A preliminary study of tonal volume, *J. Exper. Psychol.*, 1, 1916, 13 ff.

In the quotations below the overlapping of the three types of contexts is evident as well as the similarity between vision and audition.

Quality.

A: [red-30v.] That's a very thick, dense, impenetrable thing. I feel that the color and the degree of illumination have a great deal to do with this.

G: [blue-95v, decreased to 30v.] It was less thick at the end when the blueness dropped out.

A: [1000 d.v.-high: 3 sec.] That pitch carries with it the meaning that it is not bulky, nor is it extremely fine or thin. The spatial meaning that is referred to by the loudness is that the room is filled up with it. The two uses of the spatialness of sound—the space of the pitch and the space of the loudness—are very confusing. I can't observe my way through them. I can't take pitch and volume separately.

G: [256 d.v.-3 sec.] It has the same mass as a pure tone of very low pitch

Intensity.

A: [yellow-60v.] There are on this illuminated area, near the center, spots of a diffused nature. All that I can say about them is that they are simply darknesses in this light area. The nearer the center of this area, the deeper the phenomenon is. Those darkish areas are nearer to me than the smooth part of the colored area.

D: [red-30v.] When the experience first came, the red appeared thinner than it did later when it became darker.

G: [yellow-60v.] It seemed as if there were a number of bright points in it. Where these points were, *i.e.* between the darker spots, light was coming through somewhere.

A: [1000 d.v.-low-6 sec.] The loudness has a great deal to do with how big the thing is, *i.e.* how much space it takes up. The fact that it was less loud physically determined its being taken as smaller. Further, the pitch is such-and-such an extent. There is an intimate relation between the pitch and the volume so far as its depth or thickness is concerned. I can't take these things separately.

G: [1000 d.v.-low] I am rather bothered about volume. I feel uncertain of my volumic judgments. It was less loud and that determined its being taken as more limited, smaller.

Texture.

A: [red-110v.] Fine particles make up the thing. The particles are alive; they don't stay put. They are working and interweaving among themselves. They are almost sparkle-like. I feel that the depth is due to that peculiar liveness of the graining.

G: [purple-110v.] It had over it a certain lability. It was as if I were looking over a field of such color which happened to be over a heated surface. Corresponding to this there was an increase in the depth-volume.

J: [purple-70v.] It was a little rough. There was something like small particles in it that made it seem diffused.

A: [320-soft] There is a rapid sifting down of extremely fine particles. It is alive, moving, progressing. This sort of onward flux is no more enclosed by specific edges than is a mist. It is there and it is thick.

G: [1000 d.v.-high] Here I think that thin means tiny, pinched up together.

J: [1000 d.v.-high] It is very thin and very compact.

Group 3. Extra-modal supplementary contexts. Thickness may be realized through an imaginal context, *i.e.* one of an unstimulated modality. In vision these contexts were chiefly tactual; in audition, predominantly visual, rarely kinaesthetic and tactual.

There is, in Group 3, greater variation among the *O*s in the degree to which they relate thickness to contexts. Thus we are led to believe that such imaginal supplements are not a fundamental part of the perception, but individual cues to it. Reports in Group 1 have demonstrated that the perception is possible without additional aids. In more than 150 reports, *G* refers to them but once; with *A* and *J* they appear more frequently; while *D* uses them in more than half the reports in the auditory series. Our study was not quantitative and therefore an attempt to quantify any aspect of our descriptive reports will be exceedingly rough; but the table below shows the trend of the type of contexts used and demonstrates the wide individual variation.

EXTRA-MODAL SUPPLEMENTARY CONTEXTS

Stimulus Context	*Obs.*	*Reports mentioning context*	*Total reports*
Visual Tactual	*A*	6	67
" "	*D*	6	47
Auditory Visual	*A*	8	62
" "	*D*	30	53
" "	*G*	1	89
" "	*J*	20	59
Tactual	*A*	7	62
"	*J*	2	59
Kinaesthetic	*A*	1	62

For *D*, the imaginal forms assumed various shapes and designs. So insistent were these images, that, when *D* attempted to separate the visual image from the sound, the sound was again represented by another visual image.

D: [1000 d.v.-medium] There seemed to be two characters which could be attended to. If I attended to the visual image, it remained thin and narrow; but if I attended to the sound itself, it was very diffuse. Even though I was considering the sound in contrast to the first visual image, the sound was translated also into another visual image.

J consistently reported visual images of two types: for small thin sounds, a streamlike image; and diffused sounds were represented by a circular, wavy visual form. The *O*s were uncertain about the exact nature of these contexts. They were called images, meanings, or the experience "was represented by" the formations. It is probable that all these extra-modal supplements refer to some sensimaginal resource of the organism to perceive more completely the object presented. The following reports show the varieties of imagery and the relation between these contexts and thickness.

A: [blue-110v.] I feel that I could have poked something through that circle and my stick would never have been long enough to reach the bottom of the thing. [128 d.v.] This sort of extensity is put in visual terms. It appears as spread-out, grayish, massive, loose stuff that is in motion.

D: [red-60v.] There was a strong conviction that one could immerse one's hand in it. [1000 d.v.-medium] The visual image that presented itself is of a straight line running horizontally about 1/8-in. in thickness and below that an area about twice that size in a lighter gray.

G: [green-30v. increased to 95v.] It seems possible to sink one's finger into this greenish light field, although the voluminousness is not unlimited.

J: [256 d.v.-3 sec.] It was visualised as a stream of water flowing around in every direction from a point. Waves travel in every direction. [1000 d.v.-high] It was like water running over my hand.

(*3*) *Descriptions under specific instructions.* When *O*s were definitely directed to describe the depth, volume or thickness which they had referred to in previous series, they had difficulty in making a clean-cut separation between two meanings of the term. Under general instruction there had been hints of this confusion, but it never became explicit until the later series. These meanings were (1) of spread, size, amount of space taken up, and (2) of compactness or condensation. Though distinct, these two meanings are not unrelated. When an experience is 'very outspread' it is usually also referred to as possessing a low degree of condensation, loosely woven. Smaller experiences are hard, more compact. This confusion of terms might have been avoided somewhat if we had defined for *O*s what we meant by thickness. But that was precisely what we wanted the *O*s to characterize. We were unwilling to separate the two meanings or to suggest, by the instructions, any relationship between them. We preferred to have the *O*s, during the course of observation, come upon whatever differences of relation were there. Fundamentally the difference seems to involve the aspect of the pattern which *O* notes. If he regards the outlines, the contours, he reports thickness in the sense of spread; if he regards the pattern's inner structure, the way it is textured within its borders, it is taken in the sense of compactness.

(*4*) *Incidental results.* (*a*) *Relation between strength of stimulation and localization.* In the visual series, at least, there was an apparent relation between the strength of the stimulus and the definiteness of localization. Experiences from less intense illumination were found to be more localizable than those of greater intensity. This is possibly due to the presence of contours, for when illumination is low there is a slight suggestion of contour which serves as a

cue for vague localization. With medium or intense illumination, there are no borders and no localization. We do not feel justified in inferring any *direct* relation between the intensity of the stimulus and the definiteness of localization, therefore, without further investigation in which the factor of contour is controlled.

Rigid control of auditory intensity was possible only with the sounds from the oscillator. Reports of differences in localization are inconsistent and therefore inconclusive.

(*b*) *Development of thickness during temporal course.* Temporal variations from small to large spread, from surface to depth, from localizability to non-localizability, and from object-meaning to non-objectivity occurred throughout the reports under general instructions. They occurred with visual and with auditory stimulation. At first sight it might appear that there are successive stages in the development of the apprehension of thickness. But when we turn to the reports given under specific instructions, even though the *O*s were directed particularly to report upon thickness and any temporal or spatial change in the course of the experience, we find no such development. *A* and *G* omit all reference to temporal changes, and *D* and *J* state positively that there were none. This is somewhat difficult to understand and to interpret. Had such developments appeared only under film-color conditions the solution might have been sought in accommodation. *O* accommodated first for the gray screen and later for the color beyond the opening. We find, however, a like occurrence in the colored-glass series, where changes in accommodation are improbable. We have, too, a similar variation with auditory stimulation, both before and after initial localizing cues had been eliminated. Our stimuli were not varied qualitatively throughout their course.[23] Since the phenomenon appears under both ocular and aural stimulation, the explanation lies without the physical or physiological conditions of the experiment.

It is not probable that such a temporal development is representative of the way thickness appears in experience for, when the same outside conditions were operative but the instructions different (specific), we find no such change. The factor respon-

[23]The changes in experience under variations of stimulus (when the intensity of the illuminating circuit was gradually increased or decreased within a given exposure) do not belong here and will be discussed in the following section.

sible for this variation, then, must be looked for in central conditions. We offer an explanation in terms of a variation in attitude. Under general instructions *O* was predisposed to look for many aspects of the experience. When the stimulus appeared, he ran the gamut of characterizations that were possible with that particular type of stimulus. In shifting from a predisposition to note color to one of noting form, etc., the function may have been accomplished more slowly than when the predisposition is definitely in one direction. During this shift the experience of thickness is taking place gradually. Disposed as he is, *O* notes only the shift in his own performance and not the attitudinal change. But when he has been instructed specifically for thickness, it is apprehended immediately upon its appearance and there is no temporal shift.

In the series of noises which were presented for a very brief interval, no such changes were reported. If, as we maintain, the variations in thickness depend upon shifts in attitude, it is conceivable that in the 'noise' series the organism was prevented from making attitudinal adjustments on account of the brief duration of the stimulus.

The following reports, given under general instructions, illustrate the phenomenon.

A: [green-30v.] I've never seen surface gradually become film before. First there is a surface, located quite definitely. Suddenly objectivity is taken off it. It softens up, becomes more diffuse, more alive. Gradually there is no surface, no localization. I can't say anything about its depth except that I could look back into it and that it was possibly closer to me than the surface. There was no hint as to how far or how close it was.

J: [blue-30v.] I could literally see it spreading out towards me. The most outstanding thing was the growing of experience. After it reached the highest point of spread it seemed to be right at my eyes.

A: [1000 d.v.-high] It seems to grow and expand in all directions. I don't believe the growing is visual or tactual. The sound itself is simple *spread.* It wells up. It is a spread of penetrable material.

D: [1000 d.v.-low] The volume did not remain constant. It seemed to be rather small; but it gradually became more voluminous. It seemed to widen, spread out. Its breadth became greater. It became more smooth and even. During the latter part, the tone tended to fill experience. It ceased to be associated with any object. At first the sound was there on the other side of the curtain; but later this sense of distance completely vanished.

G: [128 d.v.] The temporal course might be divided into two parts, the second of which closely resembled the first. The second lost the 'meaningfulness' of the first. It was less 'bodiful,' less dense.

J: [600 d.v.] After listening to it, it lost the 'there' reference and became non-localizable. It spread out everywhere.

(*c*) *Emergence of filmy thickness from a 'surfacy' experience, with gradual increase of illumination.* When the illumination was

gradually increased during a 10-sec. exposure, experience became more and more penetrable and filmy and when decreased less. No new qualitative descriptions resulted. Reports of the transition are given below.

A: [lavender-30v. increased to 95v.] At the beginning the thing is almost surfacy about 6 inches away from my eyes, a ragged surface. As it went on, it gradually became softer. Those sharp irregular daubs dropped off. Then a true film appeared, the localization of which was extremely indefinite.

G: [yellow-30v. increased to 95v.] Its filmy plane-likeness seemed to lose whatever localization and surface character of that sort it had had.

J: [red-95v. decreased to 30v.] It extended everywhere before me. It began to back off into the background and to become more dense. At this point it takes on an almost shiny character. At this stage a surface meaning comes in. It didn't have the film character at the end that it had at the beginning.

(*d*) *Differences in thickness between noises and tones.* Our purpose in introducing a series of noises was twofold; it provided a greater range of stimulus and it might reveal significant differences between noises and tones. The reports are probably in large measure a function of our choice of stimuli. The noises were of shorter duration than the tones. We chose noises that were qualitatively simple and which would be perceptualized as little as possible. Therefore, in the interpretation of results, allowance must be made for this temporal difference. The noises were compact, small in extent, dense and impenetrable.

A: [1] The thing was so concentrated that it took up only a little space. It was tight, hard and impenetrable.

D: [1] Its volume was very small. I believe this was influenced by the brevity of the experience and, partly, by the small visual image.

G: [2] It was very small, impenetrable, compact.

J: [3] Was a dull, woody-like thing. It also had spread. It was more easily localized than those that continue for a time.

We have here nothing qualitatively new. The characterizations read quite like those of tones. Localization occurs as with tones before the cues were eliminated. We conclude, therefore, that on the side of volume or thickness noises are much like tones.

(*e*) *Direct comparison of thickness in vision and audition.* When the *O*s were asked to note any similarities or differences between the visual and the auditory experiences in respect to thickness, it was found that there were no gross differences; the differences were always of minor degrees.

A: [1000 d.v.-low, followed by red 30v.] The sound is very small, weak, little. The visual experience was somehow of the same order of size. They were very similar in that their extent in all directions is indefinite and rather soft.

D: [red-95v., followed by 1000 d.v.-high] Although the sound as it continued tended to be spread out in space somewhat the way the color was, it never occupied as great a space.

G: [blue-95v., followed by 1000 d.v.-high] The sound is very much smaller than the visual experience. The latter had a considerable expanse and volume. The former was relatively tiny.

J: [blue-60v., followed by 1000 d.v.-low] The spread and volume of the light were in every direction. The sound was the same way.

In comparing the 'extent' of a visual with an auditory experience, the method of comparison or the criterion of judging degrees of thickness was not precisely uniform for all the *O*s. They do not state specifically *how* they compare; they simply give the results of the comparison. (It will be recalled that our instructions merely directed them to report likenesses or differences; not to comment on the process of comparing.) Obviously our only source of knowing what they did is from the comparisons reported. We can detect two main types of performance. *A* and *J* place a given auditory experience on a scale of auditory extents and the visual on a continuum of visual extents. The relative positions on the two scales are compared. *D* and *G* placed all experiences on a single line, so to say, with auditory at the small and visual at the large end. In reports on density, *A* and *J* again judge in terms of separate continua and compare colors and sounds on the basis of relative position on the continua. *D* and *G* place all the visual phenomena on one end of the scale.

The significant thing, however, is not these slight differences in the mode of performance; it lies rather in the agreement of all the *O*s that there is in visual and in auditory experience a thickness aspect that is directly comparable. We are dealing not with unrelated perceptions of visual depth and auditory volume, but with an aspect of experience which is fundamentally the same whether mediated by the eye or by the ear.

Conclusions

(1) There is a pre-spatial attribute of thickness in vision and audition which is clear-cut in observation. The list of descriptive terms for this 'pre-perception' is practically identical for the two modalities. In both, certain types of experience are characterized as 'big,' 'soft,' 'penetrable,' 'diffuse,' 'filmy,' 'dull,' and 'vaguely contoured'; while others are referred to as 'small,' 'hard,' 'opaque,' 'compact,' 'surfacy,' 'bright,' and 'rather well defined.' The immediate descriptions *may* be supplemented by other contexts,

either from the modality stimulated or in sensimaginal terms. From the wide individual variation in the nature and frequency of these supplementary constituents, we conclude that they are not essential to the apprehension of thickness. The 'pre-perception' is simple and possesses a common character in vision and audition. By simple we mean that, when directly observed by the method of inspection, it is not further analyzable.

(2) Thickness in sight and hearing is not only characterized in identical terms, but is comparable first-hand. Observers without confusion or hesitancy describe and comment upon the thickness of visual and auditory phenomena occurring in immediate sequence. The two modes of stimulation are taken as a single presentation would be; a presentation whose successive aspects are noted and reported as similar.

THE RANGE AND MODIFIABILITY OF CONSONANCE IN CERTAIN MUSICAL INTERVALS

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It is now well-known that simplicity of ratio does not in itself determine the degree of consonance of any interval. Aside from the fact that no mechanism is known by which the ear can react to such ratios directly, it is to be noted that most of the consonant intervals in the equal temperament scale are, as a matter of fact, of very complex ratios, while others, not tolerated at all as consonances, are far more simple. Moreover, it can no longer be maintained that reaction to consonance is purely a cognitive affair, as writers like Stumpf maintained on the basis of an older psychology. It is still good technique in experimental work to avoid the disturbances of affective and emotional factors as far as possible, but we cannot escape from effects of habituation from which no subject is free and which make interesting and constructive differences in the reactions of members of different ethnic groups.

It is also generally known that a number of physical factors and physiological conditions common to all reactors (*e.g.* clashing partial tones or difference tones and roughness between difference tones and fundamentals, as well as physiological conditions in the ear which bring about interference of all periodicities approximating one another in frequency) make certain interval ranges between pairs of gradually diverging tones intolerable; and it is also admitted that between certain of these intolerable ranges lie possibilities of consonant relationships which have not all been cultivated yet by our musical environment. A multiplicity of factors of different kinds unite in different combinations to produce various degrees of tolerance and of preference among the so-called consonances that have evolved in our own civilization. Certain of these consonant intervals are so definitely and favorably located by the physical and the physiological factors, as to be found in common use by practically all ethnic groups. Here we think especially of the octave, the fifth, and the fourth. But even

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these intervals, which have the simple ratios of 1:2, 1:3, and 3:4, respectively, receive about the same sort of reaction from both musical and non-musical subjects when they are slightly mistuned (*i.e.* when they diverge slightly from their true frequency relationships) as when they have the frequency ratios indicated. While this general fact is well known and is employed in the use of the equal temperament scale, we need more data on the reactions of both musically and non-musically trained subjects to the several consonant intervals as they are thrown out of their most simple, or 'true,' relationships in varying degrees. It is this problem to which we have devoted ourselves in the present investigation, which is to be regarded as only preliminary, even though some of our results are highly interesting and will probably be supported in further researches.

Our specific problem was to ascertain how far certain musical intervals in just temperament may be augmented or diminished without marked effect on the reactions of our subjects.

The Experiment

A set of Edelmann tuning forks of fine quality and exact pitch, together with variable forks of closely similar tone quality, were used, all forks being mounted on resonance boxes. The *Ss* were 36 college students, half of whom had had musical training in various lines extending over a period of from 3 to 8 yr. The mean training was 5.11 yr. The other 18 *Ss* had had no training in music whatever. The forks were arranged in pairs, each pair including the tonic (c', 256 d.v.), so that the experimenter, holding in the right hand two rubber mallets, the handles of which were bound together at a fixed angle, could actuate simultaneously any pair of forks which he desired to sound. The rubber mallets were covered with cloth so as to eliminate any high overtones which might disturb the *Ss*, and the forks were sounded so gently and uniformly as to keep the intensity down just to the easily audible degree. Under these conditions the combination tones were not prominent enough to produce perceptible clashes or roughness, and the upper partials were eliminated for all practical purposes. The tones were therefore practically pure tones uncomplicated by secondary physical factors. Every interval sounded was permitted to continue for approximately 3 sec. In every trial *E* sounded these six intervals: major second, major third, perfect fourth, perfect fifth, major sixth, and major seventh, in the order here given. Every one of the *Ss* recorded on prepared sheets the one of the four consonant intervals which seemed to him 'to be changed or unnatural.'

The experiments were carried out on individual *Ss*, except for a few cases who were tested in groups of not over four. Every *S* sat with his back toward *E* about 4 ft. away from the sounding forks, under conditions which excluded visual cues. Preliminary exercises and instructions, which were uniform for

all *Ss*, made clear what was to be done. Every *S* was instructed that in each trial one of the four consonances—*i.e.* one of the four intervals not at the extremes of the series—would be changed slightly, and he was asked to write down on prepared blanks after the trial the number of the consonant interval (3, 4, 5, or 6 for major third, fourth, fifth, and major sixth, respectively) in which he noticed a change, or which seemed to be 'unnatural.' In all trials in which no such changes were noticed he was permitted to omit any judgment, and was asked simply to make a check mark. This, of course, introduced an unequalizing factor into the responses in that some *Ss* will be more likely than others to attempt no judgment on practically the same degree of uncertainty. But since many of our *Ss* were not acquainted with psychological methods we wanted to avoid forcing upon them the seeming unnaturalness and restriction of having to give a judgment of difference when they perceived no real basis for it. To compensate for and throw light on this lack of uniformity of conditions in judgments by the several *Ss*, we took the record of every *S* after the experiment was completed and marked every fourth omission on every interval correct, since this ratio of responses would have been correct by pure guess. In the tables giving results we treat both kinds of data; that is, judgments under conditions not requiring a guess when no change is noticed and 'judgments' handled as if a guess were required. It is to be understood that the data of the latter type have been modified as explained, since it would be impossible to get both kinds of judgments on the same situation. This second type of record, so far as we can observe, is practically what it would be if the *S* had been compelled to guess in all cases of uncertainty, except that the percentages right would probably be slightly larger for both groups.

The mistuning of each of the consonant intervals was effected simply by the substitution of the variable fork for the tonic without any shifting of the positions of these forks. The variable forks had previously been raised or lowered in pitch by a predetermined number of vibrations per second, according to a fixed program. Every one of the four consonant intervals was thus altered 9 times by these several amounts: 10, 8, 5, 3, 7, 6, 4, 2, and 1 d.v., and in the order given, and 10 judgments were secured in a random order on each modification. Half of these modifications were made by raising the pitch of the variable c'-forks and half by lowering it by the number of double vibrations indicated. The former process, of course, diminished the intervals, while the latter augmented them. In addition, 20 trials were given every *S* with no alterations in the justly tempered intervals, to make sure that he was not reacting to mere quality differences in the c'-forks. These 20 trials occurred in an irregular order among the other trials and the *Ss* did not know in those cases that no interval was modified. Thus 380 judgments were obtained from every one of the 36 *Ss*.

Results

In Table I the general results are given in the form of the percentage of correct judgments on each deviation both by the 18 musically trained (MS) and by the 18 non-musically trained (NMS) subjects. The percentages actually correct are given under

A, and the percentages which would have been right if the subjects had been forced to guess on all cases of uncertainty are given under B. The differences, in double vibrations (d.v.), in the percentages correct by the two groups are given in the column at the right under A and B, respectively; and at the extreme right of the table, under the caption B–A, are given the differences between B and A, which differences constitute, for each group of *S*s separately, the percentage of 'right judgments' that were added by the method employed in B (already explained) for each degree of divergence. It will be noted from the differences under A and B

TABLE I

PERCENTAGE OF CORRECT JUDGMENTS IN THE ENTIRE TEST BY 18 MUSICALLY TRAINED (MS) AND 18 NON-MUSICALLY TRAINED (NMS) SUBJECTS

Deviation of interval in d.v.	A: Changed interval indicated if noticed			B: Guess is made when no change is noticed			B–A	
	MS	NMS	Diff.	MS	NMS	Diff.	MS	NMS
10	89	48	41	90	52	38	1	4
8	87	53	34	88	55	33	1	2
7	84	49	35	85	56	29	1	7
6	75	47	28	77	52	25	2	5
5	55	31	24	63	40	23	8	9
4	29	16	13	43	32	11	14	16
3	12	7	5	30	31	−1	18	24
2	3	3	0	27	27	0	24	24
1	2	1	1	26	27	−1	24	26
0	1	2	−1	26	26	0	25	24
						Totals	118	141

that the music *S*s far exceed the non-music in accuracy in all cases of divergence of over 3 or 4 d.v., but on smaller modification these differences do not occur. About this fact more will be said later. If we get the totals of the percentages given for each of the two groups of *S*s in the columns under B–A, and divide the NMS total by the MS total (141/118), we find that the non-musical omitted judgments on an average of 19% more trials than did the musical when both groups were left free to omit judgments about which there was uncertainty. Under the conditions of B, already described, approximately 25% of all judgments will be correct by the laws of chance. This percentage is approximated closely by both groups of *S*s when degrees of modification of intervals are so small (0 to 3 or 4 d.v.) as to be ineffective.

The subsequent tables give data only as if all students were compelled to judge in every trial, that is, to guess whenever there was uncertainty.

Table II compares the two groups of *Ss* as to percentage right judgments both on the augmented and on the diminished intervals. The important thing to note in this table is the very great consistency with which the music students were more correct on the diminished than on the augmented intervals (shown by the negative signs under the column MS on the next to the right column of the table). This same consistency is not found in the results of the non-music students, as shown in the column at the extreme right. This difference in the two groups we shall attempt later to explain when we shall have considered other data.

TABLE II

PERCENTAGE OF CORRECT JUDGMENTS IN THE ENTIRE TEST ON AUGMENTED AND ON DIMINISHED INTERVALS BY 18 MUSICALLY TRAINED (MS) AND 18 NON-MUSICALLY TRAINED (NMS) SUBJECTS

Deviation of interval	Augmented intervals			Diminished intervals			Aug.-Dim.	
in d.v.	MS	NMS	Diff.	MS	NMS	Diff.	MS	NMS
10	89	56	33	91	49	42	−2	7
8	86	61	25	89	49	40	−3	12
7	86	58	28	83	55	28	3	3
6	74	49	25	80	53	27	−6	−4
5	60	38	22	66	41	25	−6	−3
4	40	32	8	46	32	14	−6	0
3	26	31	−5	34	31	3	−8	0
2	26	26	0	27	27	0	−1	−1
1	25	28	−3	26	27	−1	−1	1

Taking the just noticeable difference (j.n.d.) in divergence of each of the four intervals from its perfect ratio as being half way between the percentage right by pure guess (25%) and that yielded at the first point of perfect certainty (100%), *i.e.* $25+(100\text{-}25)/2 = 62.5$, we get for the several intervals the results for the MS group shown in Table III.

TABLE III

RANGE OF UNNOTICED CHANGE BY MUSIC STUDENTS

Interval	Range of unnoticed change in d.v.		Range of unnoticed change in 'cents'
	By the A-method	By the B-method	By the B-method
Fifth	4.45 ± 0.64	4.70 ± 0.60	29
Fourth	4.54 ± 0.64	4.95 ± 0.66	31
Major Third	4.95 ± 0.68	5.15 ± 0.70	32
Major Sixth	5.27 ± 0.75	5.74 ± 0.76	36

Comparable data for the NMS group are not available because even the median point (62.5% right by the B-method and 50% by the A-method) was not reached by this group of *Ss* within the range of 1 to 10 d.v. used in the present experiment; *i.e.* the range of modification unnoticed by the non-musical students in the four intervals and at the pitch considered here was over 10 d.v. It is evident from the data in Table III that the range of unnoticeable divergence from the justly tempered intervals for music students is slightly greater in the case of the major third and the major sixth than in that of the fifth and the fourth, but to test the reliability of this difference it is necessary to use larger groups of *Ss* than were employed in this experiment. Our unnoticeable difference of 29 'cents' for the fifth and of 35 'cents' for the major sixth is not reliable for the 18 *Ss*.

TABLE IV

THE PERCENTAGE OF ERRORS MADE IN SUCCESSIVE TRIALS

	Trials									
Ss	1	2	3	4	5	6	7	8	9	10
Musical*	12.1	11.9	12.2	12.3	10.3	7.1	7.9	9.2	9.1	8.0
Non-musical†	15.6	14.6	11.6	10.8	8.5	8.4	7.7	7.7	7.7	7.2

*324 judgments at every trial.
†270 judgments at every trial.

Our experiment incidentally yielded some direct data on practice effects of the judgments. Taking together all the errors made on the fifth, the fourth, and the major third (nothing different was evident on observation in the major sixth, so it was not considered), we have errors made in 30 judgments by every one of the 36 *Ss* (1,080 in all) on each degree of divergence from 1 to 10 d.v. Since 10 judgments were obtained from every *S* on every degree of divergence on every interval, it is possible to classify the pooled results of all the *Ss* into 10 consecutive trials and to find for every one of these ten trials its percentage of the total errors made in all the ten trials. We have worked out these results separately for the musical and the non-musical *Ss* (except that the results do not include the judgments on intervals of less divergence than 4 d.v. for the MS group, and of less than 5 d.v. for the NMS group; because success within this range was due almost wholly to chance). These results are shown in Table IV. They indicate considerable improvement due to practice through the 10 suc-

cessive trials, despite the fact that the *Ss* did not know the results of their judgments until the entire series of trials was completed. The improvement by the MS group from the first to the 10th trial is 36% and for the NMS group it is 54%. It is appreciably greater in the case of the non-musical than in that of the musical students.

Now, in Table V, we come to results which seem very important both from the standpoint of the psychology of consonance and dissonance and also from that of the possibilities of changes in intervals in actual musical practice. At the top of this table are given in separate heads the four intervals about which we have been concerned in this paper. Going down each of these columns, one finds first the ratios in both just and equal temperament and then the actual vibration numbers. The next line below this in each column indicates whether the interval in equal temperament is augmented or diminished as compared with its size in just temperament. Here we find that for the pitches used in this experiment the equally tempered interval is smaller than the justly tempered by 0.4 d.v. in the case of the fifth; that in the fourth it is larger by 0.4 d.v.; that in the major third it is larger by 2.5 d.v.; and that it is also larger by 4.0 d.v. in the major sixth. The musical *Ss* have been exposed to and have become habituated to this divergence from the ratios more than have the non-musical.

What effect has this difference in habituation to changes in the equally tempered scale on the judgments of the two groups of *Ss*? This question is answered, so far as our data are concerned, in the percentages of right judgments by the two groups given in the lower parts of the table. Note first the records of the musical *Ss*. The interval of the fifth in the equally tempered scale is slightly smaller than the perfect fifth. Therefore it should be easier for the *Ss* to recognize an augmented change in the perfect interval than a diminished change because the augmented change is the reverse of that to which they have become more or less habituated. The results agree with this supposition. In 7 cases of different degrees of modification out of a possible 9 the percentage right is higher on augmented than on diminished changes, while in only two is the reverse condition found. (Cases of no differences are counted ½ each way.) This gives a probability of 0.778 that training was influential, since otherwise the chances would be even for a positive or a negative difference. We have

TABLE V

PERCENTAGE CORRECT JUDGMENTS ON THE SEVERAL INTERVALS WHEN AUGMENTED AND WHEN DIMINISHED

Interval	Fifth (c':g')			Fourth (c':f')			Major Third (c':e')			Major Sixth (c':a')		
Ratio in just temp't	2:3			3:4			4:5			3:5		
Ratio in equal temp't	$1:\sqrt[12]{2^7}$			$1:\sqrt[12]{2^5}$			$1:\sqrt[12]{2^4}$			$1:\sqrt[12]{2^9}$		
d. v. in just temp't	256:384.0			256:341.3			256:320.0			256:426.6		
d. v. in equal temp't	256:383.6			256:341.7			256:322.5			256:430.6		
Interval in equal temp't is	smaller, 0.4 d.v.			larger, 0.4 d.v.			larger, 2.5 d.v.			larger, 4.0 d.v.		
Interval change	Aug.	Dim.	Diff.	Aug.	Dim.	Diff.	Aug.	Dim.	Diff.	Aug.	Dim.	Diff.
10 d.v.	96	93	3	91	96	−5	87	90	−3	84	84	0
8	92	92	0	88	92	−4	87	86	1	81	82	−1
7	92	90	2	93	89	4	86	87	−1	76	82	−6
6	86	84	2	86	86	0	62	79	−17	60	74	−14
MS 5	72	67	5	64	62	2	56	67	−11	44	54	−10
4	47	42	5	47	49	−2	33	42	−9	31	40	−9
3	32	31	1	28	36	−8	28	34	−6	26	32	−6
2	26	27	−1	26	25	1	32	28	4	27	29	−2
1	27	27	0	25	28	−3	25	26	−1	27	28	−1
	Ratio + to −, 7:2			Ratio − to +, 11:7			Ratio − to +, 7:2			Ratio − to +, 17:1		
Prob'ty of infl. of training	Yes		0.778	Doubtful		0.611	Yes		0.778	Yes		0.944
											Mean	0.778
10	62	56	6	59	53	6	53	46	7	46	47	−1
8	57	57	0	71	61	10	58	53	5	52	47	5
7	60	54	6	57	52	5	56	50	6	52	47	5
6	52	54	−2	53	59	−6	44	52	−8	42	46	−4
NMS 5	42	42	0	40	44	−4	37	38	−1	30	37	−7
4	33	27	6	34	30	4	30	33	−3	26	37	−11
3	29	31	−2	30	33	−3	36	31	5	27	28	−1
2	28	26	2	26	31	−5	32	26	6	26	26	0
1	29	28	1	30	28	2	30	28	2	24	23	1
	Ratio + to −, 2:1			Ratio − to +, 4:5			Ratio − to +, 1:2			Ratio − to +, 11:7		
Prob'ty of infl. of training	Slight or ?		0.667	No		0.444	No		0.333	Doubtful		0.611
											Mean	0.514

therefore listed this case ('yes') as showing the effect of habituation to equal temperament in the case of the music students. The effect is not so marked (probability of influence 0.667), and is listed as 'slight or doubtful,' in the case of the non-musical *Ss* who show 6 in 9 cases of higher percentage right on the augmented changes.

In the case of the fourth, in which the equally tempered interval is slightly larger than the perfect fourth, we find the opposite effect in the percentages right by the MS group, but the effect is only slight ('doubtful'), the ratio of cases of higher percentages right on the diminished interval being only $5\frac{1}{2}$ in 9, or 11 in 18 (probability of influence 0.611). It is not present or slightly reversed in the NMS group (probability of influence only 0.444). These results are probably to be expected because of the small difference between the just and the equal temperament interval. Why the probability of influence by habituation should be so far below 0.500 we do not know.

The other two intervals, however, the major third and the major sixth, are considerably larger in the equally than in the justly tempered scale, the difference being absolutely greater in the major sixth than in the major third; and the results of the musical *Ss* accord well with the degree of these two differences. In both cases the diminished changes effected in the experiment were more clearly recognized than were the augmented changes, but the probability of influence, as would be expected, is greater in the major sixth (0.944), whose justly tempered interval diverges absolutely more in the other direction, i.e. is augmented more, than it is in the major third (0.778). Since in both cases the *Ss* of the MS group had become habituated more than had those of the NMS group to the augmented changes in equal temperament, they more readily noticed changes from the just temperament interval in the other direction, i.e. diminished changes, than changes in the same direction (augmented). This tendency is hardly present in the NMS group of *Ss*, going for them one way in the major third (probability of influence 0.333) and another in the major sixth (probability 0.611). Why the former is so much below 0.500 we cannot say. In no case, of course, did any of the *Ss* know the direction of change in the intervals to which they responded.

When we take into consideration all the judgments at different d.v. deviations from the ratio of the justly tempered interval in the fifth, the fourth, the major third, and the major sixth together, and note that if habituation to the equal temperament has no effect the chances are even that each group on each of the 36 (4 x 9) judgments will make a positive or a negative difference, we can determine the probability of influence by habituation in the case of each group. For the MS group this probability is 0.778 ± .0660,[1] and for the NMS group it is 0.514 ± .0795. The difference is 0.264 = .1033, indicating a probability of about 958 in 1000 that the difference is not due to chance. Taking only the last two intervals (major third and major sixth), in which the change in equal temperament is larger, we get a difference of 0.389 ± .0965 (probability of 0.861 ± .0549 for the MS and of 0.472 ± .0794 for the NMS group), indicating a probability of about 999 in 1000 that the musical *Ss* were more affected by habituation than the non-musical. This difference reaches the point commonly regarded as statistically reliable.

From these data, involving a large number of judgments (9 x 10 x 18 = 1620, omitting 20 x 18 cases of no changes in just temperament intervals) on every interval by each group, the conclusion seems justified, that effects of habituation on reactions to consonance are perceptible even in the case of musical training extending over only a few years. These results[2] agree with those

[1]Cf. K. J. Holzinger, *Statistical Methods for Students in Education*, 1928, 248, formula 106.

[2]M. F. Meyer (The musician's arithmetic, *Univ. Missouri Stud.*, 4, 1929, 141-144), working on the basis of a report of this work which he evidently did not understand very adequately from the senior author's brief presentation, has commented interestingly on our results. While his suggestion of using the octave as one interval is good, he overlooks the fact that the fifth and the fourth are also practically unchanged in the equal temperament and that our differences in effects of habituation on the two groups of *Ss* are unreliable in the case of these intervals. This would evidently also be the case with the octave. We hope, however, to try this out in a further experiment including also other intervals. He has grossly misunderstood our report and abstract in taking us to mean that habituation is the only factor operating to make certain intervals 'acceptable.' He seems also to have overlooked an article on consonance by the senior author of this study (Joseph Peterson, A functional view of consonance, *Psychol. Rev.*, 32, 1925, 17-33), which should have corrected his erroneous understanding of our view. We, of course, do not regard Meyer's vague phrase "biochemical affinities of tones" as explanatory, though we recognize the value to psychology of much recent physiological research on the nature of the nerve impulse. Meyer's own 'applications' to consonance perceptions of ideas assumed from these researches cannot be taken seriously by anyone who is not satisfied with mere phrases like that mentioned, "the power of 2," etc. This 'power of 2' has been scientifically (*Continued on next page*)

obtained by Meyer[3] and by Moore.[4] Habituation during the life of an individual is evidently an important determinant of consonance in any musical interval. There is no desire to imply in this report that persons not explicitly trained in music are free from the effect of habituation here pointed out, but only that the training given the music students is such as to increase this effect. It is desirable to continue this investigation with *S*s working on such instruments as the violin and the 'cello and who have as little contact as possible with the equally tempered instruments.

SUMMARY AND CONCLUSIONS

Two groups of 18 college students in each, one group (MS) trained in music and the other (NMS) not, were individually taken through experiments in which they were to indicate which of four musical intervals (fifth, fourth, major third, and major sixth) was "slightly changed or unnatural." In every experiment one interval was either augmented or diminished to the extent of 1 to 10 d.v. in a chance order. Occasionally as a 'control' no interval was changed. The *S*s were permitted to omit a judgment and simply make a check mark whenever they did not notice any change. In individual records every fourth of these omissions was marked right, since by chance they could guess correctly 25% of the time. Every one of the 36 *S*s made 380 judgments.

The results show that for the MS group the ranges of unnoticed change in the fifth, the fourth, the major third, and the major sixth were 4.70 ± .60, 4.95 ± .66, 5.15 ± .70 and 5.74 ± .76 d.v. (or 29, 31, 32, and 36 cents), respectively, while for the NMS group it was over 10 d.v. on all intervals. A given absolute change was recognized in the first two intervals more readily than in the last two. Changes in the justly tempered intervals in the

(*Continued*) analyzed to a considerable extent by W. V. D. Bingham (Studies in melody, *Psychol. Monog.*, 12 1910, (no. 50), 1-88). The successive presentation of our intervals *could* be employed in an experiment like this, it is true, but it would at first not be as precise in its bearing on the present problem, since it would involve slightly different factors. The acceptability of successive tones is certainly also conditioned by habituation, though not based fundamentally on this, as is also true of the simultaneous method we have employed. Meyer overlooks the reliability of the differences in judgments of our two groups.

[3] M. F. Meyer, Experimental studies in the psychology of music, this JOURNAL, 14, 1903, 207-214.

[4] H. T. Moore, The genetic aspects of consonance and dissonance, *Psychol. Monog.*, 17, 1914, (no. 73), 1-68.

THE EFFECT OF ATTITUDE UPON FEELING[1]

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In the course of an extensive study of affective experience it was found that feeling varies markedly with the nature of the initial attitude or 'set' which the *O* adopts immediately before the stimulus is presented.[2] This led us to make an intensive study of the effect of certain specific attitudes upon feeling. The attitudes chosen for investigation were (1) the critical affective attitude, (2) the critical perceptive attitude, and (3) the naive or common-sense attitude.

Our experiments fall into three groups, distinguished by the type of instructions employed.[3] In Group I, we used a general instruction, requiring a description of the total experience from the moment preceding the presentation of the stimulus (initial attitude) to the termination of the affection. The observational attitude was not defined. In Group II, we used specific attitudinal instructions. The *O*s were required to adopt a particular observational attitude, as defined in the instructions. No instructions were used in the experiments of Group III. Experience was allowed to run its course without artificial determinations, and the *O* was then asked to give a retrospective report.

Group I includes 10 series of experiments, and 772 separate observations. 183 different stimuli were used. They were simple sensory stimuli of all modalities (Ser. I-VII); phonographic selections, including classical, sacred, and popular music (Ser. VIII); colored prints (Ser. IX); and an assortment of cartoons,

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[1]From the Psychological Laboratory of Cornell University. The research was directed by Professor Hoisington.

[2]The term *attitude* is used throughout this paper in the sense of a psychophysiological set or predisposition toward a particular type of response. *Cf.* the 'sensory set' *vs.* the 'muscular set' in the reaction-time experiments.

[3]The experimental work was done at Cornell University between October 1927 and April 1928. The *O*s were Dr. L. B. Hoisington (*H*), Assistant Professor of Psychology; Mr. O. D. Anderson (*A*), Assistant in Psychology; Mr. G. L. Freeman (*F*), a graduate student; and the author (*W*). All of the *O*s had had considerable previous experience in psychological observation. Miss Emeline Moul kindly acted as *E* whenever the author observed throughout the experiment.

A detailed description of the experiments, with complete lists of the stimuli used, may be found in the original thesis, entitled *An Experimental Study of Affective Experience,* which is deposited in the Library of Cornell University.

comic pictures, jokes, simple problems, statements designed to arouse affirmation or negation, *et cetera* (Ser. X). The general procedure was uniform throughout this group of experiments. The details varied as the stimulus situation demanded. The stimuli were presented in haphazard order. *O* was seated in such a position that he could not see *E*'s manipulation of apparatus. Two signals were given before each observation, "Ready" and "Now." At the *ready*, *O* closed his eyes, except when the stimulus was visual, and prepared to observe. The stimulus was presented immediately after the *now*.

Group II includes 3 series of experiments, and 289 separate observations. A mixed group of 30 stimuli, selected from Ser. I-X (3 stimuli from each series), was used.[4] The instructions for the first series of the group (Ser. XI) required the adoption of a strictly common-sense attitude, the kind of an attitude with which one takes affective experience in everyday life. In Ser. XII, the instructions called for a purely perceptive attitude, an attitude of inquiry regarding the nature of the stimulus experience. In Ser. XIII, the *Os* were instructed to adopt their 'normal' attitude for the experiment. The 'normal' attitude for all 4 *Os* was a critical affective (or affective-sensory) attitude. The procedure in Ser. XII and XIII was essentially the same as in the experiments of Group I. In Ser. XI, the usual procedure was modified so as to facilitate the adoption of a common-sense attitude. With this end in view, the stimulus-situation was made as informal as possible. The *ready-now* signals were abandoned, and the stimuli were presented with such a statement as "I am going to give you a smell," or "I want you to look at this picture." Sometimes the stimulus was presented without any warning, taking *O* by surprise.

Group III consisted of 10 affective situations, so arranged that the *O* did not realise that he was being experimented upon until, during the course of the feeling, he was asked to report. His attitude was consequently naive.[5] 32 such reports were recorded.

RESULTS

(1) *Variation of attitude under general instructions.* The *Os*' reports in the work under general instructions (Ser. I-X) show that their initial attitude—the set with which they awaited the stimulus—was by no means constant. It varied widely and unpredictably. When minor variations are properly subordinated, three main types of attitude emerge; the critical affective, the critical perceptive, and the naive or common-sense attitude. All of the attitudes reported represent either one of these 3 types or intermediate forms.

a. The critical affective attitude. The critical affective attitude is described as a set to react affectively to the stimulus, and

[4]The same group of stimuli was used in all 3 series except that the musical and ideational stimuli were varied so as to eliminate the effect of habituation.

[5]In one case, *e.g.*, *E* asked Observer *A* a question regarding one of his reports. While *O* was reading the report, *E* placed a piece of cold steel on the back of his neck. A moment later he was asked to report.

to observe the total experience critically. It is a set more for feeling than for perceiving. Under this set, the O is predisposed toward affective experience, more specifically toward affective *content* rather than affective *significance*.

b. The critical perceptive attitude. The critical perceptive attitude is a set to observe critically, to perceive rather than feel, to react perceptively instead of affectively to the stimuli presented. This set predisposes the O toward a clear-cut, non-affective perception of the stimulus-experience.

c. The common-sense attitude. The common-sense attitude is the attitude with which we meet affective experience in everyday life outside of the laboratory, as opposed to the attitude of psychological observation in an experimental situation. It is a 'natural' unrestricted set, a set to react naively to whatever comes without attempting to scrutinize experience. It is not critical. Such a set predisposes O toward affective significances and affective reactions rather than toward affective content. The common-sense attitude may be induced experimentally by appropriate instructions and procedure (Ser. XI).

(2) *Affective experience under the critical affective attitude.* Under the critical affective attitude, experience invariably develops as a pattern of qualitative content, describable in sensory terms. This pattern is usually affective (774 cases out of 809, or 96%).

a. Pattern characteristics. The pattern is typically a total fusion, without any clear-cut division into focus and background. Stimulus-experience and bodily experience tend to merge together, to fuse, more or less at one level of clearness. The degree of fusion varies. In the limiting case, the fusion is complete; there is absolutely no differentiation within the total.

Observer A. (10 I P)[6] It (the affective diffusion) became part of the odor experience; the two were inextricably bound up together. The affective experience was just as focal as the odor experience.

Observer F. (35 VII P.) The bodily response was given as a totality, as homogeneous; diffuse bright pressures and a bright reddish glow all beaten up.

[6] The symbols which precede each quotation from Ser. I-X indicate (1) the period of observation, (2) the series number, and (3) the affective characterization of the pattern, (P, pleasant; U, unpleasant; M, mixed; or O, no affection). In quotations from Ser. X-XIII and XVI, the serial number is followed by a second Roman numeral, which indicates the series to which the stimulus originally belonged. In quotations from Ser. XIV, the period of observation is not recorded.

Observer H. (3 I *U*) Experience lost its focus-and-background character. There seemed to be a single total without differentiation into focus and background.

Observer W. (31 VI *U*) The whole pattern merged together as usual, losing its sharp division into focus and background.

This total pattern has no specific localization. With the affective fusion, experience loses its specific space reference, and the resultant is an ill-defined mass, with only a vague center of reference or locus. This locus varies. It may be (1) bodily, referred vaguely to the body as a whole; (2) abdominal, referred to the abdominal region;[7] (3) stimulus, referred 'out there' in the direction of the stimulus-experience; or (4) transcendent, without spatial reference, not referred to any physical object.

Observer A. (37 VII ?) The total experiential pattern isn't any definitely localized 'out-thereness.'

Observer F. (27 III *P*) It seems to pervade the entire body; it is everywhere and nowhere in particular (bodily locus). (46 VI *P*) Bodily experience is included with the sound out there, slightly projected (stimulus locus).

Observer H. (15 II *U*) A mass of bodily pressure, very vaguely localized, yet somehow referred to the abdominal region (abdominal locus). (19 III *P*) All reference to things drops out and there is just sheer enjoyment; as we say, being carried away completely (transcendent).

Observer W. (30 VI *U*) The sound itself was no longer localized specifically. It was just out there in that general direction.

Although there is no invariable correlation between type of stimulus and affective locus, there are certain unmistakable trends. Intensely *U* stimuli normally give rise to patterns with abdominal locus. Of stimuli which are not intensely *U*, taste, smell and touch induce patterns with bodily locus, as a rule. With auditory stimuli, bodily locus is less frequent; stimulus and transcendent loci are more common. With visual stimuli, stimulus-locus is the rule; bodily and transcendent loci are infrequent.

b. Content. The content of the affective pattern includes stimulus-experience and bodily experience. The latter usually takes the form of the specific affective stuff: diffuse, loosely textured, vaguely localized, pressury-like experience; liveness, brightness, softness, or bright pressure, in *P;* heaviness, dullness, deadness, or dull pressure, in *U*.[8] This specific affective content diffuses through the total experience, altering it qualitatively and otherwise.

Observer A. (26 II *U*) Diffuse, bulky, heavy, voluminous stuff. This seemed to be infused into every bit of the total pattern.

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[7]This type of locus occurs only with *U* feelings.

[8]*Cf.* Nafe's results. (J. F. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 507-544, *esp.* 508-517.)

Observer F. (19 II *P*) Experience became definitely brighter and livelier. The smell quality became more pressury, beaten up with the lively brightness.

Observer H. (49 IX *P*) It was suffused with a lively something like brightness.

Observer W. (34 VII *U*) The whole thing took on a dullish pressury tone. It was a rather loose, weak, dullish pressure.

c. Temporal course. The affective reaction usually occurs very soon after the presentation of the stimulus, before the stimulus-experience is fully perceived. It is as though the affective shift were touched off by the first hint of the nature of the stimulus experience. The development of the characteristic affective pattern follows immediately upon this shift. At the end, the affective pattern disintegrates, the feeling terminating in one of three ways; (i) affective stuff and stimulus experience drop out simultaneously; (ii) affection disappears before the stimulus-experience, and the *O* shifts to a perceptive attitude; or (iii) stimulus experience drops out before the affection, leaving an affective hang-over. In the latter case, the affection is usually referred to, or directed upon, the stimulus-experience that has disappeared. But, in a few cases, no such reference was observed; and the affection seemed to exist independently for a brief interval.

Observer A. (46 VIII *P*) A sort of carry-over in auditory-imaginal terms of the particular part of the piece that seemed to strike me as most *P*.

Observer F. (28 III *P*) It (the odor) dropped out completely, and, at that very moment, the affective pressures and their meaning dropped out also.

Observer H. (18 III *P*) A new qualitative resemblance appeared, and it switched right over (from affection) to perception of the odor. (40 VIII *P*) A *P*-like toning. It had practically no objective reference. The idea of the music did not come in at all.

Observer W. (27 V *P*) The bright liveliness of bodily experience remained for some little time afterwards. There wasn't any reference to the stimulus-experience that had been there. The *P* wasn't referred to anything.

(*3*) *Affective experience under the critical perceptive attitude.* Under the critical perceptive attitude, affective experience simply does not occur. The attitude excludes feeling. Experience develops as a perceptual pattern, sharply divided into two levels, a clear focus and an obscure background.

a. Affective reaction with shift of attitude. Potent affective stimuli frequently induce a shift of attitude and consequent affective reaction. Our *O*s found that it was practically impossible to maintain a constant perceptive attitude, as demanded by the instructions (Ser. XII), over against such stimuli as hydrogen sulphide, the tickle of a feather, and music, which somehow compel an affective reaction.

In 35% of the observations under instruction for perceptive attitude, an affective response occurred. In every case, the *O* reported that he had failed to maintain the required attitude.

b. Characteristics of the affective reaction. When an affective reaction occurs in spite of the initial perceptive attitude, it comes, usually, as the end-phase of a fairly complete perceptual development. The stimulus is fully perceived, reacted to perceptively, before the shift to affection. The affective pattern is incompletely fused, as a rule; the feeling is relatively weak and of short duration, generally terminating with a shift back to the original perceptive attitude before the stimulus experience is gone.

(4) *Affective experience under the common-sense attitude.* Under a common-sense attitude, such as our *O*s assumed in Ser. XI and XIV, affective experience occurs practically as frequently as it does under the critical affective attitude (185 out of 194 cases affective, or 95%). But the nature of the affective reaction differs radically. Under a common-sense attitude, affective experience comes as a patterned reaction rather than as a pattern of qualitative content. The content of the pattern is too obscure to be observed in detail, and it is the meaning, the significance, of the reaction that is the dominant aspect of the total.

The reaction pattern may be described as either positive or negative; a pull forward, *attraction*, or a pull backward, withdrawal, *repulsion*. In the one case, the *O* is attracted toward the stimulus-experience; he is 'open' and receptive toward it. Frequently he tries to get more of it by moving toward the stimulus, breathing more deeply, *etc.* In the other case, he is repelled by a stimulus-experience; he shrinks away from it, attempts to escape from it, by inhibiting breathing, withdrawing the part stimulated, removing the stimulus, or by general bodily withdrawal.

Under this attitude, bodily experience does not have a definite qualitative content. At best, it is vague undeveloped stuff; there is nothing as specific as bright pressure or dull pressure in it. The most that the *O* can say is that there is some stuff in the background of indefinite nature, with perhaps a hint of brightness-dullness.

Observer A. (53 XI-V *U*) The thing was *U* common-sensically in the same way that a fly crawling over one's forehead is *U*. The typical pattern of the stuff wasn't there at all. The thing was irritating, annoying, and that is all I can say about it.

Observer F. (52 XI-III *U*) That was affective in the sense of being disturbing. I don't know whether it was bright or dull. There was a bodily reaction, but I didn't get any of its qualities.

Observer H. (XIV *P*) The whole experience focussed upon the roses, something of a pull toward them. They were beautiful. When I smelled, the odor came as more than just the odor of roses; it was 'that delightful odor.'

Observer W. (53 XI-I *P*) That was a *P* smell. Again, I didn't notice any bodily change when experience became affective. It was simply that the smell was *P*, that I liked it.

The effect of the common-sense set upon the temporal course of affective experience seems to be primarily negative in character. The course of experience varies widely and unpredictably, apparently determined by factors quite apart from the initial attitude. No positive influence is manifest.

Our results show quite clearly that the attitude with which one approaches an affective situation has an important effect upon one's reaction to it. In fact, it determines to a large extent what that reaction is to be. It determines, not only whether one is to react affectively or non-affectively, but also whether the pattern is to be one of qualitative content primarily (sensory stuff), or of reactive content primarily (significant movements and meanings).

The results of the work under the common-sense attitude indicate at least one important reason for the unsettled state of affective psychology; they show why feeling has gone undescribed for so long. When feelings are stripped of the naive common-sense attitude, in which they normally have their being, and are observed under a critical affective attitude, they are reduced to modes of tactual experience; to pressure or something very much like pressure. Such pressures may be regarded as the laboratory equivalent of our normal feelings, but experientially they are far removed from the feelings of our everyday life; they are not feelings in the sense that that term is generally understood. Rather they are laboratory artifacts, products of a specific unnatural observational attitude, yet none the less psychologically genuine.

When we once recognize the importance of attitude as a determinant of affective experience, there need be no incongruity in admitting that the 'affective pressures' found in the laboratory do not occur in everyday life. Indeed, we should not expect to find them there. What we find, in any given case, will be a product of the attitude under which we make the observation.

Summary

1. When observing affective experience under a general instruction to describe the total, the *O*'s initial attitude tends to vary widely and unpredictably.

2. Three types of initial attitude are described; the critical affective, the critical perceptive, and the common-sense attitude.

3. The critical affective attitude is a set to react affectively to the stimulus and to observe critically the total experience.

4. The critical perceptive attitude is a set to observe the stimulus-experience critically without reacting affectively to it.

5. The common-sense attitude is the set with which we take experience in everyday life as opposed to the critical laboratory set.

6. Under the critical affective attitude, experience usually develops as an affective pattern in which qualitative content predominates. This pattern is typically a total fusion without clear-cut division into focus and background or specific localization.

7. Under the critical affective attitude, the content of the affective pattern usually includes diffuse pressure-like experience; affective stuff, which diffuses through the total; bright and lively in *P;* dull, dead, and heavy in *U.*

8. Under the critical affective attitude, the affective reaction usually occurs very soon after the presentation of the stimulus. At the end, the affective stuff may drop out before the stimulus-experience, after the stimulus-experience, or simultaneously with it.

9. Under the critical perceptive attitude, affective experience does not occur. Experience develops as a perceptual pattern, sharply divided into a clear focus and an obscure background.

10. When an affective reaction occurs under instructions for a critical perceptive attitude, there is always a shift away from the perceptive attitude. In such cases, the feeling usually occurs late in the course of experience and drops out before the stimulus experience.

11. Under the common-sense attitude, affective experience occurs as a patterned reaction, in which the content aspect is too obscure to be observed in detail and the meaning or significance of the reaction dominates. Bodily experience comes as vague undeveloped stuff. There is nothing as specific as bright pressure or dull pressure in it.

12. These results show that the attitude with which one approaches an affective situation plays an important part in determining one's reaction to it. The nature of the affective reaction varies widely under different attitudes.

THE MEASUREMENT OF TONUS BY DEFORMATION OF THE TENDON

By G. L. Freeman, Yale University

Psychologists and physiologists recognize the importance of a reliable indicator of muscular tonus in the intact human organism. The extensive literature on the subject indicates, however, that the results obtained by most available techniques are not susceptible of precise quantitative treatment. The trial of several of the existing means of record led the writer to exploit a new indicator which he wishes to describe.

Before dealing with the new method and apparatus, it may aid the reader to recall the several ways in which muscular tonus has been heretofore recorded. There are two types of indicators, mechanical and electrical. Two different mechanical systems have been used; the one records the movement of the mobile limb which is attached to the tendon of the contracting muscle, the other records the thickening of this muscle. The only electrical phenomenon which may be safely correlated with muscular contraction at present is the electromyogram. Each of these methods may be briefly discussed.

Mechanical systems. The earliest means of recording changes in tonus involved the use of the tendon reflexes, notably the knee-jerk. Periodic blows were delivered to the tendon and the movement of the mobile limb was recorded. Since the resistance offered by the tendon is dependent upon the tone of the attached muscle, variations in the extent of the limb-movement were held to be an index of tonus-change. Such a method has been used extensively by Muskens,[1] Lombard,[2] Tuttle,[3] and many others.

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[1]L. J. J. Muskens, Muskeltonus und Sehnenphänomene, *Neur. Centralbl.*, 18, 1889, 1074-1086.

[2]W. P. Lombard, The variations of the normal knee jerk and their relation to the activity of the central nervous system, this Journal, 1, 1887, 2-71.

[3]W. W. Tuttle, The distribution of tone in skeletal muscle, *J. Exper. Psychol.*, 8, 1925, 319, 322.

Similar methods, *e.g.* those of Golla[4] and Travis,[5] record the slight movements of the limb due to increased tonus without evoking the reflex. Another variation, introduced by McKinley and Berkwitz,[6] measures the torque around the elbow-joint as the muscles of the arm sustain various weights.

Since the publication of Dodge's[7] systematic exploration of the knee-jerk, muscular thickening has been used extensively as an indicator of isometric contraction. While this is a more indirect measure, it has certain advantages over records obtained from a mobile limb, notably the escape from the momentum and inertia of the member. A common system of recording thickening uses two Marey tambours connected by an air column.[8] The receiving tambour is placed over the antagonistic muscle and the recording tambour marks on smoked paper. Such systems distort the curves by their inertia and momentum and by the elasticity of the air column. Moreover, protracted fine adjustment is difficult. Somewhat more satisfactory results are obtained by placing a lever over the muscle and photographing its excursions. There are two main limitations to the use of muscle-thickening as an indicator of tonus, (1) the small changes that are produced and (2) the fact that with high magnification these small changes may be masked by pulse and respiration.

Electrical systems. Following Piper's[9] monograph on the electrophysiology of human muscle, the electromyogram has been increasingly useful in the study of muscular contraction. Electrical indicators of tonic contraction cannot, however, be successfully exploited at present. A characteristic electromyogram of voluntary contraction may be obtained from two zinc electrodes placed on the surface of the arm and connected with a string galvanometer which records on a rapidly moving film. The interpretation of the record is simplified, since the significant facts are those of a rapidly oscillating rather than of a steady current. So long as no attempt is made to interpret the constant deflections

[4]F. L. Golla, The objective study of neuroses, *Lancet*, 201, 1921, 118.

[5]R. C. Travis, A study of muscle-tonus and its relation to fatigue, *J. Exper. Psychol.*, 7, 1924, 202 ff.

[6]J. C. McKinley and N. J. Berkwitz, Quantitative studies of muscle tone, *Arch. Neurol. and Psychiat.*, 19, 1928, 1036-1056.

[7]R. Dodge, A systematic exploration of the normal knee jerk, *Zsch. f. allg. Physiol.*, 12, 1910, 1-58.

[8]F. L. Golla, *op. cit.*, 119.

[9]H. Piper, *Elektrophysiologie menschlicher Muskeln*, Berlin, 1912.

of the string, it is quite permissible to use any sort of electrodes, including needles placed directly in the muscle. Such needle-electrodes, first introduced by Rehn,[10] have the advantage of producing a better localized electromyogram.

Recently Lewy[11] (using plate electrodes) and Golla[12] (using needle electrodes) have presented evidence that tonus changes are correlated with the *constant* deflections of the galvanometer-string. These observations are intimately related to the whole question of the possible sympathetic innervation of tonic contraction. This question has been made the subject of exhaustive review by Adrian,[13] Fulton,[14] and Forbes,[15] all of whom conclude that the data do not justify the conclusion. The mass of physiological evidence shows that, with favoring conditions and suitable amplification, small electrical variations of an oscillatory character will be found to accompany tonic contraction. That oscillations have not always been observed with even the most sensitive recorder[16] may be due to the possibility that the action currents involved were in opposite directions. Since the use of pad electrodes gives only the average current for the whole muscle group, the galvanometer-string need not be set in oscillation if the innervation of this group is sufficiently asynchronous.

Records of constant deflections are inconclusive evidence of muscle-tone chiefly because similar deflections are also produced by polarization-changes in the skin, in the electrodes and—in the case of needle electrodes—in the muscle fluids. But there are yet other sources of error. Cobb[17] has exhibited records quite indistinguishable from those of Lewy, which were produced by a

[10]E. Rehn, Elektrophysiologie krankhaft veränderter menschlicher Muskeln, *Dtsch. Zsch. f. Chir.*, 142, 1921, 155 ff.

[11]F. H. Lewy, Die Lehre vom Tonus und der Bewegung, 1923, Springer, Berlin.

[12]F. L. Golla, *op. cit.*, 119

[13]E. D. Adrian, Oliver Sharpey lectures on the interpretation of the electromyogram *Lancet*, 208, 1925, 1229-2344, 1282-1286.

[14]J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement*, 383-426, Baltimore, 1926.

[15]A. Forbes, Tonus of skeletal muscle in relation to sympathetic innervation, *Arch. Neurol. and Psychiat.*, 22, 1929, 247-264.

[16]C. Liljestrand and R. Magnus, Über die Wirkung des Novokains auf den normalen und den tetanusstarren Skelettmuskel und über die Entstehung der lokalen Muskelstarre beim Wundstarrkrampf, *Pflügers Arch. f. d. ges. Physiol.*, 176, 1919, 168-208.

[17]S. Cobb, Electromyographic studies of paralysis agitans, *Arch. Neurol. and Psychiat.*, 8, 1922, 247, 264. See also Review of the tonus of skeletal muscle, *Physiol. Rev.*, 5, 1925, 518-550.

passive movement of the electrodes. And Einthoven[18] has shown that similar deflections may be obtained by the passive movement of a completely denervated muscle.

In summary, then, the oscillatory character of an electrical response is probably the only fact at present acceptable as an indicator of tonic contraction. The slight nature of these responses usually requires triodic amplification. Even assuming the great care which the use of such systems necessitates, the records obtained cannot be treated in a quantitative manner. Frequency and amplitude of the oscillations are the only variables which might be correlated with the force of contraction. Adrian[19] states that frequency is apparently determined by the "natural tendency of the motor neurones to discharge at the rate of 50 a second when their excitability is raised by a cortical effect" and that the amplitude of the oscillations "may depend upon many factors which are not necessarily related to the force of contraction."

The relation of tendon-deformation to muscular contraction. This survey convinces one that the mechanical response of tonically contracting muscle is more susceptible to measurement in human subjects than the electrical. The problem of measuring changes in tonus by mechanical means resolves itself into one of optimal magnification with a minimum of distortion. A likely solution is found in the relation of tendon-deformation to muscular contraction. When the tendon attached to a muscle is depressed by a constant pressure, changes in its deformation will be related to increments in muscular tension. These changes will theoretically be most extensive for the lower range of tensions. The most important difficulty in the exploitation of this method is that the quantitative relation of tendon-deformation to muscle-pull has never been established.

The immediate subject of our exploration was to discover the relationship between patellar tendon-deformation and quadriceps tension. The experiments were carried out mainly on a single subject. The results obtained, however, were not only regular in themselves but also correlated closely with occasional records obtained from two other subjects and with theoretical

[18] N. Einthoven, Sur les phénomènes electriques du tonus musculaire, *Arch. néer. de physiol.*, 2, 1919, 489-499.
[19] E. D. Adrian, *op. cit.*, 1231, 1286.

values. This suggests that the relationship found in the present exploration may be expressed as a law valid for all tendon-muscle groups of all individuals.

Theoretical relationship. Anatomically regarded, the patellar tendon is a *cord* attached to a fixed point (the tibia) at one end and to the quadriceps muscle at the other.[20] The tendon works

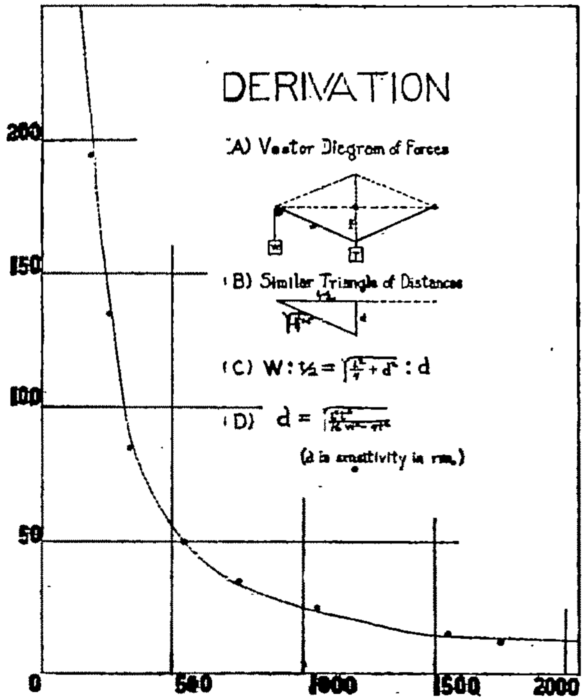


FIG. 1. SHOWING THE SENSITIVITY OF THE DEFORMED CORD FOR VARIOUS PRESSURES. THE DOTS ALONG THE CURVE REPRESENT VALUES OBTAINED IN THE EMPIRICAL VERIFICATION

over the knee-cap, which serves as a simple pulley in the system. When the tendon is depressed we have a system which resembles the theoretical construction of Fig. 1. In this figure and the corresponding formula, T represents the constant weight on a cord and W the pull of a variable weight exerted over a pulley, p. Any increment in pull at W raises T and so lessens the deformation, d. Any deformation of the cord will, in theory, bear a certain mathe-

[20] We shall assume in this treatment that the muscle is pulling directly on the tendon; although the actual pull of the muscle fibers is oblique.

matical relation to the amount of stress exerted at *W*. This relationship is expressed in the formula $d = \sqrt{\frac{L^2 t^2}{16W^2-4t^2}}$. *L* is a constant representing the length of the cord. (In the case of the patellar tendon it would be the distance between the point where the tendon is attached to the tibia and the axis of the knee-cap pulley.) The derivation of this formula is given in Fig. 1, as well as a theoretical curve. This curve represents the changes of a depressed cord effected by various weights when *T* equals 200 grams and *L* equals 50 centimeters. The heavy dots along the curve give the values obtained in an empirical test of the formula.

As the pull at *W* approaches zero, *d* approaches infinity. And as *W* approaches infinity, *d* approaches zero. The point of zero sensitivity for the curve in Fig. 1 is approximated when *W* is 2000 grams. Any further increase in the upper range of weights will have a negligible effect on *d*. Since, however, increase in the lower range of weights results in a disproportionately extensive movement of *T* along *d*, the system of the deformed cord should register the effects of slight pulls to great advantage.

Technical considerations. In our theoretical treatment we have assumed the tendon to be a freely moving cord. This is, however, not exactly the case. Pressure of a constant weight on the tendon establishes an elastic equilibrium not only against the tension of the attached muscle but also against the elastic tissues underlying the tendon. This elastic equilibrium would vary with changes in the tension of the muscle (after the manner expressed in the formula) only in case we could assume that the pressure of the cutaneous and subcutaneous tissues remains constant throughout and that the limb does not move.

Apparatus

Deformation of the patellar tendon was produced and its changes photographically recorded in the following manner. The subject sat in a chair with the right leg stabilized at the knee and with the tibia at right angles to the thigh. The foot was placed upon a movable platform from which an offset rose in the form of a stirrup. The stirrup offset rested on the platform with knife-edge joints, and was held against the tibia by constant pressure. This stirrup carried an adjustable axis which formed the fulcrum

of a lever resting against the patellar tendon. Deformation of the tendon was produced by pressing the lower end of the lever against the skin overlying it. This pressure was approximately 500 grams. A mark was made on the skin below the knee and the pressure was always applied at approximately the same point. Changes in the tendon were photographically recorded by magnifying the movements of the lever which depressed it as follows. A thread attached to the lever at the point of contact with the tendon encircled a cylinder of small known diameter (10 mm.) carrying a convex mirror of suitable curvature. The thread was held under constant tension by a rubber band. A recording camera was placed approximately 17 cm. from the mirror. The changes in tendon-deformation were thus magnified 68 times. (Magnification equals the distance from the camera divided by the radius of the cylinder multiplied by 2.)

Data on the relation of tendon-deformation to muscle-pull were obtained by having the subject raise various weights attached in the rear of the movable platform. This platform was held in position by a stop with the lower limb at right angles to the quadriceps. In the experiments the subject pushed the platform away from the stop just enough for the muscle to support a given weight. An electric circuit, including a Becker marker, recorded this moment on moving photographic paper. Since the measurement was taken at the beginning of the movement, the error from motion of the limb was negligible.

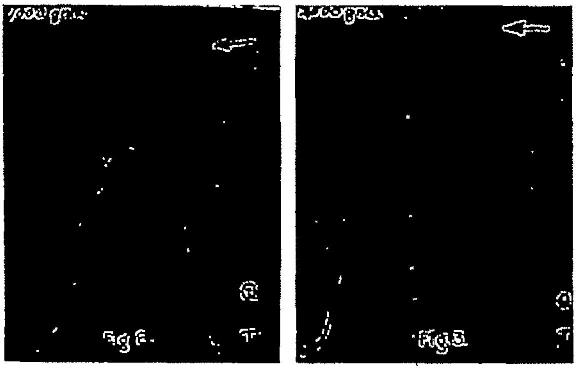
Friction of the system through which the pull of the quadriceps acted was reduced to a minimum, as follows. Three ball-bearing castors under the platform moved over a surface of polished brass. The cord which carried the weights ran over two pulleys with finely machined cone-bearings. The movement of the platform was kept in a straight line by means of two horizontal levers, of equal length, attached by bearings to the platform at one end and to a parallel block at the other. In order to obtain equivalent excursions in the records of quadriceps-thickening, an independent recording system was used, modeled after that described by Dodge and Travis.[21] It magnified the changes in thickening about 15 times.

[21]R. Dodge and R. C. Travis, The relationship between muscle pull and muscle thickening, this JOURNAL, 42, 1930, 295.

The Experimental Results

1. Comparison of recorded changes in muscle-thickening and tendon-deformation. It will be seen from inspection of the records (Figs. 2, 3) that, under the conditions described above, the form of the two curves is much the same. Both begin at approximately the same time (there is an apparent lag of less than 1/50 sec. in the quadriceps record). Both excursions reach a maximum at about the same time, though the tendon record seems to be somewhat more accelerated. Both events have approximately the same period of recovery. It is safe to assume, therefore, that the changes in tendon-deformation, as recorded by our technique, accurately reflect the changes in muscle-contraction as indicated by a lever resting on the muscle. It should be noted that there is an interval of time elapsing between the onset of the records of strain and the lifting of the weights as indicated by the movement of the Becker marker. That is to say, the weights are lifted only when the strain reaches a certain magnitude.

2. Comparison of the two records for various known pressures. The quadriceps muscle was made to lift weights of 500, 1000, 1500, 2000, and 2500 grams, attached to the foot as previously described. Two hundred records for each weight were taken over a period of several days. Distortion due to progressive changes in subcutaneous deformation was distributed as evenly as possible over all records. The deflections corresponding to tendon and muscle changes were read from the base line to a point on the records in line with the 'break.' The averages are given in Fig. 4. These deflections (in mm.) are indicated on the ordinates of the graph and the several weights are represented along the abscissa. The graph of muscle-changes shows that thickening increases in approximate direct arithmetical proportion to increments of weight. Although the graph of tendon-changes is the reverse of the theoretical curve (Fig. 1), the relation expressed is the same. The reason for the reversal is clear. The theoretical curve gives the total deformation of the cord for various weights, whereas Fig. 4 gives the changes in deformation from a depression produced in the tendon by a pressure of 500 grams. If Fig. 1 were turned 180° and the theoretical curve read from right to left we should have analogous curves. The relationship of deformation to pressure-increments is the same, therefore, regardless of



Figs. 2 and 3. Deformation of Tendon and Thickening of Quadriceps with Weights

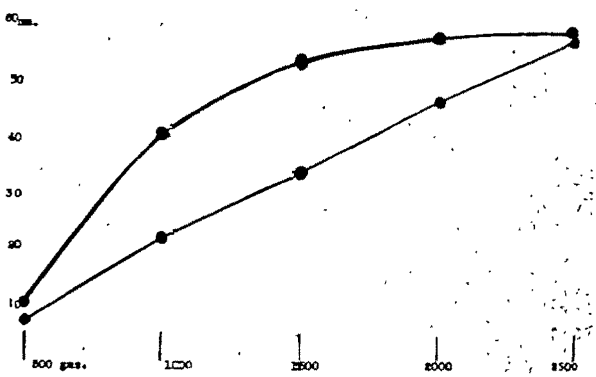


Fig. 4. Average Changes in Tendon (Upper Curve) and Quadriceps Records with Equal Weight Increments

Weight	Tendon change	Quadriceps change
500 gm.	11.1 mm.	7.2 mm.
1000	41	22.2
1500	54.2	34.3
2000	58.4	47
2500	50	58.5

FIG. 5. TENDON AND MUSCLE RECORDS WITH MINIMAL CONTRACTIONS

FIG. 6. TENDON AND MUSCLE RECORDS WITH MENTAL ARITHMETIC

whether the curve is drawn from the zero position of the cord or from a point of initial depression. Fig. 4 indicates that tendon-deformation changes unequally with equal increments in pressure. The greatest change is between the pressures of 500 and 1000 grams. It is presumptively still greater for lower pressures, *i.e.*, the rate of change increases progressively in a manner analogous to the theoretical curve.

It will be noted that the graph of quadriceps-thickening and that of tendon-deformation do not approach the same zero position. This is due in part to differences in optical magnification of the two systems. Since such differences are constant for all readings taken, we may safely infer that, wherever the two graphs are approximately parallel, the sensitivity of the systems is the same. Tendon-deformation, as measured by our technique, is a more sensitive index of muscular contraction than is quadriceps thickening only for pressures below 1000 grams. Pressures above 2500 grams will register little or no effect since above this point the tendon-deformation is not measurably changed.

We feel that the theoretical and empirical curves are sufficiently analogous for us to venture a tentative formulation of the following law. Decreases in tendon-deformation which accompany increases of strain in isometric muscle follow the formula of the deformed cord, subject to a correction (K) for the resistance offered by subtendinous pressures. The change in deformation is most rapid for small pressures and least rapid for large. Pressures above 2500 grams will register little or no effect, since around this point the tendon is no longer noticeably depressed.

3. Minimal contractions. In order to demonstrate the extreme sensitivity of the tendon record for small muscular contractions, a comparison was made between the changes in quadriceps-thickening and tendon-deformation at this level of strain. The lever-system placed over the quadriceps was made to conform to the exact design and measurement of that producing the tendon-deformation. Movements of the two levers were given, by approximate means, the same optical magnification. The subject alternately relaxed and tensed slightly the quadriceps. Fig. 5 reproduces a record obtained under these conditions. The reader should note the relative freedom from pulse of the tendon record in spite of its extreme sensitivity. This is an important advantage.

4. The application of tendon-deformation to psychological operations involving tonus-changes. Fig. 6 shows the increase in tonus (as recorded by our tendon technique) during the solution of a problem in mental arithmetic. The 'make' movement of the Becker marker indicates when the experimenter began dictation of the problem. The optical magnification of this record is 58. The line marked 'c' reproduces the position of a small mirror on the stirrup offset and indicates that the changes in the tendon record were not caused by a mechanical movement of this apparatus. Records such as Fig. 6 suggest a wide field of application of our technique in psychological studies of effort, fatigue, *etc.*

Summary

Our exploration of the measurement of tonic contraction by the method of tendon-deformation leads to the following tentative formulations.

1. With a constant weight pressing on the patellar tendon, decreases in its deformation are in some direct relation to increases in contraction of the quadriceps.

2. The nearest theoretical approximation of the relation of tendon-deformation to muscle-pull is found in the formula for the deformed cord, which was empirically verified by the author. Before this formula ($d = \sqrt{\frac{L^2 \; t^2}{16W^2-4t^2}}$) could be successfully applied to tendon-deformation it would have to be corrected for changes in cutaneous and subcutaneous pressure, the exact nature of which we have no independent means of measuring.

3. Records of tendon-deformation offer an extremely sensitive indicator of small muscular contractions, particularly those known as tonus-changes. The effect of pulse on these records is minimal. The system does not recommend itself, however, to the study of gross muscular contractions.

4. The use of tendon-deformation for the study of tonus-changes occurring during mental effort, fatigue, *etc.*, is practicable; but quantitative interpretation of these changes is dependent on careful experimental controls.

WHOLE AND PART METHODS WITH UNRELATED REACTIONS

By LELAND W. CRAFTS, New York University

The object of the present experiment was to compare, in massed and spaced practice, the efficiency of whole and part methods of learning when materials were used that were both serially and spatially unrelated. We selected materials of this type in order to test the proposition that the value of the whole method of learning varies directly with the coherence and unity of the materials learned—a proposition which is implied in assertions as to the superiority of the whole method for "getting the meaning of the whole,"[1] or for seeing "the connections of the several parts,"[2] and is experimentally supported by the studies of Sawdon[3] and Winch[4] on the learning of poetry.

With the exception of a few experiments with paired associates[5]—the results of which, incidentally, disagree—all of the experiments on the whole and part methods of learning have dealt with serial materials,[6] in which the reaction-elements, since every response becomes the stimulus for the next following response, are closely integrated. Because of this, the author repeated the experiment with non-serial materials, *i.e.* with a card sorting test. The results of this study, which have recently been published,[7] clearly showed that the whole method of learning was, in spite of the fact that serial relations were eliminated, superior to any of the part methods employed.

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[1]A. I. Gates, *Elementary Psychology*, rev. ed., 1928, 364.

[2]R. S. Woodworth, *Psychology*, 1921, 364.

[3]E. W. Sawdon, Should children learn poems in wholes or in parts? *Forum Educ.*, 5, 1927, 182-197.

[4]W. H. Winch, Should poems be learnt by school-children as 'wholes' or in 'parts'? *Brit. J. Psychol.* 15, 1924, 64-79.

[5]L. A. Pechstein, Whole versus part methods in learning nonsensical syllables, *J. Educ. Psychol.*, 9, 1918, 381-387; Warner Brown, Whole and part methods in learning, *ibid*, 15, 1924, 229-233.

[6]With poems, lists of nonsense syllables, paragraphs of prose, mazes, and the like.

[7]L. W. Crafts, Whole and part methods with non-serial reactions, this JOURNAL, 41, 1929, 543-563.

The card sorting test, however, involved spatial relationships; consequently the conclusion could not be drawn that the superiority of the whole method did not depend upon the apprehension of relationships. It seemed desirable, therefore, to continue the study and to employ a type of material which lacked not only serial but spatial relationships as well.

A simple letter-number substitution test was selected, which was non-serial insofar as it did not permit the development of serial reactions. The final responses to be learned were the writing of certain numbers opposite certain letters. As far as the subjects were concerned the order of the letters on the test sheets, and consequently the order of the responses to them, was entirely a matter of chance. One reaction could not, therefore, function as the stimulus for another. The reactions, furthermore, were definitely non-spatial.[8]

The first problem of the present experiment being, therefore, to determine the values of whole and part methods for the learning of responses which are unrelated both serially and spatially, the second problem was to compare the efficiency of whole and part methods for the learning of such responses under massed and spaced conditions of practice. The most important study relevant to this latter inquiry is the well-known monograph of Pechstein's.[9] Using as his material a "very difficult" maze he found the whole method to be superior when the practice was spaced but part methods to be superior when it was massed. His results are no doubt valid. But since they are based on maze learning only, and since we have on this problem only one other experiment—that of Gopalaswami's[10] on mirror-drawing—which although in general confirmatory of Pechstein's is unfortunately very similar to it in the type of reaction employed, it would seem worth while to see if similar results would be obtained with the unrelated material selected for the present experiment.

[8]The movements involved, during the early stages of learning, in turning the eyes to the key or code, may have been guided in part at least by spatial considerations, but as learning progressed those movements became less and less frequent until finally they were entirely omitted. The final number-writing response, *i.e.* whether *S*, for example, wrote a '4' or a '5,' was in no way influenced by them. We feel justified, therefore, in stating that the reactions were not only non-serial but that they were non-spatial as well.

[9]L. A. Pechstein, Whole *vs.* part methods in motor learning: a comparative study, *Psychol. Monog.*, 23, 1917, (no. 99).

[10]M. Gopalaswami, Economy in motor learning, *Brit. J. Psychol.*, 15, 1925, 226-236.

Procedure

Subjects. The *S*s were 281 undergraduate men and women. They were all students of psychology and were drawn mainly from the classes of the writer.

Materials. The materials used were mimeographed sheets. In addition to the instruction and preliminary exercise sheets described below, every *S* received 10 experimental sheets bearing at the top all, or a part of, the following key:

a	b	c	d	e	f	g	h	i	j	k	l
4	10	7	1	9	2	11	5	12	8	3	6

Below this key were 15 horizontal rows of 12 letters each. The letters were arranged by chance except that identical letters were not allowed to come together, and that the same number of every kind of letter appeared in the successive pairs of rows.

The preliminary exercise sheet was identical with the experimental sheets described above except that the following letter-letter key was employed:

a	b	c	d	e	f	g	h	i	j	k	l
p	v	s	m	u	n	w	q	x	t	o	r

Instructions. The following instructions were given the *S*s:

"You will receive sheets of paper at the top of which will be printed letters and below every letter a number. For example: y = 28, z = 41. This constitutes the key or code. Below this will be printed in horizontal rows the letters of the key in a mixed order.

"The work of the test is to write to the right of every letter in the rows the number which goes with that letter in the key. For example, if you were using the sample key above you would write the number '28' after every 'y,' and '41' after every 'z.'

"When the signal to start is given, turn the first sheet face up and begin to work at once. Try for speed. Your score is the number right in the time allowed, 2 min. If you make an error, go on to the next letter without delay; you only lose time if you try to make corrections.

"Begin work at the left of the first row, work across that row, pass to the left of the second row, work across that row, and so on. Take every letter as it comes in the row. Do not attempt any grouping of letters, such as doing 'a' first. If you finish a sheet before the time is up, go on at once to the next one. When the signal to stop work is given, stop immediately."

The *S*s were also informed that the main experiment would be preceded by a 2 min. practice with a letter-letter key. They began at the word 'go,' and stopped at the word 'stop.' All were given the same instructions regardless of the method of practice which they were later to employ.

Amount and distribution of practice. The time allowed for work on every sheet, including the preliminary, was 2 min. The interval between successive sheets was 1 min. Time was kept with a stop-watch.

In the massed practice all the 11 sheets were allotted to a single sitting. In the spaced practice the distribution was as follows: The number of days on which practice was given was 3. The length of the intervals between them was 48 hr. Hence the total time covered by the practice was 5 days. The lengths of period (omitting the 1 min. interval between sheets) were 8, 6, and 8 min. on the first, second, and third days of practice respectively. In terms of

practice sheets the distribution was specifically as follows: on the first day the preliminary exercise and sheets 1, 2, and 3; on the second day sheets 4, 5, and 6; on the third day sheets 7, 8, 9, and 10.

Scoring. The score on every sheet was the number of substitutions made. Errors were so infrequent that no attempt was made to record them.

PRACTICE METHODS

(*1*) *The whole method.* For the *Ss* who worked with the whole method all of the 10 practice sheets bore the entire 12-letter-12-number key, and the letters below it, for which the numbers were to be substituted, were equally divided among the 12 key letters.

(*2*) *The pure part method.* In the pure part method every one of the first 6 sheets bore only a part of the 12-unit key. On some sheets the first four letters (a, b, c, and d) with their appropriate numbers were given; on other sheets the second four letters (e, f, g, and h) and their numbers were given; and on still others the third four letters (i, j, k, and l) with their numbers. The letters in the 16 horizontal rows for which numbers were to be substituted were of course only the four which appeared in the part of the key given. The different part keys occupied the same position on the sheets that they would have occupied had the entire key been presented, *i.e.* if a sheet bore the first four units of the key they were placed to the left of the center of the sheet.

Practice with the above parts was distributed as follows. (a) For the massed practice: on sheets 1 and 2 (after the preliminary) the first part was presented; on sheets 3 and 4 the second part; and on sheets 5 and 6 the third part. (b) For the spaced practice: on the first day, on sheets 1, 2, and 3, parts 1, 2 and 3; on the second day, on sheets 4, 5, and 6, parts 1, 2, and 3 also. In both kinds of practice the parts were successively presented to the *Ss* in either a left to right order, *i.e.* first 4, second 4, and third 4 units of the original key, or in a right to left order reversing the above. Each of these two orders was used approximately an equal number of times. Sheets 7 to 10 inclusive for the last 4 trials were identical with those throughout for the whole method, *i.e.* they bore the entire 12-unit key.

(*3*) *The combination part method.* In the combination part method sheets 1, 2, and 3 bore different 4-unit parts identical with those used in the pure part method. Sheets 4, 5, and 6 bore different 8-unit parts composed of two of the 4-unit parts previously presented. If, for example, sheets 1, 2, and 3 bore what may be termed parts 1, 2, and 3, then sheets 4, 5, and 6 bore, respectively, parts 1 and 2 combined, parts 1 and 3 combined, and parts 2 and 3 combined. The order of presentation of the parts and of their combinations was either from left to right or from right to left as described above in the pure part method. This distribution of practice was the same under both the massed and the spaced conditions. Sheets 7 to 10 inclusive were, as before, identical with those used for the whole method.

Comparison of the three practice methods will show that when *S* reached the seventh sheet, which in all cases bore the entire key, he had, regardless of his previous practice method, worked the same amount of time, namely, 12 min., on 6 sheets, even though the amount of practice, the total number of substitutions actually performed, might have been different.

Selection of groups. The entire experiment was conducted with college classes of 10 to 40 individuals. The sheets for the three practice methods were distributed to the students as they sat in their seats; one student received the sheets for the whole method, the student sitting next to him those for the pure part method, and the one next to him those for the combination part method, and so on. Hence the original 'selection' was made by chance. When the data were tabulated, however, it was found that this method had given unsatisfactory results; certain individuals were obviously far superior to others in substitution test work and by chance more of these individuals were present in some practice groups than in others. Hence it was necessary to *equate* at least roughly the various groups. This was done by eliminating the scores of individual *Ss* until the following average scores had been rendered approximately equal: (a) those for the whole method massed and the whole method spaced groups on experimental sheet 1;[11] (b) those for the two part method massed and also for the two part method spaced groups on experimental sheet 1; and (c) those for all six groups on the preliminary exercise. In addition all *Ss* who failed for any reason to work the full 2 min. on any sheet were eliminated. Hence the number of *Ss* (281) whose records were used in this study, was considerably less than the number of *Ss* (330) who worked in the experiment.

Results

Tables I and II give, for all three practice groups and for both the massed and the spaced practice, the average number of substitutions made on the preliminary and on every one of the 10 practice sheets, and the accumulative totals of the previous number of substitutions for sheets 2 to 10 inclusive.

Since the time spent in practice was equal for all the groups, the number of substitutions made is the principal criterion available for judging the efficiency of the three practice methods. Furthermore, the number made on sheet 7, which involved for all groups the use of the entire key, is, we presume, the best test of their relative efficiency, since after this trial the part method groups became rather groups whose practice had been with the whole method also.

The massed practice. Inspection of Table I shows the average number of substitutions on sheet 7 to have been for the whole method group 93.8, for the combination part 93.2 and for the pure part 88.8. Hence although the pure part method yielded slightly inferior results there is no very significant difference between the

[11]The results of this procedure can be seen in the tables following which show these scores to have finally been averaged at 71.2 and 71.6.

three groups. The scores on later sheets suggest, however, that the whole method group is slightly inferior and that the part method groups are approximately equal.

TABLE I

SHOWING FOR THE MASSED PRACTICE THE AVERAGE NUMBER OF SUBSTITUTIONS MADE BY THE THREE DIFFERENT PRACTICE GROUPS TOGETHER WITH THEIR ACCUMULATIVE TOTALS

	Whole Method		Combination Part Method		Pure Part Method	
	N = 45		N = 53		N = 46	
Sheet	No. of subs.	Previous total	No. of subs.	Previous total	No. of subs.	Previous total
P	58.0		58.0		58.4	
1	71.2	0	93.0	0	92.7	0
2	79.3	71.2	90.7	93.0	107.2	92.7
3	83.7	150.5	94.8	183.7	95.3	199.9
4	86.5	234.2	84.9	278.5	115.5	295.2
5	89.5	320.7	97.7	363.4	101.0	410.7
6	91.9	410.2	95.5	461.1	118.7	511.7
7	93.8	502.1	93.2	556.6	88.8	630.4
8	94.9	595.9	99.7	649.8	100.0	719.2
9	98.1	690.8	102.8	749.5	101.9	819.2
10	100.1	788.9	106.1	852.3	106.7	921.1

TABLE II

SHOWING FOR THE SPACED PRACTICE THE AVERAGE NUMBER OF SUBSTITUTIONS MADE BY THE THREE DIFFERENT PRACTICE GROUPS TOGETHER WITH THEIR ACCUMULATIVE TOTALS

	Whole Method		Combination Part Method		Pure Part Method	
	N = 43		N = 47		N = 47	
Sheet	No. of subs.	Previous total	No. of subs.	Previous total	No. of subs.	Previous total
P	57.3		56.6		57.7	
1	71.6	0	93.1	0	92.6	0
2	78.7	71.6	95.1	93.1	95.8	92.6
3	86.4	150.3	97.1	188.2	98.5	188.4
4	91.9	236.7	88.9	285.3	112.3	286.9
5	98.9	328.6	95.7	374.2	113.2	399.2
6	101.8	427.5	98.6	469.9	112.5	512.4
7	108.5	529.3	102.0	568.5	96.1	624.9
8	113.7	637.8	110.7	670.5	107.0	721.0
9	116.4	751.5	112.0	781.2	111.2	828.0
10	118.0	867.9	112.8	893.2	115.1	939.2

It is worth noting, however, that the part method groups had more practice in the sense of actually performing more substitutions than had the whole method group. For example, although the latter made on sheet 10 a score of 100.1 only, compared with scores of 106.1 and 106.7 for the part method groups, they had previously made only 738.9 substitutions, compared with 852.3 and 921.1 for the other two groups. There was, however, no way

in this experiment of equalizing both time and amount of practice, and of the two time is presumably the more important criterion. Furthermore, inspection of the various accumulative totals shows that even if time were disregarded and amount of practice taken as the criterion the whole method group would still fail to become superior. To illustrate: the pure part group previous to work on sheet 8, on which they scored 100.0, had made 719.2 substitutions. The whole method group had not made as many as 719 until they reached sheet 10, previous to which they had made 788.9. But even on this sheet they scored only 100.1, no higher than the pure part group had on sheet 8.

The spaced practice. Inspection of Table II shows that according to the criterion of number of substitutions on sheet 7 the whole method was superior, the pure part method inferior, the combination part method intermediate in efficiency. Furthermore, although on later trials the pure part comes to equal or even slightly surpass the combination part group, the whole method group remains superior throughout. It may also be noted that the amount of practice, *i.e.* the total number of substitutions previously made, was less for every sheet for the whole method group.

Massed vs. spaced. Comparison of the scores made on sheets 7 to 10 inclusive under massed and spaced conditions shows: (1) The scores of all three practice groups are without exception higher in the spaced practice on all of the above 4 trials. (2) The whole method group is in the spaced practice consistently superior to both the part method groups, whereas in the massed it is at no time superior to the combination part and is superior to the pure part on sheet 7 only. Hence the spacing was of greater advantage to the whole than it was to either part method; or, conversely, massing the practice was most detrimental to the whole method.[12]

[12]The reliabilities of these differences in the scores on sheet 7 are:

		Diff.	S.D.
Massed practice,	Whole-Combination part	0.6	3.52
	Whole-Pure part	5.0	3.73
	Combination part-Pure part	4.4	4.06
Spaced practice,	Whole-Combination part	6.5	5.19
	Whole-Pure part	12.4	5.28
	Combination part-Pure part	5.9	5.46
Massed-Spaced,	Whole method	14.7	4.17
	Combination part method	8.8	4.68
	Pure part method	7.3	4.94

Discussion

The results of this study show that in the massed practice the whole method is consistently inferior (though sometimes but slightly) to both of the part methods used. This inferiority may be due to the following factors.

(1) To the absence of relationships. The absence of important relations (certainly of the serial and spatial) between the reactions learned in this study lends support to the view that the value of the whole method is correlated with the degree of relationship between the responses acquired.

(2) To the effects of transfer. As Pechstein pointed out,[13] and as our results clearly show,[14] positive transfer from one part to another, and the "avoidance of diminishing returns" are factors that intrinsically favor a part method.

(3) To emotional factors. We must consider also, in the interpretation of our results, the factors that Woodworth calls "the emotional," *i.e.* "interest, confidence, and visible accomplishment,"[15] which are closely related to what Pechstein characterizes as the less emotional disturbance, the greater "consciousness of power."[16] It seems probably that in our work with the substitution test the part method groups were, as compared with the whole method group, favorably stimulated by their greater "visible accomplishment" (as evinced by their higher scores on the earlier sheets) and that this factor, together perhaps with the constantly changing, relatively non-monotonous character of their work, helped to maintain in them a greater confidence, interest and incentive—in short a higher degree of motivation.

(4) To fatigue. We must also, following Pechstein further,[17] point to the relation between the difficulty of a given task or problem and the likelihood that the whole method under massed conditions will prove the most economical. A whole method is presumably a more fatiguing, a more effortful mode of practice

[13] Pechstein, *Psychol. Monog.*, *op. cit.*, 58.

[14] Shown most clearly in the present experiment by the higher scores made by the pure part group under the massed condition on part 2 (practised in sheets 3 and 4) than in part 1 (practised in sheets 1 and 2), and by the still higher scores on part 3 (practised in sheets 5 and 6).

[15] Woodworth, *op. cit.*, 345.

[16] Pechstein, *ibid.*, 57.

[17] Pechstein, Massed *vs.* distributed effort in learning, *J. Educ. Psychol.*, 12, 1921, 92-97.

than a part method—for one reason perhaps because of the emotional factors mentioned above—and massed practice is presumably more fatiguing than spaced. Hence for a hard problem the combination of whole method and unspaced practice may constitute a highly ineffective mode of learning. In the present case it is doubtful whether a simple 12-unit substitution test can be classified as a 'hard problem,' but observation showed that for the *S*s of all three practice groups the continuous work with the 10 sheets was a hard task in the sense at least of being a fatiguing one. The factor of fatigue may, therefore, be of especial significance because it would be apt (since it tends to lessen interest and to be itself augmented as incentive decreases) to affect most visibly the mode of practice which is likely to be the least motivated; namely, the whole method.

The results of this study show, in confirmation of the results obtained by Pyle,[18] by Dearborn, [19] and by Starch,[20] that spaced practice is more efficient than unspaced.

We also find, in confirmation of Pechstein's results with the maze, that the whole method is more efficient under spaced than under massed practice. This result suggests that the relative inferiority of the whole method, when the practice was massed, may have been due less to factors intrinsic to it as a mode of learning than to the effect of such relatively extrinsic influences as the motivation and fatigue factors previously described. For all such factors as the degree of relationship between the reactions learned, the positive transfer from one part to another, the avoidance of diminishing returns, the greater visible accomplishment, are intrinsic either to the material employed or to the nature of part methods. Hence they would in no way be eliminated, probably not even significantly altered, merely by a spacing of practice periods. One need not, to be sure, assume that through spacing alone learning by the whole method becomes forthwith more interesting or more stimulating, but it may become less fatiguing; and, as we have already pointed out, the effect of fatigue might, so far as scores are concerned, be revealed most clearly with the least motivated method.

[18] W. H. Pyle, Economical learning, *ibid.*, 4, 1913, 148-158.

[19] W. F. Dearborn, Experiments in learning, *ibid.*, 1, 1910, 373-388.

[20] D. Starch, Periods of work in learning, *ibid.*, 3, 1912, 209-213.

Our results, however, disagree with those of Pechstein[21] in two respects. (a) He found the efficiency of both pure and progressive part methods for learning mazes to be much less under spaced than under massed conditions. Our results for substitution tests show both pure and combination part methods to have yielded higher scores with spaced practice. In his explanation, however, he attributes the inferiority of part methods under spaced conditions to their being under such circumstances too easy; the organism does not develop the habits of "long application" which are essential for the final and difficult task of connecting the parts. But when, as in work with a substitution test, the reactions have no such important serial and spatial relationships as they possess in maze learning, there is of course no difficult connecting of parts to be accomplished. Hence the discrepancy between his results and ours does not suggest that his conclusions are erroneous but only that they are perhaps not applicable to unrelated material.

(b) His most efficient method of practice was the progressive part method massed, our most efficient method was the whole method spaced. Such a disagreement is probably incapable, however, of any completely adequate explanation. The actual score achieved by any method in any given experiment is determined by a great number and variety of factors: determined not only by the intrinsic characteristics of the methods themselves and their adaptability to the particular material employed, but also by such variables as the nature of the always somewhat arbitrarily chosen parts, the nature of the spacing, if any, the degree of motivation aroused and of fatigue engendered, the previous practice habits of the *S*s, and possibly their age and intelligence as well. It is admittedly possible that in the present experiment some one of these factors could have been so varied as to render some form of a part method superior. Nevertheless our data do suggest that for this work with substitution tests the whole method was in some sense really superior to the part methods employed and that spacing the practice had the effect simply of permitting an intrinsic advantage to disclose itself. The source of any such "intrinsic" superiority remains, we admit, uncertain. But it is possible that even in working with a substitution test the *S*s do relate the elements thereof in a manner advantageous to rapid learning; that, for example, they find it

[21]Pechstein, *loc. cit.*

easier to learn that 'b' is 10 if they also observe that 'e' is 9, 'g' 11, 'd' 1, etc. If this be true, then a learning-method which permitted from the first observation of and work with the entire key might finally reveal itself as the most efficient.

Summary

The purpose of the experiment was to compare the efficiency of whole, pure part, and combination part methods for learning material unrelated either serially or spatially, under both massed and spaced conditions of practice. The material selected was a simple letter-number substitution test. The total amount of practice, 20 min., was under the massed condition approximately continuous, but under the spaced was distributed over 3 days separated by 48-hr. intervals. Under the massed conditions the whole method proved to be slightly inferior, or at most equal, to the part methods. This result was thought to support the general opinion that the efficiency of a whole method is correlated with the degree of relationship between the reactions learned. Under the spaced condition, however, the whole method was consistently superior to either part method, and was moreover more efficient than any mode of learning had been under the massed condition. These results agree with those of Pechstein[22] for mazes respecting the greater efficiency of the whole method when practice is spaced. They differ from his, however, in finding the part methods also more effective when spaced and in finding the whole method spaced to be the most economical of all. Although various suggestions were advanced the explanation of these differences remained uncertain. But in any case it seems unlikely that the relative values of whole and part methods in maze learning, which demands the acquisition of responses closely related both serially and spatially, would necessarily be identical with their values for acquiring the unrelated reactions involved in work with substitution tests.

[22]Pechstein, *loc. cit.*

THE EFFECT OF DEHYDRATION ON PAROTID SECRETION[1]

By A. L. Winsor, Cornell University

In a recent extended investigation of factors affecting the occurrence of seizures in epileptic children, McQuarrie has indicated what appears to be a significant relationship between water balance, or the state of dehydration of the body, and the frequency of seizures.[2] Children having as many as twenty seizures a day when the intake of water approximated 2,000 cc. had no attacks when the water intake was reduced to 200 cc. per day. Although a special diet was provided throughout the experiments, McQuarrie concluded that the water balance was the significant factor. The following statements appear in his conclusions: "Convulsions tend to occur when a positive water balance above a certain magnitude is established.' . . . "Suddenly increasing the intake of water during the course of treatment by dehydration tends to cause recurrence of seizures at least in the severe cases." "A disturbance in water balance, perhaps affecting the central nervous system more specifically, appears to be closely identified with the etiology of epilepsy."[3]

This study suggests the possibility of water exchange playing a more important rôle in mental phenomena than has been suspected. Although extensive research has been carried on to determine the physiological and psychological effects of deprivation of food or oxygen, very little attention has been given to the study of the influence of the reduction of water intake. In general it has been assumed and recommended that each individual should flood his system with liquids each day. While such a dictum might have value in some physiological processes, it is possible that it might have disadvantages for at least some individuals. It seems desirable, therefore, that investigations of the effect of water balance on mental activity be undertaken.

Some of the obvious difficulties which confront such an investigation are lack of practical technique for determination of the state of dehydration of an individual at any given time, and lack of exact information on the relative value of various methods of dehydration. The common method employed by physicians has been to determine the quantity and quality of urine excreted over a given period while controlling the intake of liquids. This method has its limitations and the technique and service essential for such a determination is not always available to the person primarily interested in mental phenomena. It is the purpose of this report to suggest another method whereby the state of dehydration may be determined.

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[1]The investigation upon which this article is based was supported by a grant from the Heckscher Foundation for the Advancement of Research at Cornell University.

[2]Irvine McQuarrie, Epilepsy in children: the relationship of water balance to the occurrence of seizures, *Amer. J. Diseases Children*. 38, 1929, 1-17.

[3]*Op. cit.*, 16 f.

Early in his study of gastric secretions in dogs Pavlov noted that these juices were definitely diminished when the intake of fluids was reduced.[4] More recently Gantt,[5] also in the Russian laboratories, found that a reduction of water intake influenced both conditioned and unconditioned salivary reflexes in children. The secretion from both the submaxillary and parotid glands was observed in his study. Cannon,[6] apparently collecting the total salivary secretions at intervals over a period of 20 hours, noted in his own case that reduced liquid consumption lowered by 50% the amount of secretion evoked by chewing tasteless rubber. Similar observations have been made by other investigators working with rabbits and dogs.

Since the quantity of parotid, submaxillary, and gastric secretions and probably of other digestive juices varies in accordance with the amount of liquid in the organism, a quantitative analysis of the secretion of any or all of these glands should afford some index of the water balance of the body. Because of the fact that the digestive secretions are composed largely of water any significant limitation of liquid intake would of course affect them first.

Method

In this series of experiments we have attempted to determine more exactly the extent to which the volume of digestive secretions may be used as an indication of water balance and to evaluate the dehydrating influence of various agencies. Because of their accessibility the parotid glands were used for the investigation. A suction cup was applied over the mouth of Stenson's duct, and the secretion was collected in a graduated tube. A stop-watch was used to time the intervals and readings were made at the end of every minute. Adult male *Ss* were used for the tests.

In order to determine the most consistent and reliable type of stimulation to be used in estimating water balance, an experiment was arranged whereby several sources of stimulation could be used and their relative merits evaluated. By the first method the flow was measured for fifteen minutes while the *S* chewed tasteless gum between the molars on the side from which the secretion was being drained. A metronome beating 60 times per min. provided a timing device to keep uniform the number of chews for each minute and the *S* was instructed to keep his lips closed while he maintained an ordinary chewing pressure. He swallowed on signal at the end of each minute. Following the chewing activity the *S* removed the gum and sat quietly awaiting the next experiment.

The second method involved no direct oral stimulation but attempted to determine the effect of dehydration on the normal flow of saliva. Although the existence of a constant flow of secretion has been overlooked by some investigators our studies[7] support Lashley's[8] findings that such a secretion obtains.

[4] I. P. Pavlov, *The Work of the Digestive Glands*, 1902, 227.

[5] W. H. Gantt, Salivary secretion and the intake of fluid, *Amer. J. Diseases Children*, 37, 1929, 1125-1127.

[6] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear, and Rage*, 1929, 322.

[7] A. L. Winsor, Factors indirectly affecting parotid secretion, *J. Exper. Psychol.*, (in press).

[8] K. S. Lashley, Reflex secretion of the human parotid gland, *ibid.*, 1, 1916, 461-493.

After a 5-min. interval provided for the effect of the chewing to subside, recording of the saliva was resumed for 15 min. to determine the effect of dehydration on this constant although meager flow.

The other method of stimulation was to permit the mucosa of the mouth and throat to become dry and parched, for it had been shown in preliminary tests that this process excited the glands to secrete an extra supply. Cotton was placed in the nostrils and the *S* told to breathe through his mouth by parting his lips slightly without stretching the muscles of the jaw. Swallowing and tongue movements were inhibited throughout the entire thirty minutes of this test. During this period secretion from the other glands of the oral cavity was permitted to flow out of the mouth into an open container over which the subject held his head. The collecting and measuring of the secretion for the various tests consumed about 1.5 hr. and was repeated three or four times a day. Acids were not used for the purpose of exciting secretion because of the difficulties involved in maintaining amounts of equal intensity in a definite area of the mouth and because of the desire to avoid ingesting liquids of any sort during the experiment. After some facility in this technique had been acquired a prolonged thirst was arranged to determine the relative effectiveness of the three situations for indicating the progress of dehydration when the intake of liquids over an extended period was greatly reduced.

Food during the experiment consisted of three pieces of dry toast at each meal. There were three such meals each day at 7:30 A.M., 12:30 P.M., and 6 P.M. Such a diet was designed to reduce the liquids ingested to a minimum and at the same time reduce hunger as a disturbing factor. Throughout the period of the test the subject chewed the same piece of gum so that the mucosa of the mouth and throat would not be dry. Strenuous exercise involving perspiration was avoided but the ordinary activities of the day were carried on as usual.

Results

Figure I shows the quantity of parotid secretion in terms of cubic centimeters obtained for successive trials by the three methods of stimulation. The subject drank his last liquid at 7 P.M. and the numbers on the abscissa represent the successive hours of the thirst after that time. Curve A represents the amount of secretions from one parotid gland when *S* chewed tasteless gum for 15 min. Curve B represents the secretion resulting from 30 min. of mouth breathing, and curve C represents the normal secretion for 15 min.

It will be observed that the secretion from the chewing activity showed a consistent reduction after the first day with significant diurnal changes in the early stages of the thirst. At the end of the thirst the secretion had been reduced to about one-sixth of its normal amount but when 1000 cc. of water were consumed during a 5-min. interval it was restored within a very few minutes to 11 cc. The *S* began to perspire as soon as he drank the water. His temperature had been reduced to 96.6°F.

The lower curves show the same general characteristics as Curve A except that their variations are less marked. Since the demands here on the liquid supply of the body are moderate compared to the chewing capacity it would be expected that mild dehydration would have but a slight effect on the normal

flow. It would appear that some activity such as uniform chewing on some tasteless substance might provide the most satisfactory technique for determining the stage of dehydration of an individual with this method.

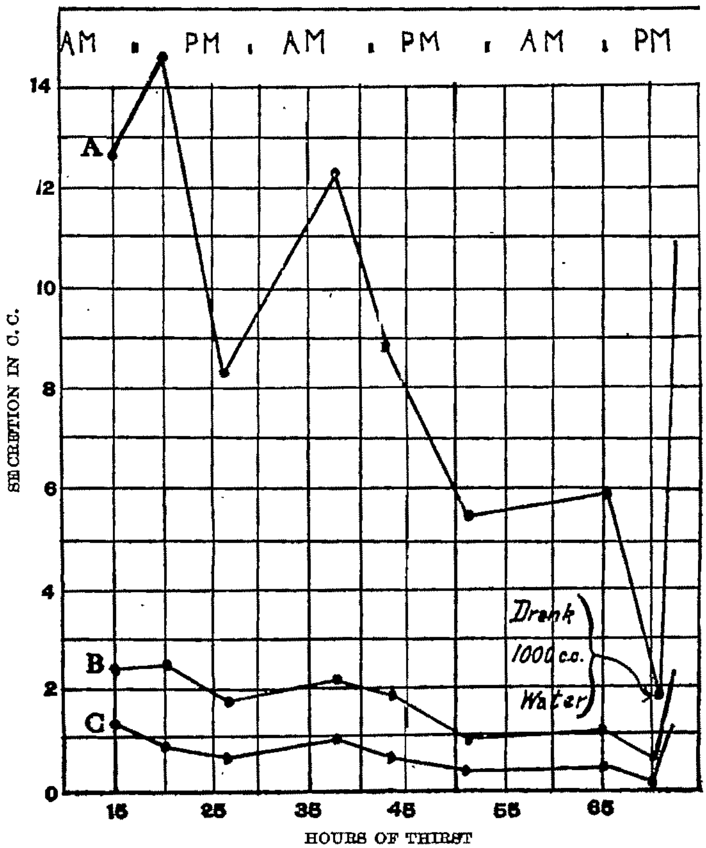

FIG. 1. CURVES SHOWING THE INFLUENCE OF DEHYDRATION ON PAROTID SECRETION FROM ONE GLAND OF AN ADULT SUBJECT

(A represents the volume secreted for successive periods when tasteless gum was chewed for 15 min.; B shows the amount for the same periods when *S* breathed through his mouth for 30 min.; and C represents the normal flow for 15 min.)

The viscosity of the secretion collected was determined with a viscosity tube after each test but no significant changes occurred. Although the small amount of secretion in the mouth on the last day appeared to be very viscous that collected from the disk showed no change. A serous gland, such as the

parotid, apparently continues to secrete its characteristic thin watery fluid even in thirst, while the mucous glands deliver a secretion which becomes more viscid with dehydration.

Throughout the investigation observations were made on the sensations of thirst. Since it is generally agreed that thirst is an experience referred to the mucous lining of the mouth and pharynx, and especially to the roof of the tongue and palate, we were interested to know what the effect would be if these regions were kept moist by constant chewing while the water balance of the organism was greatly reduced. It will be recalled that the *S* chewed gum constantly except when asleep or undergoing tests where chewing was not a part of the procedure. As long, therefore, as chewing evoked secretion the entire oral cavity would be kept moist. 'False thirst,' caused from a drying of the mucosa, was thus eliminated, except when the *S* was breathing through his mouth or when he first awoke. 'True thirst,' on the other hand, which represents an actual bodily need as a result of insufficient liquid in the organism, expressed itself after the second day in the form of a general craving for liquids. This craving was dominant over other desires, and seemed much like the craving for sweets soldiers experienced after long periods of deprivation at the front, or it might be similar to the craving men sometimes have for salt when living on a salt-free diet for an extended period. It did not seem to be assignable to any particular region of the organism as in 'false thirst.' The eating of toast became more or less of a task before the experiment was concluded so completely had other desires been submerged by that for liquids.

During some previous tests that were made in which gum was not chewed and the mouth was not kept moist, discomfort arose from the drying of the tongue and palate as soon as the water supply of the organism began to get low. It seems clear that when liquids are withheld from the body, the membranes around the base of the tongue are affected first and that dryness of these tissues is a normal stimulant for salivary secretion. This sensation of dryness is what is ordinarily thought of as thirst, but when this area is not allowed to become dry as in the case of the experiment under discussion, there is still craving for liquids which would provide protection against the body's becoming dangerously dry. 'False thirst' may or may not be a reliable index of dehydration. A few minutes of mouth breathing will cause one to want to drink although the organism may be saturated with fluid. 'True thirst,' on the other hand, is perhaps always an indication of a low water balance.

Since this study was primarily concerned with technique for estimating the normal processes of dehydration no effort was made at this time to determine the mental effect of a low water exchange. The *S*'s behavior certainly showed no ill effects and he reported that he felt unusually well and capable of sustained mental effort. A further study is being planned in which the effect of dehydration on various phases of behavior will be studied. At the present time it seems probable that at least some individuals reduce their efficiency and perhaps health by overloading their bodies with liquids.

Incidentally the influence of various stages of dehydration on psychogalvanic responses needs to be taken into consideration by those studying behavior with this technique. Obviously the conductivity of the organism would change as the water balance changed.

A Rapid Method of Dehydration

While the normal process of dehydration shown in Chart I proceeded over a period of several days, it is possible to speed up the process materially. It is a common observation that strenuous bodily activity or the subjection of the body to heat causes a rapid lowering of the water balance as indicated both by the consumption and excretion of water. Some idea of the rate at which this process takes place may be gained from an experiment with hot water. The *S* chewed the tasteless gum for 15 min. as in the previous experiment and then reclined in a tub of water the temperature of which remained between 96° and 100°F for one hour. Throughout this process he perspired profusely. At the end of the hour the same chewing activity evoked approximately 50% of the secretion that was obtained before the hot bath was taken. A state of dehydration was apparently reached in one hour by this method which had required from 24–48 hr. by simply stopping the intake of liquids. If a low water exchange is desired in the treatment of abnormal behavior it may be obtained in a short time by this procedure. The known value of hydrotherapy with certain types of mental disturbance may be at least partially due to a reduced water balance. Here again the ingestion of a pint of water restored the organism to its original state.

Aspects of glandular activity which need to be observed in determining the stage of dehydration such as diurnal variations, daily variations, differences in glands on opposite sides of the mouth, and the influence of rest periods on glandular functioning will be considered in a subsequent report.

Summary

The results of this study may be summarized as follows:

(1) The beneficial effect of dehydration in preventing epileptic seizures in some cases studied suggested the need for an investigation of the influence of a low water balance on mental behavior.

(2) A method for determining the state of liquid exchange through a quantitative analysis of parotid secretion under different exciting situations was presented, and the normal progress of dehydration when the intake of liquid was materially reduced over a period of 70 hrs. was charted.

(3) Sensations referred to the mucous lining of the mouth and pharynx called 'false thirst' were prevented during the experiment by the constant chewing of tasteless gum. 'True thirst,' however, expressed itself when the actual bodily need for liquid arose in the form of a dominant craving for liquids. 'True thirst' is apparently a much more reliable index of low water balance than 'false thirst.'

(4) A hot bath causing excessive perspiration was found to reduce the secretions approximately fifty percent in one hour. The water balance was quickly restored when liquid was ingested.

SOME EFFECTS OF HETEROGENEITY ON THE THEORY OF FACTORS

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The effects of heterogeneity in the experimental group upon the results of investigations designed to measure the extent and relative importance of mental abilities have been the subject of considerable controversy. Spearman suggests that such heterogeneity may give rise to spurious group factors which may tend to obscure the general factor.[1] Kelley questions this statement, and maintains that heterogeneity should spuriously increase the relative importance of the general factor.[2] He lists as the principal types of such heterogeneity differences in maturity, race, sex, and general nurture.

In the spring of 1929, the American Council Psychological Test, Series of 1928, was given to all applicants for admission to the University of Hawaii and the Territorial Normal and Training School. A considerable portion of this material was made available to the writers.[3] At the largest high school in Honolulu, the tests were given to all seniors, regardless of application or non-application, as a regular part of the guidance program. At all other schools, the testing was carried on directly by the University and Normal School, and this latter group of test papers, numbering 356, constitutes the experimental data of this paper.

Since the group tested consisted entirely of high school seniors, we may assume that there are no important differences in maturity; *i.e.* that, with reference to the test abilities, the subjects may all be considered adult, or practically so. We may also assume that one, and probably the most important single one, element in general nurture, namely, scholastic background—is constant. The two sexes are represented in about equal numbers. In regard to race, the heterogeneity is extreme. The group contains considerable numbers of Chinese, Haoles,[4] Hawaiians, Japanese, Koreans, and Portuguese, together with a sprinkling of other races, and a fairly large number of students of mixed ancestry.

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[1]C. Spearman, *The Abilities of Man*, 1927, 155-7. (Hereafter referred to as *Abilities*.)

[2]T. L. Kelley, *Crossroads in the Mind of Man*, 1928, 17-19. (Hereafter eferred to as *Crossroads*.)

[3]The writers are indebted to Dr. E. V. Sayers, Director of Education of the Territorial Normal and Training School, and Professor T. M. Livesay, Head of the Department of Education of the University of Hawaii, for permission to use this valuable material.

[4]North Europeans and American whites.

The test consists of five sub-tests. The first of these is a completion test. In each sentence, only one word has been omitted, and the number of letters in the omitted word is given, so that of the otherwise possible responses only one will be correct. There are 40 sentences. The second is an artificial language test. A vocabulary of 10 words, and 6 rules of grammar are given. Following are 20 sentences to be translated. Alternate sentences are in English and the artificial language. Every word correctly translated counts one point, provided the subject does not skip an entire sentence, in which case no further credit is given. There are 73 words altogether. The third is an analogies test of spatial relations. At the left of each row are three geometric figures, and at the right, five. The subject must pick from the five the one bearing the same relation to the third that the second does to the first. There are 28 items in this test. The fourth consists of 20 arithmetic reasoning problems. The last is an opposites test. On each line are four words, two of which are either synonyms or antonyms. The subject must pick these and indicate whether they are the same or the opposite.

All of these tests seem to satisfy Spearman's demand that they call for eduction rather than reproduction on the part of the subject; and all are sufficiently dissimilar so that any group factors found cannot be ascribed to overlap 'due to conspicuous resemblance between tests'.[5] They therefore would seem to be well suited for an investigation of the effects of heterogeneity upon the relative strengths of general, group, and specific factors.

In this study, an analysis has been made of the factors in the total group of 396, and in a sub-group of 87 boys of pure Japanese ancestry, in an attempt to determine the effects of race and sex heterogeneity. In order to carry out this investigation it was necessary to devise certain statistical tools.

Statistical Theory

The basic technique in the investigation of the relative importance of group factors is the tetrad, which may be defined by the equation,

$$t_{1234} = r_{12}r_{34} - r_{13}r_{24},$$

the first two subscripts of the t determining the first r, and the first and third subscripts, the third r, so that

$$t_{1243} = r_{12}r_{34} - r_{14}r_{23}, \text{ and}$$
$$t_{1342} = r_{13}r_{24} - r_{14}r_{23}.$$

The standard error of the tetrad as given by Kelley is

$$\begin{aligned}\sigma_{t_{1234}} = 1/\sqrt{N}\,[&r^2_{12} + r^2_{13} + r^2_{24} + r^2_{34} + 2r_{12}r_{14}r_{23}r_{34} \\ &+ 2r_{13}r_{14}r_{23}r_{24} - 2r_{12}r_{13}r_{23} - 2r_{12}r_{14}r_{24} \\ &- 2r_{13}r_{14}r_{34} - 2r_{23}r_{24}r_{34} \\ &+ t^2_{1234}(r^2_{12} + r^2_{13} + r^2_{14} + r^2_{23} + r^2_{24} + r^2_{34} - 4)]^{\frac{1}{2}} \ldots [1]\end{aligned}$$

As an approximation to the standard error of any random tetrad in a table of all the tetrads of a given system, the writers propose

$$\sigma_{t_4} = 2/\sqrt{N}\,[r'^2(1-r')^2 + s^2 - (\Sigma t^2_4/3_nC_4)(1 - 1.5R^2)]^{\frac{1}{2}} \ldots\ldots\ldots\ldots [2]$$

[5] *Abilities*, 150-153.

where t_4 is the random tetrad;[6]

N the total frequency;

n the number of variables;

3_nC_4 the number of tetrads;

r′ the average intercorrelation between variables;

R^2 the average of the squares of the intercorrelations; and

s^2 the mean square variation of the intercorrelations from their mean, so that $s^2 = R^2 - r'^2$.

Formula [2] was arrived at by substituting for each of the intercorrelations in [1] the average intercorrelation r′; for each of the squared intercorrelations, the mean square intercorrelation R^2; and for t^2_{1234}, the mean square tetrad.

The remainder of this section will be devoted to a consideration of methods for investigating the relative importance of specific factors. The basic function in this analysis will be designated as the triad.

As a preliminary, we shall note certain theorems.

Proposition I. Two variables may be thought of as due to one general factor and no specific factors when their correlation, corrected for attenuation, is equal to one. The errors of measurement always introduce chance specific factors whose magnitudes depend upon the reliabilities of the tests used rather than upon the mental make-up of the *Ss* and such errors should be allowed for by using correlations corrected for attenuation, except in formulas such as the tetrad, in which they cancel.

Proposition II. The variance of the specific factor in either of two correlated variables, expressed as a fraction of the total non-chance variance of such variable, lies between the values $1 - r_{\infty\omega}$ and $1 - r^2_{\infty\omega}$. By a specific factor, we mean any part of the variance contributed by factors other than those which are common to the two variables under consideration. Any factor is specific only with regard to a specified system. This proposition has been proved by R. C. Tryon.[7] Proposition I is a special case of II.

Proposition III. Three variables may be thought of as due to a single general factor and two specific factors when the product of two of the intercorrelations, corrected for attenuation, is equal to the third. We define

$$\alpha_1 = c_1\sigma_g/\sigma_\infty, \quad \alpha_2 = c_2\sigma_g/\sigma_\omega, \quad \alpha_3 = c_3\sigma_g/\sigma_\gamma$$

in the system

$$x_\infty = c_1 g, \quad x_\omega = c_2 g + s_2, \quad x_\gamma = c_3 g + s_3$$

where x_∞, x_ω and x_γ are the variables x_1, x_2 and x_3, corrected for attenuation;

g is the general factor;

s_2 and s_3 are factors specific to x_2 and x_3 respectively;

c_1, c_2, and c_3 are constant weights.

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[6] The single subscript 4 after t indicates a tetrad. The single subscript 3 after t will designate a triad. The triad will be defined later.

[7] R. C. Tryon, Interpretation of correlation coefficients, *Psychol. Rev.*, 36, 1929, 419-445.

In this system,

$$\alpha_1^2 = 1$$
$$\alpha_1\alpha_2 = r_{\infty\omega}$$
$$\alpha_1\alpha_3 = r_{\infty\gamma}$$
$$\alpha_2\alpha_3 = r_{\omega\gamma}$$

whence

$$r_{\infty\omega}r_{\infty\gamma} = r_{\omega\gamma}$$

and

$$t_{123} = r_{\infty\omega}r_{\infty\gamma}/r_{\omega\gamma} = 1 \ldots\ldots\ldots\ldots [3]$$

This equation is the triad. It is similar to Kelley's equation [14][8] except that all correlations are corrected for attenuation. The variable, x_∞, which in this system has no specific factor, appears twice in the numerator of the triad, but not in the denominator. If x_ω were the variable without any specific factor, the triad would become

$$t_{231} = r_{\infty\omega}r_{\omega\gamma}/r_{\infty\gamma} = 1$$

and if x_γ were the variable without a specific factor, it would be

$$t_{312} = r_{\infty\gamma}r_{\omega\gamma}/r_{\infty\omega} = 1.$$

As a corollary, we have:

Proposition IV. n *variables may be thought of as due to one general factor and* n-1 *specific factors when all the triads which involve one variable in the numerator twice are equal to one.* Kelley's Proposition 11 is a special case of the above.[9] His equations [29] will be shown to be equivalent to our triads.

Let r_{12}, r_{13} and r_{23} be intercorrelations between variables;

r_1, r_2 and r_3, reliabilities for the respective variables, obtained by correlating random halves;

R_1, R_2 and R_3, estimated reliabilities for the total variables, according to the equations

$$R_1 = 2r_1/(1 + r_1),\ R_2 = 2r_2/(1 + r_2),\ R_3 = 2r_3/(1 + r_3).$$

Then $$r_{\infty\omega} = r_{12}/\sqrt{R_1R_2},\ r_{\infty\gamma} = r_{13}/\sqrt{R_1R_3},\ r_{\omega\gamma} = r_{23}/\sqrt{R_2R_3}, \ldots\ldots [4]$$

and $$t_{123} = r_{\infty\omega}r_{\infty\gamma}/r_{\omega\gamma} = r_{12}r_{13}/r_{23}R_1 \ldots\ldots\ldots\ldots [5]$$

Also $$r_{\infty 2} = r_{12}/\sqrt{R_1},\ r_{\infty 3} = r_{13}/\sqrt{R_1},$$

and $$t_{123} = r_{\infty 2}r_{\infty 3}/r_{23} = r_{12}r_{13}/r_{23}R_1 \ldots\ldots\ldots\ldots [5a]$$

The second expression of equation [5a] is the first of Kelley's equations [29], and is seen to lead to a value identical with [5]. Similarly

$$t_{231} = r_{\infty\omega}r_{\omega\gamma}/r_{\infty\gamma} = r_{\omega 1}r_{\omega 3}/r_{13} = r_{12}r_{23}/r_{13}R_2,$$
$$t_{312} = r_{\infty\gamma}r_{\omega\gamma}/r_{\infty\omega} = r_{\gamma 1}r_{\gamma 2}/r_{12} = r_{13}r_{23}/r_{12}R_3.$$

Proposition V. If three variables may be thought of as due to one general factor and three specific factors, then the correlation between the general factor and any

[8] *Crossroads*, 40.
[9] *Crossroads*, 50-51.

one of the variables will be equal to the square root of the triad involving that variable twice in the numerator.

Let $x_\infty = c_1 g + s_1$

$x_\omega = c_2 g + s_2$

$x_\gamma = c_3 g + s_3.$

Then $r_{\infty\omega} = \alpha_1\alpha_2$

$r_{\infty\gamma} = \alpha_1\alpha_3$

$r_{\omega\gamma} = \alpha_2\alpha_3$

and $t_{123} = r_{\infty\omega} r_{\infty\gamma}/r_{\omega\gamma} = \alpha^2_1.$

Now $r_{\infty g} = r_{g(g+s_1)} = (c^2_1\Sigma g^2 + c_1\Sigma g s_1)/Nc_1\sigma_g\sigma_{(g+s_1)}$

but by definition r_{gs_1} is zero, so that

$$r_{\infty g} = c_1\sigma_g/\sigma_\infty = \alpha_1 = \sqrt{t_{123}} \quad \ldots\ldots\ldots\ldots\ldots\ldots [6]$$

Spearman, by a different line of proof, has arrived at a similar result,[10] except that he used correlations uncorrected for attenuation, so that his specific factors included the errors of measurement. Proposition III may be seen to be the special case of V in which the correlation between the variable and the general factor is equal to one.

From [6], $t_{123} = c^2_1\sigma^2_g/\sigma^2_\infty$, but since $x_\infty = c_1 g - s_1$, and s_1 is uncorrelated with g,

$$\sigma_\infty^2 = c^2_1\sigma^2_g + \sigma^2_{s_1}$$

$$c^2_1\sigma^2_g/\sigma^2_\infty + \sigma^2_{s_1}/\sigma^2_\infty = 1 \quad \ldots\ldots\ldots\ldots\ldots\ldots [7]$$

so that the variance of the variable x_∞ may be considered to be composed of two independent portions, one determined by g and one by s_1, the relative respective contributions being t_{123} and $1 - t_{123}$.

If group factors are present in addition to the general and specific factors, $1 - t_{123}$ should in most cases give a fair approximation to the relative magnitude of the specific factor in x_∞; so that if we have two bodies of data, we may compute all the possible triads for each system; the mean of all the triads in one system compared with the mean of all those in the other should then give some indication of the relative importance of specific factors in the two systems.

In general, before we can use the triad effectively, we need its standard error. The derivation is as follows:

Let x be measured by fallible scores x_I and x_{II} whose sum is x_1.

Then $r_{III} = r_1$ of our previous notation, and $R_1 = 2r_1/(1 + r_1)$, as before.

From [5], $t_{123} = r_{12}r_{13}/r_{23}R_1$.

Taking logarithmic differentials, squaring, summing and dividing by the population,

$$\sigma^2_{t_{123}}/t^2_{123} = \sigma^2_{r_{12}}/r^2_{12} + \sigma^2_{r_{13}}/r^2_{13} + \sigma^2_{r_{23}}/r^2_{23} + \sigma^2_{R_1}/R^2_1 - (\sigma_{R_1}\sigma_{r_{12}}r_{R_1r_{12}})/R_1r_{12}$$
$$-(\sigma_{R_1}\sigma_{r_{13}}r_{R_1r_{13}})/R_1r_{13} + (\sigma_{R_1}\sigma_{r_{23}}r_{R_1r_{23}})/R_1r_{23} + (\sigma_{r_{12}}\sigma_{r_{13}}r_{r_{12}r_{13}})/r_{12}r_{13}$$
$$-(\sigma_{r_{12}}\sigma_{r_{23}}r_{r_{12}r_{23}})/r_{12}r_{23} - (\sigma_{r_{13}}\sigma_{r_{23}}r_{r_{13}r_{23}})/r_{13}r_{23} \quad \ldots\ldots\ldots\ldots\ldots\ldots [8]$$

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[10] *Abilities*, Appendix, XVI.

Assuming normal distributions in the sampled population and a large sample, we may obtain $\sigma_{r_{12}}$, $\sigma_{r_{13}}$ and $\sigma_{r_{23}}$ by the usual formula, $\sigma_r = (1-r^2)/\sqrt{N}$. Under the same assumptions, Shen has shown[11] that $\sigma_R = 2(1-R)/\sqrt{N}$. These formulas enable us to evaluate the first four terms of [8]. The last three may be evaluated by Pearson and Filon's formula.[12] To evaluate the fifth and sixth terms, we require two similar product-moment coefficients. For the first of these, taking differentials, $dR_1 = 2dr_1/(1+r_1)^2$, $dr_{12} = dr_{12}$. Multiplying, summing and dividing by the population,

$$\sigma_{R_1}\sigma_{r_{12}}r_{R_1r_{12}} = 2\sigma_{r_1}\sigma_{r_{12}}r_{r_1r_{12}}/(1+r_1)^2.$$

Assuming that $\sigma_1 = \sigma_I$, $r_{1I} = r_{I1} = (1+r_1)^{\frac{1}{2}}/2^{\frac{1}{2}}$.

Assuming in addition that r_{12} and r_{I2} are equal, $r_{12} = r_{I2} = r_{12}\,(1+r_1)^{\frac{1}{2}}/2^{\frac{1}{2}}$.

Substituting these values in Pearson and Filon's formula,[13]

$$r_{r_1r_{12}} = r_{11}/2, \text{ so that}$$

$$\sigma_{R_1}\sigma_{r_{12}}r_{R_1r_{12}} = (r_{12}/N)\,(1-r^2_{12})\,(1-R_1)$$

and similarly,

$$\sigma_{R_1}\sigma_{r_{13}}r_{R_1r_{13}} = (r_{13}/N)\,(1-r^2_{13})\,(1-R_1).$$

To evaluate the seventh term of [8], we require one further product-moment coefficient. Proceeding as before,

$$dR_1 = 2dr_1/(1+r_1)^2,\ dr_{23} = dr_{23}, \text{ and}$$

$$\sigma_{R_1}\sigma_{r_{23}}r_{R_1r_{23}} = 2\sigma_{r_1}\sigma_{r_{23}}r_{r_1r_{23}}/(1+r_1)^2.$$

Assuming as before that $\sigma_1 = \sigma_I$,

$$r_{12} = r_{I2} = r_{12}\,(1+r_1)^{\frac{1}{2}}/2^{\frac{1}{2}},\ r_{13} = r_{I3} = r_{13}\,(1+r_1)^{\frac{1}{2}}/2^{\frac{1}{2}},$$

and from Pearson and Filon's formula,

$$\sigma_{r_1}\sigma_{r_{23}}r_{r_1r_{23}} = [(1 + r_1^2/2N)]\,[2r_{12}r_{13} - r_{23}(r^2_{12} + r^2_{13})].$$

Substituting in [8], we have for the standard error of the triad,

$$\begin{aligned}\sigma_{t123} = t_{123}/\sqrt{N}[&(1-r^2_{12})^2/r^2_{12} + (1-r^2_{13})^2/r^2_{13} + (1-r^2_{23})/r^2_{23}\\ &+ 4(1-r_1)^2/(2r_1)^2 - (1-r^2_{12})\,(1-r_1)/2r_1 - (1-r^2_{13})\,(1-r_1)/2r_1\\ &+ (1/r_{23})\,(1-r_1/2r_1)\,\{2r_{12}r_{13} - r_{23}(r^2_{12}+r^2_{13})\}\\ &+ (1/2r_{12}r_{13})\,\{(1-r^2_{12}-r^2_{13})\,(2r_{23}-r_{12}r_{13}) + r_{12}r_{13}r^2_{23}\}\\ &- (1/2r_{12}r_{23})\,\{(1-r^2_{12}-r^2_{23})\,(2r_{13}-r_{12}r_{23}) + r_{12}r^2_{13}r_{23}\}\\ &- (1/2r_{13}r_{23})\,\{(1-r^2_{13}-r^2_{23})\,(2r_{12}-r_{13}r_{23}) + r^2_{12}r_{13}r_{23}\}]^{\frac{1}{2}} \quad \ldots.[9]\end{aligned}$$

This is the full formula for the standard error of a single triad, under the assumptions above noted.

As an approximation to the standard error of any random triad in a table of all the triads of a given system, the writers propose

[11] E. Shen, The standard errors of certain estimated coefficients of correlation, *J. Educ. Psychol.*, 15, 1924, 462-465.
[12] T. L. Kelley, *Statistical Method*, 1923, 170, formula [129].
[13] T. L. Kelley, *ibid.*, formula [128].

$$\sigma_{t_3} = (T^2/N)^{\frac{1}{2}} [1.5r'^2 + 3/r'^2 + r' - 1/r' + 1/r_1'^2 - (3-r')/r'_1 - 3.5]^{\frac{1}{2}} \ldots [10]$$

where t_3 is a random triad

$T^2 = \Sigma t_3^2 / {}_{3n}C_3$
N is the total frequency;
n the number of variables;
${}_{3n}C_3$ the number of triads;
r' the average intercorrelation of the total scores;
r'_1 the average reliability of the half-scores.

Formula [10] was obtained by substituting for each intercorrelation in [9] the mean intercorrelation r'; for each reliability, the mean reliability coefficient r'_1; and for the triad, the root mean square triad $(T^2)^{\frac{1}{2}}$.

Experimental Evidence

The papers used in this study were originally corrected in terms of the total scores for each sub-test. The writers later had them re-scored so as to get comparable halves on each sub-test. In those few cases where the total score on the two halves of a sub-test did not agree with the original score, the paper was scored a third time. For all sub-tests except Artificial Language, the odd and even items were taken as comparable halves. For the latter, items 1, 4, 5, 8, 9, 12, 13, etc. were taken as one half, and 2, 3, 6, 7, 10, 11, etc., as the other; because the odd and even items on this test were respectively English to Artificial and Artificial to English.

The intercorrelations were computed between the total scores on the sub-tests, using a chart devised by the writers which gives an absolute check on all numerical operations. The same chart was used in computing the reliability coefficients. For the latter, the intraclass correlation coefficients were used.[14] Their values were then substituted in the Spearman-Brown formula to give the estimated reliabilities for the total sub-tests. All the intercorrelations were then corrected for attenuation by formula [4]. Tables I and IA give the basic data for the heterogeneous and homogeneous groups respectively.

The corrected intercorrelations are all noticeably less than unity, the largest in the two tables being the r_{18} of Table I, whose value is approximately 0.867 ± 0.015.[15] It is therefore evident that no two variables can be thought of as due to a single general factor plus errors of measurement.

The tetrads were computed for both groups and are given in Tables II and IIA.

The mean of all the tetrads in Table II is 0.0644, and of those in Table IIA, 0.0361. We assume that the standard error of the mean tetrad of a system is equal to $({}_{3n}C_4)^{-\frac{1}{2}}$ times the standard error of a random tetrad from that system. From formula [2], the standard errors of these random tetrads were estimated as 0.0262 and 0.0468 respectively, giving the values 0.0068 and 0.0121 as the standard errors of the corresponding mean tetrads.

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[14]E. E. Cureton and J. W. Dunlap, The basic measure of reliability. Accepted for publication by this Journal.

[15]E. E. Cureton and J. W. Dunlap, Spearman's correction for attenuation and its probable error, this Journal, 42, 1930, 235–245.

TABLE I

INTERCORRELATIONS AND RELIABILITIES FOR THE HETEROGENEOUS GROUPS*

(N = 396)

Tests		1	2	3	4	5
Completion	1	*.7417* *.8517*				
Art. Lang.	2	.4135 .4592	*.9086* *.9581*			
Analogies	3	.4643 .5359	.3910 .4268	*.7880* *.8814*		
Arithmetic	4	.4570 .6103	.3534 .4464	.4923 .6462	*.4908* *.6584*	
Opposites	5	.7454 .8658	.4295 .4724	.4716 .5391	.3901 .5159	*.7672* *.8683*

TABLE IA

INTERCORRELATIONS AND RELIABILITIES FOR THE HOMOGENEOUS GROUP*

(N = 87)

Tests		1	2	3	4	5
Completion	1	*.6512* *.7858*				
Art. Lang.	2	.3173 .3640	*.9291* *.9632*			
Analogies	3	.3119 .3608	.2639 .2831	*.8217* *.9021*		
Arithmetic	4	.2650 .3797	.2265 .2960	.4975 .6717	*.4369* *.6081*	
Opposites	5	.3446 .4298	.2907 .3281	.3387 .3950	.2792 .3966	*.6879* *.8151*

*The upper intercorrelation in each cell is uncorrected. The lower intercorrelation in each cell is corrected for attenuation. The upper reliability coefficient in each cell along the long diagonal is the intraclass correlation between comparable halves of the test. The lower reliability coefficient in each diagonal cell is the Spearman-Brown estimate of the reliability of the total test.

TABLE II

TETRADS FOR THE HETEROGENEOUS GROUP

(N = 396)

a	b	c	d	t_{abcd}	t_{abdc}	t_{acdb}
1	2	3	4	.0395	.0249	−.0146
1	2	3	5	−.0044	−.0965	−.0921
1	2	4	5	−.0350	−.1021	−.0671
1	3	4	5	−.0344	−.1859	−.1515
2	3	4	5	−.0142	−.0589	−.0447

TABLE IIA

TETRADS FOR THE HOMOGENEOUS GROUP

(N = 87)

a	b	c	d	t_{abcd}	t_{abdc}	t_{acdb}
1	2	3	4	.0873	.0885	.0012
1	2	3	5	.0168	.0166	−.0002
1	2	4	5	.0101	.0085	−.0016
1	3	4	5	−.0020	−.0843	−.0823
2	3	4	5	−.0030	−.0709	−.0679

As an empirical check on the accuracy of formula [2], the standard errors of the tetrad having the smallest absolute value (t_{1325}), the tetrad having the largest absolute value (t_{1354}), and the tetrad whose absolute value lay nearest to that of the mean (t_{1452}), in Table II, were computed by formula [1].

$$\sigma_{t_{1325}} = .0177$$
$$\sigma_{t_{1354}} = .0292$$
$$\sigma_{t_{1452}} = .0329$$

The mean of these three values is 0.0266, which differs from the value found by formula [2] by 0.0004. As a further check, the standard errors of the tetrad having the smallest absolute value (t_{1352}), the tetrad having the largest absolute value (t_{1243}), and the two tetrads whose absolute values lay nearest to the mean (t_{1235} and t_{2451}—two because neither lay very close to the value of the mean), in Table IIA, were also computed by formula [1].

$$\sigma_{t_{1352}} = .0440$$
$$\sigma_{t_{1243}} = .0548$$
$$\sigma_{t_{1235}} = .0469$$
$$\sigma_{t_{2451}} = .0560$$

The mean of these four values is 0.0504, differing from the value found by formula [2] by 0.0036.

The difference between the two mean tetrads is 0.0283. Assuming that the correlation between the two mean tetrads is zero, the standard error of this difference would be 0.0139. This last value is overestimated by the above assumption, as correlation is obviously present, since one group forms a part of the other. Hence, we may conclude with some confidence that homogeneity tends to decrease the relative magnitude of group factors in comparison to the general and specific factors.

It is of some interest to examine the nature of the group factors. Following Kelley,[14] we allocate each tetrad to the two pairs of variables in which it might operate as a positive bond. Tables III and IIIA show the allocation of tetrads.

The probable importance of a bond as an indicator of the existence of a group factor, may be judged roughly by the number of tetrads allocated to it in excess of the number to be expected by chance, and by the significance of the deviation of the median of these tetrads from zero. These facts have been noted in Tables III and IIIA. As a crude approximation to the standard error of a median tetrad, we may take $1.2533/\sqrt{6}$ times that of the random tetrad from the same table. For Table III, this value is 0.013, and for Table IIIA, 0.024.

In Table III, there appears to be a 1-5 bond and a 3-4 bond, with some indication of a 2-5 bond. In Table IIIA, we find evidence for a 3-4 bond again, and some indication of a 1-2 bond. Reference to the tests would indicate that the 1-5, 2-5 and 1-2 bonds could be explained by the assumption of a verbal factor. For the 3-4 bond, which appears strongly in both tables, we could

[14] *Crossroads*, 115-119.

also assume the verbal factor, operating negatively, since variables 3 and 4 are the only non-verbal ones, and every tetrad is allocated to both of the pairs of variables from which it might have arisen. It could also be explained directly by the assumption of a mathematical logic factor. A check on these alternative hypotheses can be obtained by an examination of the correlation coefficients corrected for attenuation. Aside from the r_{15} in Table I, r_{34} is the highest

TABLE III

BONDS FOR THE HETEROGENEOUS GROUPS
(N = 396)

Variables in pairs	12	13	14	15	23	24	25	34	35	45
	.040	.004	.015	.097	.015	.102	.004	.040	.034	
	.025		.035	.092	.097	.067	.035	.025	.014	
Tetrads			.034	.102	.092	.014	.059	.186		
allocated				.067			.045	.152		
				.186				.059		
				.152				.045		
Excess no. of + bonds				3			1	3		
Median tetrad if positive*				.100			.020	.052		

*$\sigma_{\text{med. } t_4}$ is of the order of .013

TABLE IIIA

BONDS FOR THE HOMOGENEOUS GROUP
(N = 87)

Variables in pairs	12	13	14	15	23	24	25	34	35	45
	.087	.001	.002	.000	.000	.001	.071	.087	.017	.010
Tetrads	.089			.002		.002	.068	.089	.017	.009
allocated	.017			.084		.003		.084	.002	
	.017			.082				.082	.003	
	.010							.071		
	.009							.068		
Excess no. of + bonds	3			1				3	1	
Median t_4 if + *	.017			.001				.083	.002	

*$\sigma_{\text{med. } t_4}$ is of the order of .024

intercorrelation in each table. Hence we may conclude that we have both a verbal factor and a mathematical logic factor in these tests. The evidence for a verbal factor is in line with that supplied by Kelley and with the more recent findings of Spearman.[17] The evidence for a mathematical logic factor, linking the geometrical analogies test with the arithmetic reasoning test, finds no support in the work of either Spearman or Kelley, and the former, in fact, specifically denies the existence of such a factor.[18]

[17] C. Spearman, Reply to T. L. Kelley, *J. Educ. Psychol.*, 20, 1929, 563.
[18] *Abilities*, 232.

The triads were computed for both groups, and are given in Tables IV and IVA.

The mean of all the triads in Table IV is 0.5731, and of those in Table IVA, 0.4175. We assume that the standard error of a mean triad is equal to $(3{}_nC_3)^{-\frac{1}{2}}$ times the standard error of the corresponding random triad. On application of formula [10], we find that the approximate standard error of the random triad in Table IV is 0.0840, and in Table IVA, 0.2020; the standard errors of the corresponding mean triads being 0.0513 and 0.0369.

TABLE IV

TRIADS FOR THE HETEROGENEOUS Group
(N = 396)

Variables a	b	c	t_{abc}	t_{bac}	t_{cab}
1	2	3	.5766	.3657	.4981
1	2	4	.6278	.3359	.5933
1	2	5	.8427	.2503	.8917
1	3	4	.5061	.5674	.7359
1	3	5	.8618	.3333	.8720
1	4	5	1.0254	.3632	.7327
2	3	4	.2948	.6178	.6759
2	3	5	.3740	.4871	.5967
2	4	5	.4088	.4875	.5459
3	4	5	.6753	.6184	.4304

TABLE IVA

TRIADS FOR THE HOMOGENEOUS Group
(N = 87)

Variables a	b	c	t_{abc}	t_{bac}	t_{cab}
1	2	3	.4755	.2787	.2876
1	2	4	.4668	.2838	.3088
1	2	5	.4768	.2779	.3874
1	3	4	.2090	.6542	.6897
1	3	5	.5976	.3399	.4591
1	4	5	.4114	.3504	.4489
2	3	4	.1248	.6424	.7023
2	3	5	.2352	.3408	.4578
2	4	5	.2449	.3578	.4397
3	4	5	.6690	.6744	.2332

As a check on the accuracy of formula [10], the standard errors of the triad having the smallest value, the triad having the largest value, and the triad whose value lay nearest the mean, were computed by formula [9] for each table. From Table IV we have

$$\sigma_{t_{215}} = .0397$$
$$\sigma_{t_{145}} = .1190$$
$$\sigma_{t_{123}} = .0898$$

The mean of these values, 0.0828, differs from the value given by formula [10], 0.0840, by 0.0012. From Table IVA we have

$$\sigma_{t_{234}} = .0814$$
$$\sigma_{t_{423}} = .3886$$
$$\sigma_{t_{145}} = .2274$$

The mean of these three values, 0.2325, differs from the value given by formula [10], 0.2020, by 0.0305.

The difference between the mean triads is 0.1556. Assuming that the correlation between the two mean triads is zero, the standard error of this difference is 0.0400. This value is overestimated by the above assumption, since correlation is evidently present. Hence we may conclude with considerable confidence that homogeneity increases the relative magnitude of special factors in comparison with those of the general and group factors.

As a first approximation to the proportion of the variance of any variable to be attributed to its specific factor, we may take the value,

$$_{s}\sigma_{x} = 1 - \tfrac{1}{6}\sum_{1}^{6}(\text{all tetrads having variable x twice in the numerator}).$$

This formula would give an exact value if all the tetrads in the system were equal to zero within their sampling errors. Table V gives the values of these proportions.

TABLE V

RELATIVE VARIANCES OF SPECIFIC FACTORS

	$_{s}\sigma^2_1$	$_{s}\sigma^2_2$	$_{s}\sigma^2_3$	$_{s}\sigma^2_4$	$_{s}\sigma^2_5$	$_{s}\sigma^2$
N = 396	.2599	.6617	.4702	.4210	.3218	.4269
N = 87	.5605	.7591	.5110	.4861	.5956	.5825
Gain	.3006	.0974	.0408	.0651	.2738	.1556

It may be seen that variable 2, Artificial Language, has the largest specific factor in both groups. In the heterogeneous group, variables 3 and 4, the mathematical tests, are next in order, while variables 5 and 1, the distinctively verbal tests, are last. In the homogeneous group, variables 5 and 1 come second and third, with 3 and 4 last. Reference to the last column of Table V shows that in general about half the non-chance variance in the two systems is due to specific factors.

The last row of Table V shows the relative increase in the specific factor for each test, due to homogeneity. The variables in order of their relative increases are 3 (Analogies), 4 (Arithmetic), 2 (Artificial Language), 5 (Opposites), and 1 (Completion). It is apparent that this is the same as the order of increasing saturation with the verbal factor. Heterogeneity, therefore, seems to operate in this case to a great extent by increasing the verbal factor. This conclusion is supported by the very high r_{15} of Table I and the radically lower r_{15} of Table IA. It agrees also with the common observation that different racial groups in Hawaii differ noticeably in ability to handle the English language. The results of this study would seem to lend some support to Kelley's penetrating observation that the general factor is in fact itself composed principally of verbal elements and heterogeneity.[19]

SUMMARY AND CONCLUSIONS

(1) The American Council Psychological Test is an excellent instrument for the investigation of the effects of heterogeneity on the relative importance of general, group, and specific factors. Data are presented for a heterogeneous group of 396 high school seniors in Hawaii, and for a sub-group of 87 boys of Japanese ancestry.

(2) An approximation to the standard error of a random tetrad is offered, based on Kelley's formula for the standard error of a single tetrad.

[19] *Crossroads*, 18.

(3) A method for investigating the relative importance of specific factors is presented, based on a new function called the triad, whose relation to certain functions used by Spearman and Kelley is discussed. The standard error of the triad is derived, and an approximation to the standard error of a random triad is proposed.

(4) Heterogeneity increases the relative importance of group factors.

(5) Evidence is presented of the existence of a verbal factor and of a mathematical logic factor.

(6) Heterogeneity decreases the relative importance of specific factors, and does so in proportion to the saturation of the variables with the verbal factor.

The last three conclusions apply strictly only to the data of this study. General conclusions can be arrived at only through the accumulation of corroborating studies.

THE RELATIVE IMMEDIACY OF SENSORY, PERCEPTUAL, AND AFFECTIVE CHARACTERISTICS

By KERMIT W. OBERLIN, Harvard University

In confuting the so-called production theory of Benussi and other members of the Würzburg school, Koffka has argued that the whole is neither less immediate than its parts nor less original. "Die typische Form der Verbindung Reiz—Erlebnis ist nicht mehr die Empfindung (psychophysische Definition des Begriffs). So wie die Gestalten descriptiv nicht weniger unmittelbar sind als ihre Teile, so sind sie, funktionell, auch nicht weniger ursprünglich."[1]

Introspective technique is not adequate to a direct test of this proposition. The experiments of Külpe[2] and of Yokoyama[3] have shown that, as far as report is concerned, determination acts selectively upon the various attributes of sensation. The immediacy of one attribute is attained only at the cost of making the others more remote. Thus the relative immediacy of wholes and parts—of *Gestalten* and *Empfindungen*—reduces experimentally to relative immediacy of their characteristics, and this problem is the present subject of investigation. The experiment[4] was divided into two parts. The first part was designed to determine the relative adequacy of sensory, perceptual, and affective post-determination, *i.e.* of post-determination for any of the so-called attributes of sensation and for other characteristics of experience, like shape or affective value. The second part was designed to investigate the relative readiness with which reports upon these characteristics occur in the absence of any specific instructional determination.

The sensory attributes used were *hue*, *brilliance*, *saturation*, and *extension*; the others were *shape* and *affectivity*.

The materials used were pairs of bits of colored papers, approximately 1 in. square and about 1 in. apart, each pair mounted on a piece of white cardboard about 6 in. square. The *O* sat in a comfortable chair about 4 ft. in front of an exposure apparatus and peered through an aperture wide enough for full exposure of each of the two members of the pair. The exposure was made by means of an electric shutter which rested between the aperture and the colors

*Accepted for publication February 21, 1930.

[1]K. Koffka, Beiträge zur Psychologie der Gestalt- und Bewegungserlebnisse, *Zsch. f. Psychol.*, 73, 1915, 57. See also Carl Rahn, The relation of sensation to other categories in contemporary psychology, *Psychol. Monog.*, 16, 1913, (no. 67).

[2]O. Külpe, Versuche über Abstraktion, *Ber. u. d. I. Kongr. für exper. Psychol.*, 1904, 56-68.

[3]For Yokoyama's experiment see E. G. Boring's note in this JOURNAL, 35, 1924, 301-304.

[4]This experiment was carried out during the year 1928-29 under the direction of Dr. J. G. Beebe-Center in the Harvard Psychological Laboratory. I also wish to thank Prof. Boring for the help he has given me in preparing his paper.

and exposed the latter to view for one second. There was an interval of 11 sec. between exposures. There were 233 pairs of colors and these were presented one after the other to each of the *Os*, with four 3-min. rest periods during every experimental hour.

In the first part of the experiment the following *Os* were used: M. E. Carver (*Ca*), A. J. Harris (*Ha*), W. D. Turner (*Tu*), and Miss D. Selling (*Se*), all of whom were research students in the Harvard Laboratory.

The *O* was given the task of comparing the brilliances of the two colors and was to report whether the left or the right color was the more brilliant of the two. At various times during this procedure he was asked, after he had given his judgment on brilliance, one of the following questions: "What were the two hues?"—"Which was the more saturated?"—"Which was the more pleasant?"—"What were the shapes?"—"Which was the larger?"

These secondary questions were chosen in haphazard order so as not to establish a definite secondary determination in *O*. On the average there were intervals of about 40 judgments of brilliance with 3 or 4 secondary questions put before a repetition of any specific question.

The *O* was also asked to tell the process and basis of his judgment in answer to the secondary questions, *i.e.* to say whether he reasoned out the answer or not, or whether his judgment was made by the mediation of an image or taken directly from the experience.

For some short trial series the data showed that the *Os* were set for the discrimination of brilliances, for the judgments were quite accurate and consistent for each *O* upon repetition of the same stimuli. This trial series was given without the secondary questions. In practically every case the introspections showed the presence of a surrogate, which resembled a memory image and lasted several seconds. Hue, as well as brilliance, was present in this surrogate. Subjective assurance of the adequacy of the surrogate was greatest with respect to hue and next greatest with respect to brilliance, a result which accords with the relative accuracies of the reports on the stimulus. The general form of the figures was present in the surrogate, but the contours of the figures were not clear. Apparently the background in the surrogate was of no quality. The figure was very distinct, but the background was insignificant.

In the formal experimental series where the secondary questions were asked, the introspective reports showed the presence of surrogates in almost all cases. It is safe to say that these surrogates were the basis for every kind of secondary judgment. Furthermore, the reports indicated a strong tendency toward a cumulative predisposition, for, upon repetition of the secondary questions the *O* came finally to be predisposed not only toward brilliance but also to many of the other attributes that were required. Yokoyama has told us nothing about this in his experiment. It would be interesting to know whether his *Os* were more easily able to give a report on his secondary questions at the end of his experiment than at the beginning.

In this case where the *O* became predetermined in the direction of the secondary questions as well as the primary, the surrogates sometimes did not appear. When some judgment was unusually difficult and took longer than

usual, however, then the surrogate always appeared and enabled *O* to make any other judgments that were required.

The judgments of affection came by means of surrogates. In one case they were reported as "slow." In two other cases they were definitely associative judgments. Apparently affective judgments are more mediate than the others.

In the case of *Ca* the judgments came so quickly and readily that it was impossible for him to tell anything about the process of making the judgment. The judgments 'just seemed to come' and were without conscious antecedents. Toward the end of the experiment, however, he became aware of taking 'the stimulus as a *Gestalt*' and of reacting to the whole configuration with the two colors as figures upon a ground. No surrogates were involved in making the judgments upon the brilliance. The judgments upon saturation involved surrogates about fifty percent of the time and the other judgments were made practically always from a surrogate.

Ha was able to tell more about the process of making the judgments. Some of his judgments seemed to come by way of a process of reasoning. For example, here is part of one of his introspective reports: "The judgment process came like this: Right was rich but a tint. Left was rather poorly saturated but on a median level. Then made up my mind that the tint was more saturated than the other, *i.e.* taking into consideration the brilliance component in judging saturation." *Ha*'s affective judgments were made associatively and always from a surrogate. After one affective judgment he remarked: "Not a real affection but a sort of cognitive thing, *i.e.* a recognition of colors of things I usually don't like." Another time he said, "The affection is a very mild sort of thing, not real. *Unpleasant* seems to be merely that I prefer other colors to it." Surrogates seemed to be present in all of *Ha*'s judgments with the exception of the judgment of brilliance. He also had many doubtful judgments and judgments of *equal*, where he could not make a positive decision toward either of the two members of the pair, in respect to brilliance, the primary question. He was, however, very certain of his accuracy in the judgments that he made.

Tu reported that surrogates were present in every case and all the secondary judgments were made from them. He stated that "affection does not come unless asked for" and that then "it builds up very slowly;" in some cases it was "just a slight preference.' He, as well as *Ca*, could tell nothing about the process of making the judgment.

Se could not give any judgments on *size* or *shape* because of her habit of squinting her eyes in order to eliminate the contours and to enable herself more easily to make a judgment on brilliance and saturation. Her method of making the judgments was quite different from the methods of the other *O*s. For example, she said that, when the two colors were seen, she projected two imagined parallel scales, one for brilliance and one for saturation, that she then localized the two colors on these two scales as to brilliance and saturation. She used a fairly detailed associative method of judging in the case of affection, a method which involved a judgment of hue and other judgments of associative intermediaries before the judgment of affection. Where there was a marked difference her judgments of affection came quickly and the consciousness of the

hues was present apparently as soon as the exposure was made. Where the difference between hues was small, however, the affective judgment was made from a surrogate. She said: "I am always looking for brilliance and not conscious of looking for anything else until asked for it. After being asked, sometimes I can answer right away but am not conscious of knowing that before. If the answer does not come at once, then I try to look back or recall a surrogate."

There was a predominance of images or some other kind of surrogate for the judgment in nearly every case. In respect to the resemblance of the surrogate to the stimulus, the surrogate was usually described as a memory image with a great variability in vividness. In the few cases where the judgment came immediately, there were, of course, no surrogates to describe.

The first part of Table II summarises the occurrence of surrogates in this experiment.

In the second part of the experiment the procedure was somewhat changed. Only about one third of the total number of stimuli were used. Here, there were four new *Os*: D. W. Chapman (*Ch*), P. E. Huston (*Hu*), Hyung Kim (*Ki*) and Johann Twersky (*Tw*). The instructions to the *Os* in this part of the experiment were as follows: "You will be presented with two stimuli simultaneously, for one second. You are to tell me the difference between the two—*any* difference. Try to be passive in this experiment and do not look for any particular difference. Report as quickly as possible. In the introspection, report on the basis of the judgment and the process of the judgment."

As soon as possible, after each judgment of a difference was given, the *Os* were asked to give their introspections as instructed. They were not allowed to ask any questions but were expected to report with no further instructions.

The introspective reports of *Ki* and *Tw*, foreigners, were written out by *E*, but the reports of *Ch* and *Hu* were dictated into a dictaphone.

The problem here was to find what differences would be experienced when the *Os* were not predetermined toward any particular difference. Would perceptual and affective characteristics ever be reported when not asked for? Would all of the sensory attributes be noticed?

Under these instructions *E* expected to find that those properties of the stimuli in which they differed the most would be reported most often, and that the largest difference would be noticed most quickly and most easily judged. Because the instructions did not limit the *Os* much, large individual differences in the reports were expected, for the *Os* would bring to this experiment their own peculiar predeterminations toward colors.

The data from *Ch* indicated that in order to experience affection this *O* required a special affective determination. He gave no judgments of differences in affection until the first third of the material had been presented. Then in his introspective report we find: "It occurred to me at this point for the first time that perhaps I could find differences in P and U between the stimuli—as far as I can recall none of them has appealed to me as P or as U—perhaps that is determined by the set." For the remaining stimuli, however, he gave judgments of difference in affection in more than forty percent of the cases. *Ch*

seemed to be the keenest observer and found more differences than any of the other *Os*. Further on in his introspections he said: "I find that in general the significance of the difference is immediate, but it is impossible in many cases to state immediately what the difference is." For him, *size* and *position* were immediate and easily statable, but for differences in hue, saturation or brilliance, he had recourse to the color pyramid in imaginal terms. He also said: "I think that after a number of these observations, a very few of them in fact, I acquire, without actually willing to do so, a determining tendency to report differences in hue, and it probably influences any judgment and certainly influences me to look for hue differences wherever I expect tham to exist." These reports also revealed a searching, on the part of *Ch*, of the surrogates for any differences that he could think of and that did not come immediately. On the whole, we can say that, even with such a large exposure-time, *Ch* based a very high percentage of his total judgments on the surrogates.

The introspections of *Hu* were not so complete as those of *Ch*. *Hu* noticed, however, the differences in affection from the beginning. In general, his affective judgments seemed to be more difficult for him to make, and he usually reported them as coming last in a temporal series. His reports indicated that he always judged the affectivity of the colors associatively. He even reported some physiological accompaniments of the affection, and they may possibly be considered as some part of the surrogate for this experience. For example, here is a report: "Affection came a little later and was accompanied by a feeling of irritation and strain in the abdominal region because of the slight difficulty in making the judgments." Then again: "Both were extremely P and delicate and aroused a warm agreeable feeling." He resorted to the use of the color pyramid in imagery a great part of the time and explained it thus: "When the differences are large, the judgments are made easily from the stimulus itself; but, when the judgments are difficult to make, then reference must be had to the color figure to get an accurate discrimination."

Ki reported differences in affectivity for almost all of the stimuli. He experienced very few differences for each stimulus as compared to the two preceding *Os*. Here was a case in which there was no accumulation of various determinations. *Hue*, *size*, and *affection* were the attributes which were by far most often noticed. In many of the cases the affective judgments were made associatively. For *Ki* surrogates were present throughout the experiment.

For *Tw* the protocols showed one judgment each of brilliance and vividness throughout the experiment; all other judgments were for size and affection only. *Tw* said that his judgments were intuitive and subconscious and that he could not, therefore, give any information about the process of the judgment. The judgments came easily. Nearly always imagery was present and the judgment on one member of the pair of colors was taken from the surrogate, while the judgment on the other member was taken directly from the stimulus. He said that the judgments based directly on the stimulus came more easily than those derived from the surrogate. He also said that if he was more attentive he could judge on the stimulus, and that otherwise it was necessary for him to reconstruct a surrogate for a basis of judgment.

Table I shows which *Os* reported spontaneously various ones of fourteen sensory, perceptual or affective characteristics.

When descriptions of the surrogate, following the report on the secondary questions, were required, the *Os* gave, essentially, the following report: The thing that persists is a hue with a certain brilliance; other things may persist but only if they are striking. The surrogate as a whole begins, immediately after the presentation, to narrow down to those things that are striking, and,

TABLE I

THE DIFFERENCES REPORTED BY THE DIFFERENT *Os*

O	Hue	Brilliance	Saturation	Affection	Size	Shape	Texture	Position	Spottedness	Glare	Delicateness	Height	Width	Wrinkledness
Ch	X	X	X	X	X			X				X	X	X
Hu	X	X	X	X	X		X			X	X			X
Ki	X	X	X	X	X	X		X	X					
Tw				X	X									

when the secondary question corresponds to one of these characteristics, subjective assurance is high; but when the secondary question does not correspond to a striking characteristic, report becomes impossible.

The second half of Table II summarizes the surrogative mechanism in these spontaneous characterizations.

CONCLUSIONS

There are great individual differences in the *Os* as to what they can experience in color stimuli without predetermination in the form of instructions. The differences in the attributes of hue, saturation, and brilliance, or at least the finding of these differences, depends upon the familiarity of the *Os* with these attributes before coming to the experiment.

On the basis of the data gathered from both parts of this experiment, we can conclude that there is no non-temporal difference between sensation and perception so far as immediacy is concerned. What the data show is that there is no difference in the immediacy of sensory characteristics as against perceptual characteristics. Because affection has proved, however, to come associatively in so many cases, and so many times with an elaborate train of surrogates preceding the judgment, it must be concluded that the affective judgment is more mediate than the other judgments.

TABLE II

ANALYSIS OF SURROGATION FOR ALL *Os* IN BOTH PARTS OF THE EXPERIMENTS AND FOR THE SEVEN MOST IMPORTANT CHARACTERISTICS

	O	Brilliance	Hue	Saturation	Size	Shape	Affection	Position
Part I	*Ca*	No surrogate	Surrogate in all cases	Surrogate in half the cases	Surrogate in all cases	Surrogate in all cases	Surrogate in all cases	
	Ha	No surrogate	← Surrogates →				Associative judgment	
	Tu	No surrogate	← Surrogates →				slow	
	Se	No surrogate	Often immediate; surrogate when delayed		No data	No data	Surrogate; associative judgment	
Part II	*Ch*	Mediate via color-pyramid			Immediate and easy		After self-determination in 40% of cases	Immediate and easy
		← Further search of surrogates for differences →						
	Hu	Mediate via color-pyramid for difficult judgments			Size and texture frequent. Often immediate	Shape not reported	Delayed via surrogates	
	Ki	Brilliance not reported		Saturation not reported			Associative judgment	
			← Always a surrogate with few properties at a time →					
	Tw	← No judgments →			Surrogate; easy	No judgment	Surrogate; easy	

THE EFFECTS OF PRACTICE ON FATIGUE

By J. A. Glaze, Texas Christian University

Several months after my first experiment on fasting,[1] it occurred to me that the long period of practice preceding the fasts might yield interesting results independently of the fast itself. One of the tests consisted of writing the letters *ab* as fast as possible for twenty minutes. Accordingly I re-graphed the practice-periods, comparing the first five-days' work-curve with the curve for the second five days, and both with the remainder of the practice-period. I also

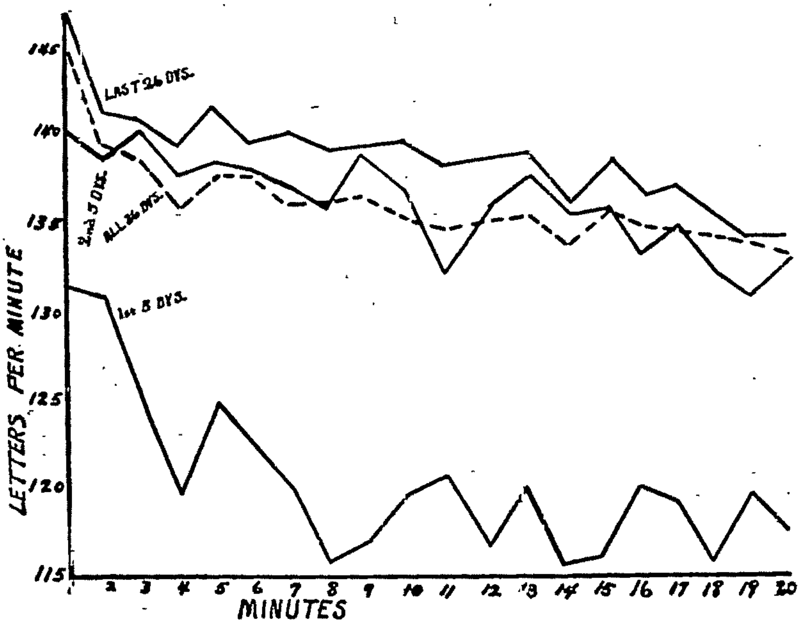

Fig. 1. Curves of Work for A

Based on writing *ab* 20 minutes daily for 36 continuous days

compared the first seven and the first ten days of work with the second seven and the second ten days, and both of these with the remainder of the practice-period. I wished to see whether appreciable differences occurred. The first comparison (the two five-day periods of practice) yielded the most conspicuous results. These I present here. Only that part of the practice-period up to the time of the fasts is considered. The longest of these practice-periods was thirty-six days and the shortest twenty-three. These periods represented a

* Accepted for publication June 17, 1930.

[1] J. A. Glaze, Psychological effects of fasting, this Journal, 40, 1928, 236-253.

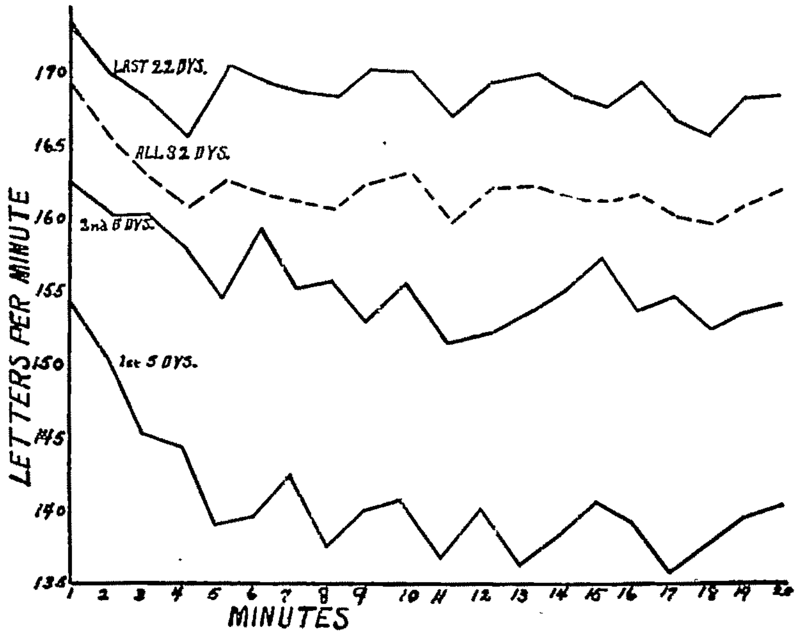

FIG. 2: CURVES OF WORK FOR B
Based on writing *ab* 20 minutes daily for 32 continuous days

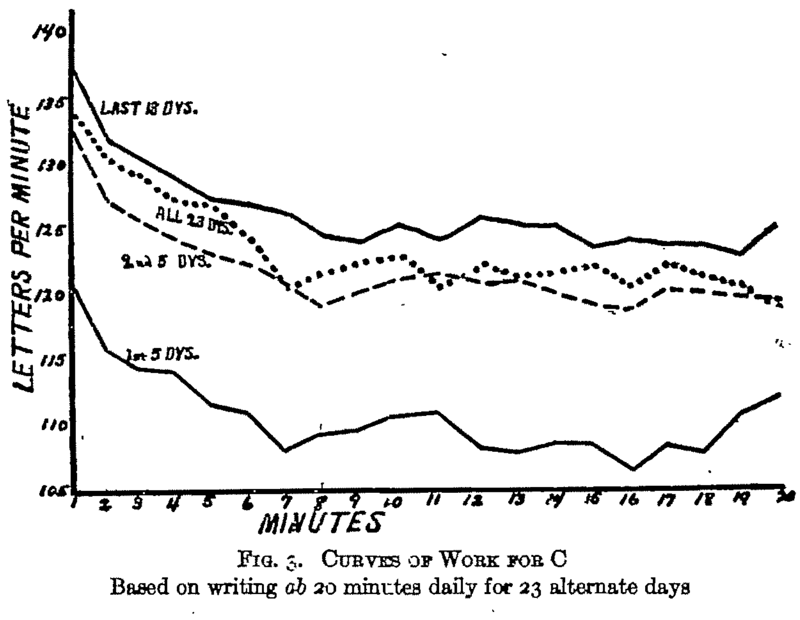

FIG. 3. CURVES OF WORK FOR C
Based on writing *ab* 20 minutes daily for 23 alternate days

fairly long and consistent task, averaging a month of actual work for each subject. A and B practiced every day for 36 and 32 days, respectively, while C practiced about every other day, his total being 23 days.

The results appear in the three accompanying graphs. In each instance the curve of work for the first five days drops rapidly during the first few minutes of work. During the second five days of practice the curve continues to drop in the early minutes of practice, but the drop is not so pronounced. The most conspicuous difference is the work-curve of A. The second five days of work tell a different story from that of the earlier period.

Dodge has contended[2] that relative fatigue is not a limitation of human efficiency, not exhaustion of the individual, but rather a preventive of exhaustion. We might add that the effects of repeated performance of such a task as ours tend to mask the manifestations of fatigue. Adaptation and the attainment of a fairly high degree of efficiency proceed regularly, eliminating the result that is manifest in the early practice-period.

Thus it appears that work-curves for the entire practice-period fail to tell anything like the whole story. Other writers have incidentally mentioned this fact; but no one, to my knowledge, has adequately illustrated it.

[2] R. Dodge, The laws of relative fatigue, *Psychol. Rev.*, 24, 1917, 89-113.

APPARATUS

A RELAY SYSTEM FOR TIMING OR COUNTING[1]

By John O. Kraehenbuehl and Max A. Faucett, University of Illinois

It is often necessary to obtain the accurate count or timing of an operation when the source of energy for operating the recording device is very small. The apparatus herein described was used in a study of the influence of personal habits and the changing of test-switches on the accuracy of the meter-tester, where it was necessary to time a definite number of revolutions made by the disk of a watt-hour meter. In this case a recording device attached to the meter would introduce a degree of error of such a magnitude as to make the results worthless; and only by using a beam of light was it possible to record the time without disturbing the energy-balance of the system measured. A beam of light was permitted to pass through a hole in the meter onto the sensitive photoelectric cell, and the electric impulses from the cell, properly amplified and relayed, were used to start and stop a cycle-counter.

This assembled amplifier and relay circuit may be used in a variety of problems, such as studies of reaction-time or of other performances where the interval of time to be measured may vary from a few sigma to any number of minutes. As a matter of fact, there is no kind of interval or series of impulses that cannot be so recorded. It thus recommends itself for use in all types of tapping tests and in every form of pursuit-meter where account has to be taken of both the total period and the number of breaks in the contact.

The complete system consists of the following units arranged in order; the exciting unit, the amplifier, the master relay, the holding circuit, and the isolating relay. Each unit may be used separately or as a part of the whole device. This flexibility of arrangement does away with numerous pieces of idle equipment.

A. The Photoelectric Cell. The beam of light may be projected on the cell-surface either by means of reflected rays from a small mirror in the path of an arc-lamp or by direct rays. In either case, the surface of the cell should be illuminated to twenty-five foot-candles when using a single-stage amplifier.

B. Amplifier. The amplifier may consist of one or more stages of resistance coupling, using a bias-battery instead of a condenser between stages. When the impulse is from direct current, the ordinary amplifier will not function satisfactorily. The single stage will prove satisfactory in most cases and requires few batteries. It is well to keep in mind the fact that any source of current in the order of five or ten micro-amperes may be used as the exciting source in place of the photoelectric cell.

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[1]The authors wish to acknowledge suggestions regarding applications to psychology from Dr. C. R. Griffith, Director of Research in Athletics and Associate Professor of Educational Psychology.

Quite frequently measurements are rendered impossible by the fact that the current is below the magnitude necessary to operate very sensitive instruments. By disconnecting the amplifier-links it is possible to measure the output in the plate circuit with a milli-ammeter. The amplifier so isolated may be used as a separate piece of apparatus and at the same time by a simple connection be made available for the relay-system.

C. Master Relay. This relay may be of the sensitive polarized type or merely a simple sensitive relay. Either of these will complete a local circuit which will operate any auxiliary apparatus desired. By means of this relay the impulses may be counted on an electrical counter or, if a timing device is placed in the relay output, it is possible to determine the period of time elapsing between single impulses. The modern electric clock and the normal lighting system prove very satisfactory for timing if the interval of time is several minutes; but when a high degree of accuracy is required, a synchronous timer recording ½-cycles may be used. It may be found desirable to isolate the master relay from the alternating current, in which case the output of the master relay may be passed directly to the isolating relay.

D. Holding Relay. While the master relay was satisfactory for timing between impulses, it was found necessary to introduce another device when the

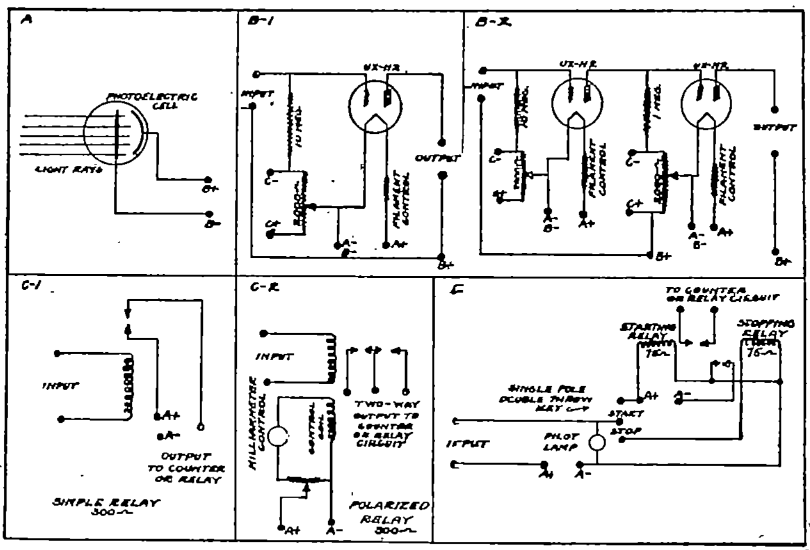

FIG. 1. ESSENTIAL UNITS FOR RELAY AND AMPLIFIER SYSTEM
(A) Photoelectric Cell, using the B-battery specified by the manufacturer. (B) Resistance Amplifiers, using A- and B-batteries to obtain the proper voltages specified for the tube and in each case using 22½-volt C-batteries. (C) Master Relays should be limited to one ampere on the contacts; in the polarized relay use a 1½-volt adjusting-battery. (D) Holding Relay. There should be a difference of 2 volts between the two A-batteries.

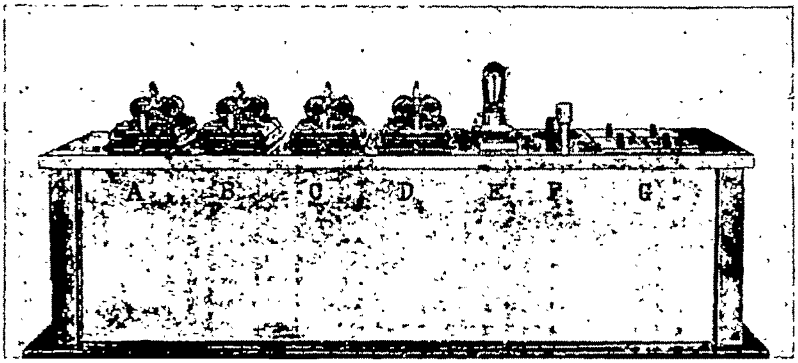

FIG. 3. ASSEMBLED FOUR-UNIT RELAY
(A) Isolating Relay, (B) and (C) Holding Relay, (D) Master Relay, (E) UX-112 Tube and Grid Resistance, (F) Front—Pilot-Light, Back—Bias-Battery Control Knob, (G) Switches for Battery Circuits.

timing period was to include several of the impulses. The device consists of two 75-ohm relays so connected that one relay is depolarized by means of a reversed battery. To start the operation, the switch is closed after a flash of the pilot-light and then released, and the flashes are counted which represent the impulses being transmitted until the desired number have appeared. The switch is then reversed and the next flash will open the circuit disconnecting the timing device. This presupposes that the interval of time between individual impulses is long enough for the operator to throw the switch. It must be borne in mind that the operator does not start and stop the cycle-counter or timing device. He merely sets the switch at the proper time and the impulse starts and stops the timing.

E. Isolating Relay. This is a 75-ohm relay of a rather insensitive type as it always functions on a local circuit of batteries.

Fig. 1 is a set of diagrams showing the various units described and their connections as a unit. Fig. 2 shows the connected relay-system in its simplest

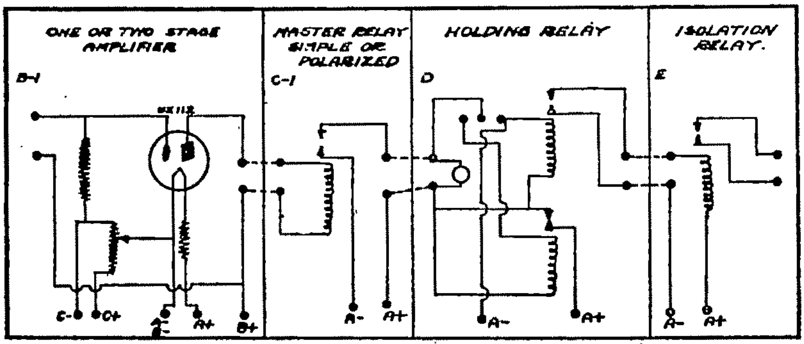

FIG. 2. FOUR-UNIT RELAY

Schematic diagram and the connecting links between the various units. Use A-, B- and C-batteries as specified in Fig. 1:

possible arrangement. Fig. 3 shows the assembled four-unit relay as laid out in Fig. 2. The compartment upon which the units are mounted contains the batteries.

The mechanical lag of the apparatus is constant. When investigated on an oscillograph with a 1000-cycle timing wave, it was found that the difference of time between the impressed impulse on the amplifier and the delivered impulse of the isolating relay was .002 sec. This is a constant error.

The flexibility of the device with its sturdiness and reliability of operation commends it for all investigations that require counting or timing. When used with the photoelectric cell, measurements may be made which in no way disturb the energy-balance of the device under observation. The cell will operate on white or colored light.

APPARATUS FOR STUDYING EYELID RESPONSES

By L. W. Allison, Peabody College[1]

The photographic technique of studying eye-movements, while offering certain advantages of refinement, has also practical disadvantages. The initial cost of the equipment is fairly high, as is also the operating cost when the nature of the work requires a large number of records. It seemed worth while, therefore, to devise a piece of apparatus less expensive but satisfactory for most purposes. The apparatus described grew out of a series of researches on various phases of eyelid-responses which are serially stimulated at intervals ranging from ½ to 4 sec.

A. Stimulus Unit. Fig. 4 shows the simple mechanical device for giving and recording the onset of the stimulus. The upright pieces A and A′ are 6 in. high and serve as supports for the two wooden rollers B and B′, around which belt C passes. This belt is 1¾ in. wide and 30 in. long and is made of oilcloth (smooth side up), used because the holes cut in it do not wear larger or ravel out at the edges. Small openings 1/16 by 3/4 in. are cut through the belt, allowing a metal finger D to come into contact with a brass plate E beneath the belt and on top of bracket F, thus closing the circuit. D is made of a piece of spring steel 5 in. long by 1/8 in. wide attached to bracket G. It has sufficient elasticity to allow the belt to move freely and enough rigidity to insure contact with plate E through the openings in the belt. H is the positive connection and I the negative. By means of an electric marker in the circuit, the onset of the stimulus is recorded on a kymographic record.

The power for driving this unit is furnished by the constant-speed motor of the Renshaw polygraph. Any good constant-speed motor would serve. The motor is adjusted to move the belt one inch per second. The time between stimuli is thus determined by the distance on the belt between the openings. But time-scales were recorded on all records with a 50 d.v. electrically-driven fork. A ½-sec. interval is thus obtained by allowing ½ in. between the openings in the belt; a 1-sec. interval by 1 in., etc. In order to determine the constancy of the speed of the motor (or the general reliability of the time-record), fifty counts of the ½-sec. interval against the time-scale were made at random from ten records. The mean count was found to be 25 (500σ). On no count was the deviation found to be more than 1/50 sec.

B. Responding Unit. Fig. 5 shows the mechanical device for recording the eyelid-response. It consists of a pair of spectacle-frames made of heavy wire ⅛-in. in diameter, which could be bent to conform to the shape and size of the head. To the left frame was attached a small cuff (A in the figure) which held in place a piece of rubber (B) .4 x .4 x .6 in. Through the center of this rubber a piece of german-silver wire (C), No. 16, runs parallel to the left frame of the spectacle. D is an adjustable fulcrum on which a lever (E) is perfectly balanced, this lever being also made of the same german-silver wire. Since this lever is balanced when at rest on fulcrum D, the slightest movement at F will bring the distant end of the lever into contact with C and thus close

[1] The writer is indebted to Dr. Joseph Peterson for valuable assistance in the preparation of this manuscript.

the circuit. By means of an electric marker in the circuit, the instant of the response is recorded on the same kymograph-record as that on which the onset of the stimulus is recorded. The fulcrum D can be slipped forward or backward on the base G. Base G is soldered to the frame at N and is loose at the other end (R), except for a small ring which fits around the frame and the base which holds fulcrum D in position after it has been properly adjusted. This adjustment is essential, due to the wide variation in size and shape of the heads of the various subjects. E is a small piece of adhesive plaster 1/12 x 1/3 in., by means of which the lever is attached to the subject's left eyelid just above the free margin. This lever is so perfectly balanced that, after a few minutes of adaptation, the subject is unaware of its movement with the eyelid.

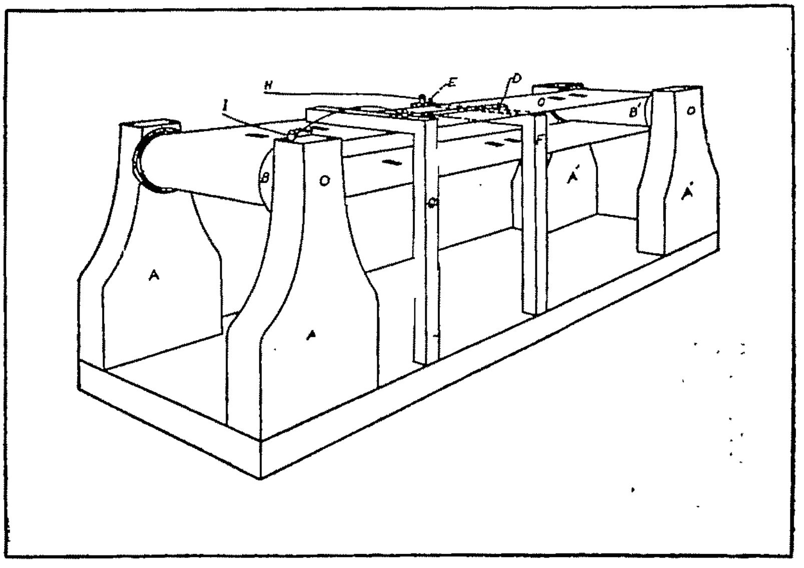

FIG. 4 STIMULUS UNIT

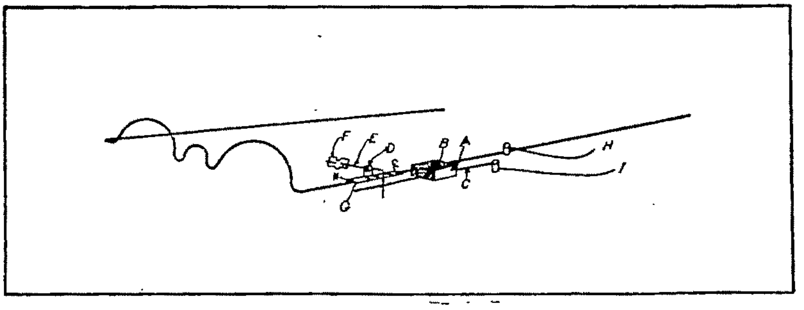

FIG. 5 RESPONDING UNIT

A DEVICE FOR READING CONTINUOUS GRAPHIC RECORDS

By G. L. Freeman, Yale University

The reading of continuous graphic records is usually a slow, awkward and unsatisfactory process. Below is shown a device which greatly relieves and improves the customary procedure.

Two rollers (a, a′) move the record over a polished wood surface (b) in a manner similar to the action of a kodak film. Counter-clockwise turns of the crank (c) draw the paper under the celluloid reference-guides (d, d′): clockwise motion allows the reader to return to points earlier in the record. The reference-

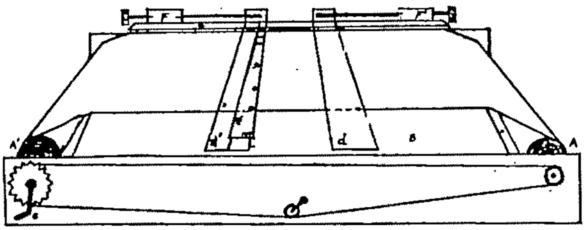

Fig. 6. Device for Reading Graphic Records

guides are placed perpendicularly to the metal bar (e) along which the record moves. The distance between the guides can be altered by means of micrometer screws (f, f′). One reference-guide carries a celluloid reading scale (g) graduated for 100 mm. above and below a zero point. Brass grooves make it possible to adjust this zero position to any record.

The device is equally suited to polygraphic, photographic and kymographic records and it will accommodate widths of paper up to eight inches. In practice one inserts a record roll over roller (a), threads the paper under the reference-guides and fastens the end to roller (a′). The use of the reference-guides is determined by the character of the material to be analyzed. If, *e.g.*, it is the number of reactions per minute, the user simply has to bring the first small mark in line with the first reference-guide and adjust the second guide so that it is over the next time-mark; then he reads the number of oscillations in the performance-curve which are included between the two guides. If the record originally moved at a constant rate, it will not be necessary to change the position of the guides for subsequent periods.

Equivalent readings may be obtained from a number of simultaneous curves by setting the guides to conform to the time-unit. If the height of any behavior curve is to be read at stated intervals, the zero position of the scale is set at the beginning of the record and the position of the curve is noted at every passage of the time-mark under the guide.

Since the device is operated by hand, the rate of reading can be suited to the type of record and to the individual performer. Turning the crank with one hand and writing with the other, the operator is able to achieve a fairly rapid pace with most continuous records. To insure accuracy, the paper must run evenly along the bar (e). At the conclusion of the reading, the record may be slipped off the roller, or it may be quickly wound back on the opposite roller.

The chief advantages are (1) greater compactness (the dimensions of the apparatus are 10 in. by 20 in.), (2) greater accessibility (discontinued reading may be resumed without difficulty in finding the place), (3) greater accuracy (the measuring instruments are applied at the same angle and height), and (4) greater speed (the rate of reading may be doubled).

AN INEXPENSIVE ROTATION TABLE

By Karl M. Dallenbach, Cornell University

We have constructed in the Cornell laboratory an inexpensive rotation table from parts of scrapped automobiles. As shown in outline in Fig. 6, half of a split differential housing forms the base of the apparatus, and a rear wheel, to which a three-ply circular wooden top (36 in. in diam.) is fastened by lag screws, serves as the rotating unit. We used a wheel with a floating axle, *i.e.* one that revolves upon the axle housing, so that the downward thrust of the weight upon the table is carried by the conically shaped lower bearings. After removing the floating axle and the internal brake band, we welded the two housings together. We loosened the external brake band so that the wheel turned freely, but we retained it so that it might be used in retarding or suddenly stopping the rotation. The far end of the brake rod connects to the brake lever of an automobile, which is securely fastened to the wall of the experimental room.

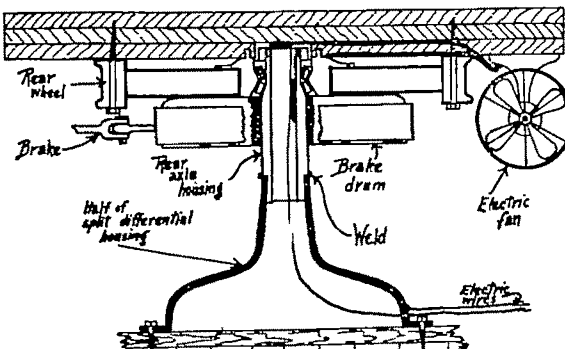

Fig. 6. A Rotation Table

The table may be rotated satisfactorily by hand, but a more convenient and exact method is offered by means of an electric fan which is fastened, as shown in the outline drawing, to the under side of the table-top. The wiring of the fan is simple. The 'hot' wire is connected by brush contact to an insulated circular copper disk, which is in turn connected to the fan; the 'cold' wire is attached to the steel base of the apparatus, and the circuit is completed by a wire leading to the fan from the metal hub of the wheel. The speed of rotation may be varied by changing the resistance in the fan circuit.

This device may be put to various uses. It may be used in rotation experiments by fastening a chair (in which the observer may be seated) or a board (upon which he may lie—and to which he should be strapped) to the top of the

table by thumb screws; it may be used, since it rotates freely and noiselessly, to present the stimuli in psychophysical experiments with lifted-weights; and it may be used, since it is strongly and solidly constructed, in experiments with animals, in particular anthropoids.

APPARATUS NOTES

A New Technique for Recording Sound Localization

I have already called attention to the fact that we have obtained many results in the field of sound localization without carefully scrutinizing the possibility of improvement in the technique of recording.[1] In comparing results obtained by the method of *visualization* with those obtained by the *pointing* method, while in a sense this work was done with an insufficient number of subjects, there was a definite indication that the method of visualization was inferior to the pointing method both in the average total displacement and in the percentage of correct judgments. The subjects believed, however, that the visual method was the more accurate of the two.

The method of visualization has been such a great favorite, among experimenters, moreover, that every effort should be made to improve this technique. In a recent thesis developed under my direction I suggested the use of luminous paint.[2] The difficulty with ordinary luminous paint is that it has to be exposed periodically to light in order to remain luminous. We therefore tried radium paint which proved entirely satisfactory. The numbers indicating the main positions in degrees were painted about 2 cm. high with marks at intervals of five degrees on black cardboard which was bent around in the form of a semicircumference with a radius of about one meter. Another sector in the sagittal plane could also be used in the same way. Instead of guessing as to the direction of the sources of the sound or at best recalling it from a visualized scheme the numbers could thus be called off from the luminiferous series in front of the observer in the totally dark sound-proof room. Although our conclusions so far are entirely empirical we feel confident that there is a distinct improvement in the warranted assurance of the reports. We are now undertaking further investigations which will be distinctly comparative in nature attempting to establish the possibilities of this technique.

University of Iowa CHRISTIAN A. RUCKMICK

Use of the Neon Lamp in Phonophotography

The short duration of the single flashes of a neon lamp, as well as the extremely high frequencies which such a lamp will follow, led us to seek a means whereby the flashes could be photographed. It was found that if a hole is melted in an ordinary neon lamp,[3] a small quantity of mercury injected and the lamp refilled to 10 mm. pressure with a commercial gas known as B-10 (a mixture of neon, helium and argon and available at all neon sign companies), the lamp will then glow with a faint bluish light to which photographic paper and film

[1] C. A. Ruckmick, Sound localization: a comparison of methods, *Proc. Iowa Acad. Sci.*, 32, 1925, 399-400.

[2] Anna Mathieson, Apparent movement in auditory perception. (Soon to be published.)

[3] Suitable lamps can be secured at approximately fifty cents each from the General Electric Vapor Lamp Co., 410 Eighth St., Hoboken, N. J.

are exceedingly sensitive. The lamp thus prepared can be made to flash in synchronism with a sound wave by connecting it to the output of an amplifier utilizing a plate voltage of the order of 350 volts on the last tube. The lamp used in this manner has been found very effective when used in conjunction with Metfessel's strobophotograph, a device which directly graphs frequency with respect to time.[1]

The advantages of photographing the flashes from a neon lamp in preference to techniques using optical levers and tambours are that the former (a) easily follows all frequencies within the range of the human voice, (b) has practically a linear characteristic over the frequency range of speech and song (if a good amplifier is used), and (c) permits the use of amplification for the recording of very weak sounds. These features make it possible to take consecutive records of all frequency changes throughout an entire vocal rendition.

We are also using the lamp very effectively in other pieces of apparatus as a time-line by omitting the amplifier and connecting the lamp in series with the secondary of a small induction coil, the primary of which is in series with a dry cell and an electrically driven tuning fork of known frequency. The neon lamp flashes only on the break of the current, hence giving as many flashes per second as the frequency of the fork used. By placing a narrow slit, extending across the width of the film, between the moving film and the lamp, parallel time-lines are secured.

Joseph Tiffin
University of Iowa
Milton Metfessel

A Manoptometer

The writer has used Parson's[2] manoptoscope in an attempt to secure data which indicate native handedness. It consists of a conical-shaped sighting tube and an exposing device which is placed "about 2 feet in front of" the subject, who indicates the letter (R-on-the-right or L-on-the-left) that he sees. Sources of error have been found and an attempt has been made to construct an instrument which will be relatively reliable and valid.

The manoptometer has a small wire attached to the top of the easel and to the sighting tube which makes it easy to keep a distance of two feet between them. The sighting tube tapers to a circular aperture of 15/16-in. Pictures of a cat, bird and boy are used instead of the letters L and R and the fixation disk, since a test of handedness is frequently given to pre-school children and to early elementary grades. The pictures of the cat and bird have been placed on separate slides so that they can be moved independently nearer to, or farther from, the center. This completely eliminates the necessity of having S move nearer or farther than two feet The chance order followed in moving the slides toward the center of the easel and the substitution of the pictures for the letters R and L have eliminated some of the errors of Parson's apparatus. The back of the easel has been divided into cm., making possible quantitative results. The changes seem to justify the statement that we have designed a new measuring device which we have called a *manoptometer* because it can be used to secure quantitative results in studying the facts of eyedness and handedness.

Eastern Kentucky State Teachers College
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[1]Milton Metfessel, The strobophotograph: A device for measuring pitch, *J. Gen. Psychol.*, 2, 1929, 135-139.

[2]B. S. Parson, *Lefthandedness*, 1924.

NOTES AND DISCUSSIONS

Some Uses and Misuses of the Term 'Aesthetic'

Scepticism as to the relevance of experimental methods to the peculiar problems of aesthetics is widespread and, in spite of valiant but sporadic efforts to dispel it, shows no evidence of abating. Pessimism as to the possibility of an experimental groundwork is, however, in all likelihood without factual warrant. The present backwardness is, in my opinion, the outcome solely of an arbitrary definition of the term 'aesthetic,' and an artificial delimitation of the field (plus the inevitable sequel, misguided methods of attack). Illustrative examples of such misleading and distorted usage may be drawn from a recent issue of this Journal, *e.g.* in an exploratory study of the effect on function of formal *versus* self instruction, Anderson[1] identifies "the impersonal valuation of objects as conforming or failing to conform to some conventional standard" or "approval and disapproval" as the typical aesthetic attitude (an obvious confusion of the aesthetic with the critical). A second instance of arbitrary narrowing of the field occurs in a suggestive study of the least perceptible difference between two silhouetted profiles, by White and Landis.[2] Duped by the chance discovery of a correlation between deviation and dislike,[3] the latter authors exuberantly forecast the discovery of an 'optimal' human profile, to serve as an artistic standard of comparison; heralding further the wide application of their method, to landscape and design as well as to faces,[4] and the establishment at last of a genuine quantitative method of 'aesthetics' (p. 435)!

The origins of these two correlative misconceptions are readily decipherable. The first, the identifying of aesthetics with the science of criticism and of norms, is obviously a hangover from the age when pedagogues and philosophers busied themselves in parcelling out the provinces of knowledge. A companion-piece was needed for Ethics, and therefore, with supreme *a priori* confidence, Aesthetics was pigeon-holed alongside it, as a normative or evaluative science. The second fallacy, the confusion of aesthetic appreciation with mere pleasurable feeling, harks back to nineteenth century hedonism, and the rigid affective dichotomy that limited feeling to the agreeable-disagreeable antithesis. A legacy of the Victorian era, it forms a fitting pendant to the 'mauve decade,' with its flair for mere prettiness and tepid sentimentality. The healthy reviving zest for the grotesque, the tragic, the ugly, the uncanny, cuts clean across it.

[1] O. D. Anderson, An experimental study of observational attitudes, this Journal, 42, 1930, 345 ff., esp. 355-359.

[2] R. K. White and C. Landis, Perception of silhouettes, this Journal, 42, 1930, 431-435.

[3] Every user of the Binet test is familiar with the analogous pseudo-aesthetic choice by the 5-year-old child of the 'prettier' of a pair of heads. The deviation from the placid, conventional and commonplace standard is rejected with illuminating comment, as unnatural, strange, 'cross,' terrifying.

[4] A somewhat similar principle is utilized in the Meier-Seashore Art Judgment Test (developed by N. C. Meier with the coöperation of C. E. Seashore, Bureau of Educational Research and Service, University of Iowa, 1929), in its 125 paired reproductions, consisting each of a standard and a variant.

Now actually, psychologist and layman will agree, 'conformity with a standard,' (Anderson's thesis) instead of furnishing the basis of aesthetic feeling, or furthering aesthetic appreciation, too often acts as an effective and all but insuperable obstacle, which must be somehow hurdled or evaded before genuine aesthetic (as distinct from practical or critical) perception can get under way. Up to a certain point, it is true, familiarity with form or structure (with the tricks and turns, key-changes and bandying of motifs, of the classical 'sonata-form,' or with the painter's legerdemain of light and line and balance) may augment one's rapport with the content of an art-work, and enhance one's affective apprehension of the whole. Yet human frailty—the ineradicable law of use and tuning of the nervous system—all too speedily converts this advantage into a handicap. Repetition (or familiarity) not merely dulls the splendors of the known, but throws one out of gear when the work of a new genius—a Franck, a Van Gogh, a Stravinsky—confronts one. The history of criticism in the several arts is stocked with instances of the lagging reception accorded the 'new' poetry, music, or drama. The customary abhorrence of deviations is the *bête noir* of the creative artist no less than of the practical sociologist or reformer. Familiarity dulls, unfamiliarity—unless human inertia is overcome—repels (a psychological truism).

Yet, curiously enough, once the inertia of perception is discounted or overcome, it is departure from, rather than conformity to, the accepted norms of composition and design which is potent to evoke the most dynamic aesthetic effect, and is the source of the highest artistic power. The so-called 'norms' (unities of time and space, simplifications of perspective, geometric formalizations of space, tonalities and modalities, key-relationships and harmonic sequences, metrical schemata) seem to serve genius principally as points of departure, *i.e.* spring-boards or standards from which to revolt. It is not the smooth, conventional academic workmanship of Field, Clementi, Jonson, Canova, Alma Tadema, which evoke our keenest aesthetic reactions, but the defiant, revolutionary violations of tradition by such men as Beethoven, Wagner, Whitman, Rodin. Nor is this a matter to be facilely pigeon-holed under the caption 'novelty' and dismissed.

In the light of the foregoing observations, one may hazard the prophecy that the Meier Art Test earlier referred to, however admirable in its aim and execution, and however useful within its limits, will never serve to discover the rising genius destined to forge a new aesthetic idiom. In its paired reproductions, the mutilated or altered member (in which some decorative canon, some maxim of balance or design or unity is violated) has often unintentionally become the more expressive and dynamic, yielding the keener aesthetic satisfaction. (One picks at random Plates 82, 88, 90, in illustration.) It is matter for chagrin that the psychologist should lag so far behind the great art and the progressive criticism of the twentieth century; busying himself with antiquated conventions and the tepid agreeable and disagreeable,[5] rather than setting out to explore the natural expressiveness of the various aesthetic factors (line,

[5]This is perhaps not quite fair to the Meier Test, since in certain instances emotional congruence or incongruence rather than the violation of some formal maxim may form the basis of selection.

form, color, tone quality, etc.), and the degree of universality with which they may evoke in perception the whole gamut of feeling (exciting, soothing, exalting, depressing).[6]

The grounding of aesthetic science upon universal norms or values (derived either from social sanction or the eternal verities) boasts, it is true, a long and venerable history; running back from Fechner through Schopenhauer and Kant to Plato and the Idea. Classicists and academicians in every age have, moreover, lent their energies to exalting into a cult and ritual the formulae for the correct outline of drama and symphony, or the laws of balance and symmetry in plastic art. Throughout the nineteenth century the quest went eagerly forward for the 'golden section,' the perfect proportion (*vide* Fechner's informal calling-card questionary as to the fitting dimensions for the rectangle), or the rules of substitutional symmetry.

Blinded by the hedonistic fallacy (the confusion of the aesthetic with the agreeable) to the true problems of aesthetics, investigators have spent themselves upon the assembling of innumerable and futile statistics on the pleasantness and unpleasantness of colors, color combinations, consonances and dissonances, harmonic sequences, lines and forms, etc. Such researches, however, have resulted neither in the setting up of canons of good taste, expedient for the education of the connoisseur, nor in the establishment of principles serviceable for the guidance of the artist in the manipulation of his media. The total upshot of so much misdirected energy is a mere handful of tentative norms of the crudely offensive or inoffensive; useful at most to the advertiser or lesser craftsman as honey-guides to syrupy stuff suitable to lure the prospective buyer, or as danger-signals warning of dynamic matter, to be handled with impunity only by a genius. Their significance for Aesthetics proper is, however, a vanishing quantity. What possible light can the charting of the relative pleasantness or unpleasantness of bi-tones throw on the charm of a tonal picture by Ravel? Or what is the relevance of Landis's projected norms of facial beauty or of design to the heightened effectiveness of the outlandish landscapes of Van Gogh or Ryder, or to Mestrovic's unrealistic sculptured portraits?

Even as contributions to 'pure science' these statistical studies of the agreeableness of simple sense-impressions and their patterns are unimpressive. Experimenters frequently admit themselves baffled. Even within limited and selected groups the expected universality of appeal is lacking. Subjects either display shocking idiosyncrasies, or manifest a perverse interest in the imperfect or slightly off-color. Such trends as are discoverable find little warrant or substantiation in the great and effective art of to-day—or yesterday. Gestaltikers seize the opportunity to drive home the dogma of the swamping of the part (plus its affective tone) in the whole. The possibility of an analytic science—or any science—of Aesthetics recedes into the distance.

Now in the study of attitudes cited earlier, it is only fair to add, Anderson obviously struggles to avoid this second fallacy, the confusion of the aesthetic with the affective. Misled, however, by the superior 'richness' of perception occasionally reported by his observers (and passing up, in my opinion, some

[6]Pioneering work along these lines has already been undertaken by Vernon Lee and Lundholm (in design and line), by Wells and by Bullough (in color).

excellent leads as to the peculiar and *personal* character of the experience), he reads the matter of norms, of approval and disapproval, and satisfaction based on the recognition of formal elements into the record (and into his subsequent instructions). In thus identifying the aesthetic attitude with the critical, and tracing the secret of aesthetic charm to the cognition of abstractions, he is of course not without precedent and support in contemporary criticism. The formal elements are indeed the A B C of art; and in every era small esoteric groups are extant who proclaim the identity of the aesthetic with intellectual pleasure in the play of form,[7] viewing Art itself as an abstraction, couched in an idiom fully intelligible only to the elect. The more impersonal and non-representative this idiom, the more purged of every hint of emotion or of the natural world of living objects, the more nearly it approaches to perfection (a view in many points germane to that of Kant, who like most metaphysicians warped aesthetics to his own ends, fabricating from it a bridge between the practical and the ideal, and reducing the aesthetic function to a kind of setting-up exercise for pure reason).

The bulk of liberal opinion, however, among both artists and critics, has always refused to hypostasize the purely formal or technical, or stress the intellectual at the expense of the emotional. The essence of art, in this maturer, saner judgment, is social expression, the heightening and broadening of experience, the 'summing up of life.' The formal elements are ancillary merely to this purpose, meaningless when abstracted from it. It is true that although subordinate, in this perspective, to the total expressive content, emotional and intellectual, the formal elements (such as rhythm, rhyme, decorative motif or color) appear on occasion autonomous, sheer embellishment, focussing attention in their own right. Even here, however, their independence, abstractness or intellectualization may be illusory merely; and, however masked, their vital articulation with the total schema the sole condition of their apotheosis from the affective to the aesthetic.

Manifold and diverse, it is true, are the auxiliary and secondary (pseudo-aesthetic) functions of the formal factors, which controlled psychological observation discloses. Sometimes they appear to serve a semi-hypnotic office, riveting attention on the aesthetic object, or absorbing and monopolizing the motor functions of the organism, so that the practical adjustive processes give way perforce to the aesthetic. Again, in art-works of a larger scope, they may operate as a concession to the limited span of apprehension, binding the discrete parts into a whole, and furthering unification and order (as *e.g.* in the dominance of the diagonal or the vertical in a mural painting, or of the ogive in the façade of a cathedral).[8] Or the rôle of formalization may be to heighten, by its very artifice, that sense of mastery of the craftsman over his media, of the spectator over his emotions and ideas, without which aesthetic exhilaration cannot reach its ultimate pitch. In many cases, emphasis on pattern and conventionalization may perform yet another subsidiary function, the 'distancing' of matter too poignant in its naked realism for aesthetic use (the

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[7]Inclusive of such personalities as Hanslick, Clive Bell, Hildebrand, C. K. Ogden—mainly of the 'Art for Art's sake' type.

[8]Hence facilitating the heightening of effect by promoting compactness, the presenting of *multum in parvo.*

Aristotelian catharsis of emotion). Yet in the last analysis the formal elements achieve legitimate aesthetic status solely by virtue of their intrinsic (or mimetic) symbolism, their germaneness to the dominant mood or emotion or idea of novel, symphony, landscape, portrait. Their final aesthetic justification lies in their character as vehicles of feeling (via movement), rather than in any intellectualizing moment (however masked this truth may be from the vision of the unpsychological observer).

The exploiting of this aspect of aesthetic process—the range of feeling absorbed from everyday emotional experience by the various rhythms, timbres, colors, melodic phrases, tonalities, lines and figures—is unquestionably the feasible and urgent problem of present-day aesthetics. The norms for expressiveness, though taking shape only as rather fluid generalizations, dependent upon objective setting and the subject's tuning, will yet, if preliminary observations are to be trusted, possess more validity and generality than those erected upon that most subjective and variable of judgments, the pleasantness-unpleasantness alternative. In any case, a moderate array of observations of the type proposed, carried on under various conditions of isolation and of incorporation in art-works in which the element studied dominates, will be found to promote more active and imaginative aesthetic appreciation in the subject than hundreds of judgments on the relative pleasantness and unpleasantness of the various sense-qualities. It is here that the silhouetted profiles of White and Landis should prove of value, as the various sets of emotional photos already have, in trying out the emotional suggestiveness of various facial configurations. Subjects will be found to differ widely in their responsiveness, although a vast mass of plastic art presupposes a universal sensitiveness and a fair agreement.

Hand in hand with researches of this order should proceed the study of the aesthetic function *in toto* (wrongly characterized, I believe, by Anderson as *impersonal*—as indeed the reports of many of his own subjects indicate). Exploration of the conditions that promote that peculiar blend of the proprioceptive and the teleceptive which marks the aesthetic moment[9] should open up new ground in the related province of 'creative imagination.'

In conclusion, it cannot be too vehemently urged that in the project in aesthetics here outlined the immediate emotional toning rather than the accessory 'associations' or imagery should constitute the focus of attack. Given the aesthetic *Einstellung* or *Anlage*, this emotional toning is as immediate and direct as the visual or auditory matrix of the perception, embuing it, fusing or amalgamated with it. Hints of this often slighted or neglected truth (of which, however, a few *Verstehen* psychologists and *Gestaltiker* appear to have taken cognizance) crop up throughout the reports of Anderson's observers. Here the developing graphic methods of psychophysiology must ultimately be relied upon to supplement and substantiate the pioneering work of descriptive psychology.

Cornell University ELSIE MURRAY

[9]Sometimes inadequately designated as 'empathy,' or the 'identification of the subject with the object,' or, better, by Piaget's infelicitous 'indissociation.'

HETEROGENEITY AND THE THEORY OF FACTORS

Amid much controversy which has been only of the 'barking in' type, it is pleasant to encounter such a genuine and fair attempt to advance science as the present work of Cureton and Dunlap.[1] In keeping with the rest of it, they have courteously sent me a copy of their manuscript and have invited me to add my own comments to their publication.

Well, the gist of the work, I think, is that I had suggested that "heterogeneity may give rise to spurious group factors which may tend to obscure the general factor." Whereas they quote Kelley as arriving at the opposite conclusion, namely that "heterogeneity should spuriously increase the relative importance of the general factor." They end by concluding that "heterogeneity increases the relative importance of group factors," which obviously agrees with the view credited to myself. In support of their conclusions they bring a formidable array of evidence; tests carried out on many hundreds of persons, belonging to nations that seem to have been nearly as numerous; pages and pages of toilsome and complicated mathematics. But I venture to think that the matter can be illuminated by a very brief consideration *a priori*.

Let *a* and *b* denote any two variables, whilst *g*, *x*, and *s* are respectively a general factor, a group one, and a specific one; *h* is the additional factor derived from the character in which the subjects are heterogeneous. For the convenience of avoiding roots, let us suppose that $\sigma_{s_a} = \sigma_{s_b}$ and that $r_{s_a h} = r_{s_b h}$. We get then at once[2]

$$r_{(g+x+s_a+h)\,(g+x+s_b+h)} = \frac{(\sigma^2_g+\sigma^2_x+\sigma^2_h)+(2r_{gh}\sigma_g\sigma_h+2r_{xh}\sigma_x\sigma_h+2r_{sh}\sigma_s\sigma_h)}{(\sigma^2_g+\sigma^2_x+\sigma^2_s+\sigma^2_h)+(2r_{gh}\sigma_g\sigma_h+2r_{xh}\sigma_x\sigma_h+2r_{sh}\sigma_s\sigma_h)}.$$

Now, by mere inspection of this equation, we see that *any* of the three kinds of factors involved, general, group, or specific, can acquire increased importance by its correlation with *h*. In the case of the present work some correlation between *h* and *g* would be expected by all who are not fanatics for racial equality; whilst some between *h* and *x* was as good as certain, owing to the verbal factor. Which should actually predominate is a mere accident depending on the construction of the tests in detail.

More serious than this point, however, seems to be another one, about which these authors and myself are perhaps less well agreed. In common with most psychologists and statisticians, they appear to treat heterogeneity as being essentially a source of statistical error—comparable in this respect with "attenuation"—so that it should always and everywhere be so far as possible eliminated. In particular, they represent heterogeneity of age, sex, race, and education as newly discovered sources of an illusory appearance of a general factor. In this sense they write approvingly of the "penetrating observation" that the general factor may be "principally composed of verbal elements and heterogeneity."

Against all this, I would urge historically that the influence of such heterogeneity of age, sex, etc. is not anything newly discovered, but on the contrary

[1]E. E. Cureton and J. W. Dunlap, Some effects of heterogeneity on the theory of factors, this JOURNAL, 42, 1930, 608-620.

[2]See C. Spearman, Correlations of sums or differences, *Brit. J. Psychol.*, 5, 1913, 417-426.

constituted the larger portion of the original work on the general factor.[3] In this work, as ever afterwards, the view was repeatedly stressed that the effect of heterogeneity is by no means spurious essentially, but only when, and in so far as, it happens to be irrelevant to the problem at issue. Thus in the very same situation, the same heterogeneity may be irrelevant and undesirable for one problem, but the reverse for another. For example, we may at one time only wish to take account of the individual differences that exist within a single race, whereas at another time we may have to bring into the reckoning the augmented differences that come from putting several races together. Here, as elsewhere, we may see that the effect of heterogeneity is not any statistical illusion, but a reality in the course of events. As for regarding it as something that should always be eliminated, I would say that *every* individual difference —whether of g, x, s, h, or otherwise—is *wholly* "composed of heterogeneity" (though I have little liking for the phrase). If you really exclude all heterogeneity, then all the differences, your very universe of discourse, incontinently vanish!

University of London C. SPEARMAN

AN UNCONSIDERED SOURCE OF MATERIAL FOR THE PROBLEM OF INDIVIDUAL DIFFERENCES

At a Conference on Individual Differences held in Washington in May, 1930, the author expressed the opinion that a profitable source for the study of individual differences was relatively untouched. This consists in a gathering together of the individual results of the studies in experimental psychology which were performed under strictly comparable procedures. Such a body of experiments exist for a great many psychological processes in a magnitude

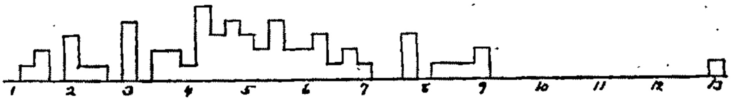


FIG. 1. DISTRIBUTION OF THE DIFFERENCE THRESHOLDS FOR LIFTED-WEIGHTS

sufficient to give the results statistical validity. Although these experiments were performed with a primary interest in a study of the central tendency, a treatment such as that here suggested must yield results of interest to differential psychology.

In order to illustrate the point, there are here gathered together the results of some 58 *O*s in the perception of lifted weights done by the method of constant stimuli in various studies by Urban, Rudisill and the author, under exactly comparable experimental procedures and instructions and with a standard weight of 100 grams. The distributions for the interval of uncertainty (which

[3] Cf. C. Spearman, The proof and measurement of association between two things, this JOURNAL, 15, 1904, 72-101; "General intelligence" objectively determined and measured, *idem.*, 201-293; also F. Krueger and C. Spearman, Die Korrelation zwischen verschiedenen geistigen Leistungsfähigkeiten, *Zsch. f. Psychol.*, 44, 1907, 50-114.

may be considered as the measure of sensitivity of the O) and for the point of subjective equality will be found in the accompanying figures (Figs. 1 and 2). The measures of central tendency for these results as given in terms of the average are:

Average Interval of Uncertainty = 5.11, P.E. 1.49
Average Point of Subjective Equality = 96.47, P.E. 1.03

In all of these studies the space error was eliminated by a turning-top table and the time errors were all in the first order. It is of interest to note that there is much less variability of the effect of the time error (as measured by the point

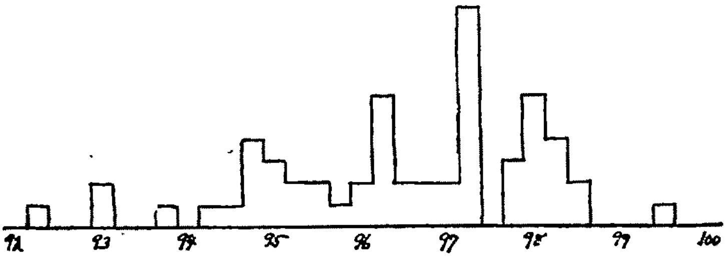


FIG. 2. DISTRIBUTION OF THE POINTS OF SUBJECTIVE EQUALITY FOR LIFTED-WEIGHTS

of subjective equality) than of the sensitivity of the observers (as measured by the interval of uncertainty). The point of subjective equality is dependent upon the relative number of greater and less judgments and the interval of uncertainty is dependent upon the relative number of equality judgments. One may therefore conclude that there is greater individual variability for the equality than for the extreme judgments.

The present note is presented in an effort to indicate what may be done for the problem of individual differences by a systematic study of the experimental literature. It is also published in an effort to urge authors of experimental studies to publish all of their individual results so that these may be of use for future studies.

University of Pennsylvania SAMUEL W. FERNBERGER

APPARENT MOVEMENT IN LISSAJOU FIGURES

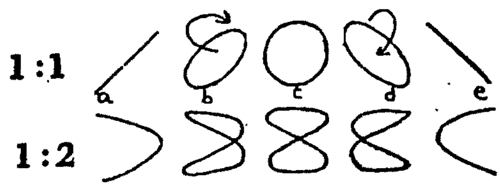
Lissajou figures, so named in honor of the investigator who first studied them, are figures which arise from the composition of harmonic movements. Since tuning forks give a quite clear simple harmonic motion, it is possible to secure interesting patterns by reflecting the combined movements of two forks upon a screen.[1] Physicists use these figures in making very exact determinations of the vibration rate of a fork. If the d.v. of a standard fork is given, the d.v. of an unknown fork may be deduced from the figure yielded by the two forks

[1] I am indebted to my colleague, Dr. P. H. Gaehr, for my first acquaintance with Lissajou figures. They can be produced by electrical means as well as by tuning forks. The Westinghouse tube oscillograph is available for the purpose, but the set-up involves niceties that may well discourage all but experienced physicists.

acting in unison. These figures, however, have a special significance for the psychologist in view of the current interest in apparent movement. The pattern of the Lissajou figure depends upon the ratio of the two vibration rates used to produce them; but I have found no printed reference to the fact that such figures execute very lively movements that have all of the familiar signs of illusory motion.

Two electrically driven forks are required, whose frequencies are adjustable. At the end of one prong of each fork, a small mirror is secured. One fork is set up to vibrate horizontally, the other fork to vibrate vertically. They are so placed, relative to each other, that a focussed beam of light strikes one mirror, is reflected to the other mirror, and then on a screen. A silver screen gives the best results.

The beam of light traverses its path on the screen so rapidly that the observer sees the path, but not the motion of the beam. If one should attach a light pendulum to the bob of a heavy pendulum, and set both bobs to swinging at right angles to each other, one would have an approximate, slowly moving



model of what happens during the formation of Lissajou figures. The type of pattern yielded by the tuning forks depends on the ratio of the two d.v. employed. In the accompanying diagram, the reader will find the figures that accompany two simple vibration ratios. If the ratio happens to be exactly like one of those entered on the left margin, one gets one of the figures in that row If the ratio departs slightly from perfection, the screen shows a series of patterns in succession, from left to right and then from right to left. This is the situation that interests the psychologist. In place of seeing one figure being replaced by another, the observer may see *one* figure rotating, presenting its different 'sides' to the observer.

The writer, while planning a more extensive study of these figures, may report some general results of personal study of the Lissajou figures.[1] The most general statement that one could make is perhaps the remark that Lissajou movement has both subjective and objective determinants. We may note

[1] A few suggestions may be made in the interest of others who wish to explore the possibilities of these figures for their psychological significance. One should have forks whose weights can be adjusted automatically while the forks are in motion. The possibilities of research will also be greatly increased by using forks whose magnets are on sliding bars, which can also be adjusted while the fork is in motion. I can recommend Lissajou figures as excellent objects of research considered from the observer's point of view. The figures are unique, the apparent movement is very realistic, much more so than the familiar demonstrations of phi, delta, gamma and other movements. Observations can easily be taken from a score of observers at the same sitting.

some of the subjective factors first. Consider, for instance, the tri-dimensional movements yielded by the figure arising from unity ratio, the ratio, 1 : 1. To begin with, the observer may not notice the tri-dimensional motion at all. *Attitude* thus emerges as a familiar determinant. If motion appears, one sees the line a as an ellipse on edge. It arrives at stage b by rotating (see arrows), at the same time altering its vertical axis so that the observer finds himself facing a circle at stage c. Still rotating, the vertical axis now swings to the opposite direction giving the ellipse d. Soon, the edges of d are again in the line of regard, giving the line e. These movements may, however, be entirely reversed, by that same mysterious change of 'mental set' which changes the familiar reversible perspective figure. What is more, it is easy to compel the figure to rotate in clockwise manner to stage c, pause, and continue the movement in counter-clockwise direction.

The subjective factors also appear in the familiar garb of *habituation*. For instance, the clockwise motion is easier to produce than the reverse. Continuous motion, whether clockwise or the reverse, is easier to secure than an alternation of the two. If, while trying to impose counter-clockwise motion, the figures suddenly become larger or rotate more rapidly, the observer finds his control 'slipping' and the figure moves in the easier clockwise direction. The loss of control is very much like the loss of coördination that would appear if a weight one handles suddenly decreases. A little practice is required to secure any form of motion. I find, as Köhler noted with a similar phenomenon with a static figure,[3] that if the figures take a new orientation on the screen (secured by changing the plane of vibration of the forks while maintaining them at right angles to each other) that the effects of previous practice are partially or wholly lost, and one must renew practice in order to recapture the motion. Past experience, indeed, reaches deeply into these phenomena. The physicist, with his entire attention fixed on the *form* of the figure may fail to see the tri-dimensional motion; but he is bound to see motion on the plane of the screen. He sees a line (a) splitting to form the ellipse (b), etc. But why this result of *one figure changing* instead of *five figures in succession*? Perhaps it is past experience with material bodies which leads him to see, not many bodies successive in time, but one body moving. But, since this inveterate habit rests upon a truth regarding physical bodies, *i.e.* the truth that many forms appearing at the same point in space, are but *aspects* of one body moving, perhaps this determinant may be regarded as objective.

There are of course objective determinants of the motion. The form of the figure, the speed of motion, the size of the figure depend on the apparatus. One cannot impose *any* motion upon a figure. It appears that the apparatus determines several possible motions from which the observer may choose.

The primary indication of these results is to contradict the view of illusory motion which seeks its complete determination in objective terms, leaving out of account such factors as the direction of attention, instruction, attitude. Within limits, one sees what one *intends* to see.

Wells College C. O. Weber

[3] W. Köhler, *Gestalt Psychology*, 1929, 186.

The Fifth Annual Meeting of the Midwestern Psychological Association

The meetings were held this year at Antioch College, Yellow Springs, Ohio, on May 23 and 24. There were five programs of papers and a round-table discussion. The customary reports from the laboratories were given in the afternoon of the last day, which was followed by the annual dinner in the college dining room.

The first session began with two parallel programs. Program I, presided over by Dr. C. A. Ruckmick (Iowa), opened with a contribution from J. A. McGeoch (Arkansas) on the influence of different interpolated activities upon retention. The results of Müller and Pilzecker were apparently contradicted in that relatively small amounts of inhibition were discovered when three different sorts of activities were interpolated. These three activities consisted of the following: (1) ten repetitions of a second list of nonsense syllables followed by reading for the remainder of the interval, (2) continuous tapping, (3) ten electric shocks administered at irregular intervals while the subjects read humorous material. In another series continuous color-naming was used in place of the electric shocks. Tapping gave the least inhibition.

T. Perkins (Kansas) showed a number of diagrams depicting the tendency towards symmetry in visual memory. This illustrated the so-called *law of pregnancy*, namely that the configuration tends to become as stable or unified as possible. In the discussion it developed that the tendency was in the direction of meaningful material when the original presentation was relatively devoid of meaning.

The rôle of head movements in auditory localization was analyzed by P. T. Young (Illinois). When the movement of the head becomes ineffective by extending the pinnae of the ears, by means of tubes, beyond the walls of the *O*'s room, sound localization is restricted to the semicircular arc outside of the visual field, generally in the direction of the occipital region. The tendency was toward limiting localization to a plane rather than to a tridimensional manifold when head movements were no longer operative.

H. Woodrow (Illinois) read a paper on individual differences in the reproduction of temporal intervals. In an extended study no 'indifference' interval was revealed below which intervals would be overestimated and above which they would be underestimated. The results indicated that variation in the instruction given to *O*s, or different attitudes tacitly assumed by *O*s, accounted for the constant errors in judgment.

M. A. Weichbrodt (Northwestern) compared three different methods of transmitting speech: lip-reading, vibrational stimulation of the fingers with the microphone before the mouth, and the same procedure with the microphone held against the larynx. The stimuli were consonants followed by the long vowel 'e'. In general, lip-reading was slightly more efficient although there were certain combinations which gave best results when the microphone was held against the larynx. Simultaneous visual and tactual presentation, however, brought still better results. Artificial deafness was produced in some cases by means of four telephone buzzers.

Program II, Dr. J. P. Porter (Ohio) presiding, opened with a paper by S. Renshaw (Ohio State). The results of a two-year investigation of the influence of motion pictures on children's sleep gave conclusive evidence that children

exposed to romantic situations in motion pictures were much more disturbed by restlessness during their sleeping period than other children not so exposed.

M. G. Cutsworth (Kansas) studied the successive brightness discrimination of chicks and found that 85% of each group learned the problem completely, making choices in terms of the relations of the total brightness-situation rather than in terms of isolated elements and absolute brightnesses.

W. Beasley (Ohio State) presented the results of work done over a period of several years on the monaural phase effect with pure binary consonances. He has perfected a very ingenious apparatus by means of which a composite tone containing five partials can be produced with the possibility of making independent changes in phase.

D. McL. Purdy (Kansas) studied the differential chromatic thresholds of the spectral colors. The threshold of a hue was found to be inversely proportional to its complementation-valence whereas the chromatic thresholds of two complementary hues were in the same ratio as the respective intensities of these hues that were required for complementation. This effect is more easily explained in terms of some theory like the Hering theory of antagonistic processes than in terms of the Young-Helmholtzian theory.

Some aspects of the psychophysics of the vibrato were presented by J. Tiffin (Iowa) using the psychometric procedure of the constant process. It was found that the vibrato at threshold frequencies was practically independent of the rate of the vibrato and independent of the scale region. The pitch of the frequency vibrato was localized as definitely as was the pitch of a tone of constant frequency; when the vibrato contained a wide extent of frequency, however, some *Os* localized the pitch extremely sharp and others extremely flat.

H. R. DeSilva (Kansas) through an interesting technique has studied the 'blindness' of the so-called blind-spot discovering a number of anomalies.

Program III, Dr. R. H. Wheeler (Kansas) presiding, was held Friday evening and had an attendance of over two hundred people. H. Klüver (Behavior Res. Fund, Chicago) showed in terms of motion pictures some of the functions involved in the 'relational' reactions of monkeys to sensory stimuli. The behavior of the animals while learning to compare weights, together with additional cues from the visual and acoustic field, was clearly demonstrated.

The next paper by L. S Tsai (Chicago) was also demonstrated by motion pictures. After a theoretical analysis of handedness, typical cases of left-handedness, right-handedness, and ambidexterity, selected from among two hundred white rats, were illustrated.

H. M. Johnson (Mellon Inst.) showed some very interesting photographs indicating various postures assumed during sleep throughout the night. These pictures were automatically taken whenever posture radically changed. A great variety were shown indicating that there is no one favorite way of sleeping.

D. F. Boder (Lewis Inst) demonstrated the ability of white rats to react to lights which were flashing at different speeds. Some of the animals were also taught to discriminate between a continuous light and a flashing light. A demonstration of the apparatus was given.

The last paper on this program by J. R. Patrick (Ohio) concluded that white rats do not learn the maze pattern alone but only in connection with other incidental stimuli acting at the same time. When there is a sudden

shift of incidental stimuli numerous errors are made in succeeding test-runs. These results were related to the problem of *redintegration* and of *Gestalt*.

On Saturday there were again two parallel programs. Program IV, Dr. W. S. Book (Indiana) presiding, began with a paper by N. W. Shock (Chicago) on the acid-base balance of the blood and mental performance. By means of the micro-chemical technique, the percentage of red cells, the pH, and the total carbon-dioxide content obtained from a single sample of two drops of blood, 15 *Ss* were found to have a slight, though consistent, positive correlation between an alkaline blood reaction and the report of well-being. There were no significant correlations, beetween blood measures and mental performance.

K. S. Lashley (Chicago) reported on cerebral function in pattern-vision of the rat. The rats were required to jump a short distance at a visual pattern. The correct pattern then dropped away and food was supplied. This was used as the measure of visual acuity. When the occipital cortex of the cerebrum was removed patterns were no longer distinguished but single objects, small differences in brightness and, to some extent, distance were still distinguished. Smaller lesions within the visual area retarded the formation of visual habits.

S. H. Bartley (Kansas) reported some preliminary work on action-currents of the brain.

L. E. Wiley (Chicago) read a report concerning an auditory habit and its cerebral mechanism in sixty albino and hooded rats After the entire cortex had been destroyed forty-nine animals that survived were taught to relearn the T-maze with the same negative response to auditory stimuli as before. The brains were then histologically examined with the result that the part of the cortex involved was fairly well determined but within this part no one portion seems to be more essential than any other.

The next paper by A. R. Lauer (Ohio) furnished miscellaneous information concerning electrical resistance in living organisms as related to age, sex, skin color, and various other physiological conditions. Resistance appeared to be relatively constant from time to time in the same individual with higher values in women than in men, and much higher values in sleep than in the waking period, and with some racial differences.

M. L. Steckel (Chicago) reported on the relation between parental age and the intelligence of offspring. With maternal ages ranging from fifteen to fifty-one years and paternal ages from fourteen to seventy-one and taking account also of the amount of disparity between the parental ages it was found that on the average children were more intelligent when the age of the mother was twenty-six or over, and that of the father thirty or over, than below these respective age limits. The nearer the ages of both parents approached each other the higher was the intelligence rating of the children apt to be but it was also favored if the age of the father was in advance of that of the mother.

Program V, Dr. J. J. B. Morgan (Northwestern) presiding, began with a study of personality by A. Jersild (Wisconsin) in which the various factors were itemized and evaluated.

L. L. Thurstone (Chicago) proposed the 'method of absolute rank order,' also called the 'order of merit method,' as one of the more promising of psychophysical procedures as was shown by plotting the relative frequencies. Very small discrepancies were obtained between the much more laborious form

of the constant method, known as the method of paired comparisons, and this much simpler method of rank order.

E. L. Schott (Henry Ford Hospital) investigated the superior intelligence of patients with nervous and mental illnesses. Data collected over a period of four years showed no causal relationship between intelligence and instability.

The relation between ocular dominance and handedness among psychopathic cases were studied by N. B. Cuff (E. Ky. St. T. C.). By means of a variety of tests given to 203 children in grades two to eight, the speaker pointed out that both eyedness and handedness were very broad in their significance and showed minor variations in each individual. From 20-30% of the children were probably natively left-handed, while 93.6% were at the time of the study ultimately right-handed.

A new objective test by introversion-extraversion was proposed by A. R. Gilliland (Northwestern). These traits were defined in terms of their extremes so that out of 125 cases only three were found to be overlapping. Tentative norms for both normal and abnormal groups were shown.

H. H. Anderson (Iowa) reported on the incidence of behavior problems and personality differences in school. A number of forms were prepared in order to determine the various factors relating to maladjustments.

F. Fearing (Northwestern) presented some interesting photographs of the postural expression of emotion made by professional dancers. Preliminary results showed the part played by face, trunk, arms and legs as a cue to emotional expression, with both expert and non-expert judges.

About twenty individuals showed a considerable interest in the round-table conducted by Dr. R. H. Wheeler (Kansas) on the contribution of Gestalt to the problem of learning, which ran simultaneously with the other two programs. During the lunch hour, which followed these programs, about fifty members and friends of the new honorary psychological fraternity gathered at the first district convention. Dr. C. A. Ruckmick (Iowa) spoke briefly and encouragingly on "The Hope of an Honorary Psychological Fraternity." Officers for the ensuing year were elected.

On Saturday afternoon general laboratory reports were called for with Dr. W. B. Pillsbury presiding. A number of these reports were available in mimeographed form. Among the laboratories represented were Antioch, Arkansas, Chicago, College of the City of Detroit, Illinois, Iowa, Inst. for Juvenile Res., Kansas, Kentucky, Lewis Inst., Michigan, Missouri, Northwestern, Ohio State, Ohio University, Ohio Wesleyan, and Wisconsin.

This was followed by a business meeting at which various official reports were read and the new proposed constitution was adopted. All advanced students of psychology now become eligible for membership. The officers are to be elected by mail ballot. A number of other changes were also made. The invitation of the University of Chicago to meet there next year was accepted.

At the annual dinner the president of the Association, Dr. M. F. Meyer (Missouri), read a carefully prepared paper on "The Religion of Adam and Eve." The Biblical story was interpreted as being no 'sex story' at all but a beautiful piece of poetry showing the transformation of the passion of love into the realization of life with its duties and responsibilities to society and the human race.

University of Iowa CHRISTIAN A. RUCKMICK

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

A History of Experimental Psychology. By EDWIN G. BORING. New York, Century Co., 1929. Pp. xvi, 699.

In terms of comprehensive scholarship well-seasoned and integrated no other work has appeared in America equal to this history since Titchener's four-volume series of manuals in experimental psychology. Although this may seem like a headline to call the reader's attention to what follows he very likely will find that there is adequate justification for it. The book begins with a broad foundation sunk deeply into the ground of other sciences and ultimately touching the basic work of Greek cosmology. The first chapter deals with the evolution of the scientific point of view and presents materials from before Aristotle to Darwin in support of the contention that science is fundamentally based on the method of observation. Rationalization is not foreign to science but comes legitimately only after observation and recording of natural phenomena have taken place on the basis of premises which "are given externally." The reviewer is not absolutely certain of the temporal sequence of these scientific processes but assumes that the author refers to the logical priority of the steps taken. Astronomy is the first science to fit into the requirements set down by the author and, therefore, the oldest science now in existence. This contrasts sharply with the reviewer's opinion that mathematics is usually referred to as the oldest science. It is not, of course, its principal task to observe natural phenomena but only to note abstract relationships between them. Some very clear expositions are given of early astronomical observations. Early physics and biology together with their gradual development to recent times are briefly reviewed. The much argued theory of emergent evolution is not discussed, however, possibly because the author does not consider its influence on experimental psychology important.

This chapter forms the introduction and serves, as does the overture to an opera, in suggesting a number of themes which are later much more extensively treated. Therefore such succinct statements as "physiology gave birth to experimental psychology" must be allowed to produce their shock of discord when one thinks of the foster parent of physics, because later the author takes considerable patience to clear up this situation. It is easy gun-play for the reviewer to note what he thinks are serious omissions but even considering the small space that is allotted to the early Greeks it is difficult to understand why the name of Alcmaeon, a younger contemporary of Pythagoras, was omitted, in view of his rather startling contributions to anatomy, physiology and medicine. This chapter ends as do all the chapters with a few pages of notes on each one of the major headings, citing references for further reading, and commenting on minor details suggested by the text. Here one misses, however, the enlightening influence of Gompers and Windelband in favor of the more modern writers on the history of science.

The next chapter admirably summarizes the outstanding work done during the first half of the last century on the physiology of the nervous system. Going with disproportionate detail into Johannes Müller's *Handbuch*, it were better here and in the notes if the various *Abtheilungen* were repeatedly referred to as "parts." In the third chapter the mind-body problem finds its early development in phrenology, although due credit is given to special writers such as Descartes who bravely faced this task and made outstanding contributions to the subject. The more detailed physiology of the brain then follows together with the general histology of the nervous system. After a very competent discussion of the doctrine concerning the specific energies of nerves with particular emphasis on Müller and Helmholtz, we are carried through the early contributions with respect to the several sensory modalities. Chapter VII furnishes an interesting summary of the development of hypnosis under the subheadings of the outstanding men. Here, as before in the discussion of brain localization, the author takes occasion to point out that the front line of science sometimes moves too fast for all supporting units to follow. Sometimes, too, progress is made through what appears to be, at least at the moment, a partial retreat. Another early contribution to experimental psychology comes from questions arising in astronomy concerning the personal equation. This closes the section on physiological psychology of the first half of the last century.

Now comes the historical treatment of a further approach through philosophy involving in some places a repetition of many names of men already partially treated. Descartes, Leibnitz, Hobbes, Locke, Berkeley, Hume, Hartley, the Mills, Bain, Spencer, Herbart, and Lotze are given due credit through extended discussion for contributions to the background of experimental psychology. There is much here that might be questioned from the point of view of interpretation but, when outstanding scholars disagree, an historical psychologist is entitled to his own independent evaluation. As a matter of fact not much controversial material is included. It must be understood, of course, that other writers are referred to beside the names mentioned.

We are presently almost at the middle of the book, about to enter the next section through the door bearing the inscription "the founding of experimental psychology." Chapter XIII begins the section with a detailed summary of the history of psychophysics. It goes into the matter of Fechner's life and continues with an appraisal of the mathematical development of psychophysics which few physiologists in this country could give. We then pass on in the next chapter to Helmholtz' contributions as a physiologist, physicist, and psychologist. His contributions are very competently given under the headings of sense-physiology, empiricism, unconscious inference, perception, scientific observation, and the fundamental laws of mind.

When we come to Wundt we find one of the richest chapters of the book. Wundt's system together with the fundamentals which underlie it are presented in such a way that one can not miss his tremendous service in staking out a valid field for psychology. The conditioning clause must be added, however, that because Wundt stressed, especially in the early years, the necessity of controlled introspection he brought in the materials which later furnished tinder for the behaviorists. The author does well to limit himself to the barest essentials of the Wundtian doctrine all the while stressing the importance of

this prodigious writer and worker in the interest of establishing a tangible, experimental science of the mind. The temptation must have been great to dig deeply here, since Wundt's energy, particularly later in life, was also directed into fields like those of social psychology and logic where the experimental techniques were presumed but not used. Brentano, Stumpf, and G. E. Müller form the theme of the next chapter where the material is again chiefly biographically treated. The reviewer easily but perhaps unfairly suggests that Stumpf's contributions in the direction of primitive music, especially his excellent work on *Die Anfänge der Musik*, do not here receive adequate treatment.

The next chapter bears the heading "The 'New' Psychology." Ebbinghaus, Külpe, Mach, Avenarius, Watt, Ach, Messer, Bühler, Titchener, Preyer, Delboeuf, Lipps, Ziehen, Münsterberg, Kraepelin, Meumann and a number of others including the psychological physiologists are among those whose contributions are woven into this chapter. Titchener naturally receives more extended treatment. The keynote to Titchener's contribution is found in the statement that he aimed to have "psychology stand upon its own legs" instead of "bolstering it up with any artificial props." Much is made of his intellectual isolation and his frequent polemics. The psychology of act, content, and form is separately treated in the next chapter with extended references to Von Ehrenfels and Lipps and Meinong.

There is a separate chapter on British psychology with separate headings for systematic psychology, animal psychology, mental inheritance, statistical method, Galton as a psychologist, and experimental psychology. Much biographical material is here as elsewhere introduced and the change to a different traditional point of view is gradually made with considerable understanding. We then proceed to the pioneering work of American psychologists with extended discussion of William James, G. Stanley Hall, Ladd, Baldwin, Cattell and others. In the last category the reviewer misses reference to Seashore, whose contributions to experimental work through his manual of "Elementary Experiments in Psychology" published in 1908 must have had considerable influence in the development of experimental psychology in those institutions which lacked the means of obtaining expensive apparatus. The movements of functional psychology, mental tests, animal psychology and physiological psychology form the ground work of the next chapter, which is followed by a chapter on *Gestalt* and behaviorism. The materials of both points of view are squarely presented. There is less negative criticism of the former than of the latter but the reviewer is surprised that the approach of Max Meyer receives no recognition here.

There are then two summaries, a recapitulation from Fechner to the present and a brief reinterpretation. The author bares his own attitude in frankly answering the question, to what extent has the new psychology justified itself? Experimental psychology is eminently worth while. "The person who doubts that the results have justified the importance that has been attached to the invention of experimental psychology must be either ignorant or influenced by a disappointment that the progress of experimental psychology has not aided in the solution of his own particular problems." The author's frankness is shown by his admission of disappointment that there has not been any great

idea or discovery that has revitalized the science, for which he gives two reasons: (1) It has had no great psychologists of the order of Helmholtz or Darwin whose contributions lie entirely in the field of psychology; (2) there has been considerable conflict within the camp of psychology itself. He believes that psychology should entirely relinquish "its philosophical heritage"—with which the reviewer heartily agrees, in so far as any science can so do. Curiously enough, one of the older sciences, physics, is now showing us an example in the opposite direction! There is a brief appendix on French Psychology and Psychology in Other Nations. The author is rather strict in his delimitations since he does not believe that France has ever been "at the core of experimental psychology." Comprehensive indices of names and subjects close the book.

The reviewer has found no occasion for retracting the statement made at the beginning of this review. Only those who know the amount of effort and discipline that lies behind historical contributions can appreciate the place which this book is bound to occupy in American psychology. Unfortunately too much material has been published in book form which lacks or fails even to approach the insight displayed in this volume. The style is lucid, direct, and lively. Interpretations, both expressed as actual textual content and implied in terms of place and area of treatment, may rightly differ from writer to writer. But there is no book now available that is as circumspect and pithy as this *History of Experimental Psychology.* Following the fashion of many modern books where such devices are found extremely useful, as in Count Luckner's *Exploits,* the inside covers and fly-leaves present a map showing the location of the more important universities in Germany, Switzerland, Austria, and Poland. The volume appears in an attractive format, the contrast of whose binding of gold on black may be significant: gold for the precious experimental material which gives psychology a right to exist; black to voice the disappointment of the author in regard to the mournful conflict within the tribes and the dearth of great *patres familiarum* in their midst.

University of Iowa CHRISTIAN A. RUCKMICK

Gestalt Psychology. By WOLFGANG KÖHLER. New York, Horace Liveright, 1929. Pp. x, 403.

In this book we have the long awaited *credo* in English of a leading member of the *Gestalt* school. While many may have expected Köhler's exposition to contain a definitive statement of the *Gestalttheoretiker,* the whole tenor of the book is against a closed systematic formulation of beliefs. The gaps and *terra incognita* of psychology are so great even for *Gestalttheorie* that Köhler speaks of the point of view as resembling "in 1928 a promising start more than a complete achievement." He says about his own efforts in this book that "nothing is easier than to criticize it." True, this book may be "the incomplete portrait of an incomplete thing;" nevertheless *Gestalt* psychology, born in 1912 at the latest, is as old as behaviorism, which Köhler treats as quite grown-up. A psychological system will be evaluated by its contemporaries before its authors feel it has reached its majority. Certainly, whatever Gestalt psychology may become, it is bound to be taken now for what it is now, and, with an exponent like Köhler whose penchant for theorizing is well known, no fears

need be felt for the position of the school nor its present expression in this book. It would be hard to find a more beautiful, clear exposition of fundamentals in the literature of any movement. Would that every school had as able a defender and as sure a position to defend!

Perhaps the dominant note of *Gestalt Psychology* is set by the question: "How shall we explain psychological phenomena?" The answer to this question takes us from a consideration of scientific method through the postulates and practices of behaviorism and introspective psychology to the point of view advocated by Köhler. The starting point of all science, according to Köhler, is and must be the world as it is found naïvely and uncritically, even though the naïveté may be lost as we proceed. In their beginnings all sciences start with qualitative facts before applying the more refined methods of measurement and analysis that characterize an adult science. Psychology is especially in need of a more naïve approach to its subject-matter since it is "in most cases easier to see 'anger' directly in a subject's attitude than to state and measure the adrenin in his blood" (p. 41). Measurement in physics has been used in the service of discovering the *nature* of the states and processes investigated but in psychology we have not been in the habit of asking questions about the processes through which a real knowledge of phenomena is gained (p. 49). Both the intelligence tests and the work with animals have failed, often, to reach to the bottom of what was being studied by neglecting to ask the right sort of question.

The attempt of the behaviorist to imitate adult physics in the young science of psychology our author regards as abortive and a failure. It has led to a fundamentalism in outlook which has delimited the field of psychology too narrowly, especially where it concerns the rejection of so-called subjective phenomena, or, more properly speaking, the facts of direct experience. "The behaviorist must be wrong when he declares that . . .direct experience when observed introspectively is not being observed scientifically, since in the case of observation in physics the situation is similar: the material to be observed and the process of observing belong to the same system" (pp. 30 f.). Dichotomizing the world into 'subjective' and 'objective' is no longer possible since all observations depend upon processes in the same system, viz., my physical organism (p. 30). Some psychologies bear the marks of 'subjectivity' and others of 'objectivity' but the observation of both is essentially the same. The contrast between them is something different from the genetic 'subjectivity' of both types of process or observational material. It might be inferred from this criticism that Köhler is a subjectivist or a sort of introspectionist. Nothing could be farther from the truth. He has just as little sympathy for the position of the introspectionist, whose psychological world is quite foreign to him. "When I talk about a chair, the chair of my everyday life is meant and not some 'subjective phenomenon' to be observed by highly trained introspectionists, but utterly unknown to me" (p. 25). Introspection, like behaviorism, approaches its data with a highly sophisticated (or shall we say prejudiced?) attitude which Köhler believes is blinding us to many interesting problems.

Introspectionists and behaviorists are essentially alike in their logical approach to their problems. Both have erred in employing what the reviewer would call 'direct' or 'simple' analysis. Köhler does not mince words in stating

what he thinks about the accomplishments of either school. Of introspectionism he says: "It was strange to see how the description of a direct experience, what happens, for instance, during a single comparison of two tones or colors, could fill hundreds of pages without giving us the slightest positive idea about how the occurrence and accuracy of such a comparison might be explained" (p. 10). Of behaviorism: "As an imitation of physics it is scarcely a satisfactory achievement for the behaviorists to have taken the old concept of reflex action from physiology (including the reflexes of inner secretion) and to give us no further comprehension into the formation of new individual behavior than is offered by the concept of positive and negative 'conditioning' " (p. 55). Finally, both schools, in spite of superficial differences, are really of the same breed, for "I do not see why introspectionism should be preferred to behaviorism or vice versa. They are so much alike in their fundamental attitudes, that all their wrangling seems like a family quarrel to the onlooker" (pp. 101 f).

The alternative to the logic of behaviorism and introspection is the fundamental thesis of *Gestalt* psychology. It asserts that *"to a context, experienced as 'one thing' belonging together, there corresponds a dynamical unit or whole in the underlying physiological process"* (p. 106). Stated a little differently, "instead of reacting to local stimuli by locally and mutually independent events, the organism reacts to an actual *constellation* of stimuli by a total process which, as a functional whole, is its response to the whole situation" (p. 106). The importance of this principle lies in the fact that "this is the only viewpoint which can explain how to a given local stimulus there may correspond altogether different experiences as soon as surrounding stimulation is changed" (p. 106).

The restrictions which must be placed upon this statement of point of view are clearly indicated by Köhler. In physical processes there are two sets of factors which must be considered: the actual forces involved and the constant limiting or boundary conditions which restrain the former. Thus preëstablished anatomical conditions are present to some extent in vision and touch, but certainly the limiting conditions in learning and habit formation are not as hard and fast as they have been pictured, nor even as set as those in the sensory fields where *Gestalt* phenomena abound. Orderly functions may run their course without being controlled by special arrangements preëstablished *ad hoc*.

Nor is the alternative to definite paths and connections vitalism. Order may result through *dynamical interaction*. To the reviewer's mind the most important and far-reaching statement in the book is the following: "Dynamical interaction, undisturbed by accidential impacts from without, leads to orderly distribution, though there are no special regulative arrangements, and definite direction arises in such systems from purely dynamical physical principles" (pp. 138 f.). This statement is diametrically opposed to the commonly held view of the consequences of the law of entropy. That some such view must be true follows from the fact that we do find order and directedness in physical processes which are in direct conflict with the second law of thermodynamics. Köhler at this point is more than psychologist, and *Gestalttheorie* becomes more than a new brand of psychology. Here is an attempt to derive order and arrangement from dynamical considerations which leave behind the simple

mechanisms and atomism of 19th century physics without an appeal to *vis viva*, *élan vital*, entelechy or any other vitalistic principles. We have here a philosophy of nature reaching down into the very foundations of modern physics.

There is one change in *Gestalttheorie*, prominent in this book, which will help us to understand Köhler's position in general. Whereas in earlier writings the emphasis has been on how things get together to form *Gestalten* (*e.g.* Wertheimer's short circuit theory), Köhler continually stresses the point that *segregated wholes or units* form the basis of this latest exposition of configurationism. This is an important change, and to the reviewer it is the outstanding contribution of the book. The change relieves *Gestalttheorie* from embarrassing questions and makes the concept of *Gestalt* less vague and mysterious. It will be no longer necessary to write at length, as Köhler was forced to do in *Physische Gestalten*, on the problem of independent or isolated systems. The concept of *Gestalt* need no longer be an ambiguous thing referring to almost any connected series of events, logical, perceptual, behavioral. *Gestalten* are those segregated wholes met with in nature, the objects fashioned by man, and those processes both in nature and the organism which can be seen and understood only as unities, possessing specific properties as such. From this point of view forces work toward separation, delimitation, and concrete unities as well as in the interests of fusion and amalgamation. Form, which previously did and did not seem to be the *Gestalt*, now emerges as "in some respects the most important property which a whole may have" (p. 219.) As a clarification of this point alone (a consummation devoutly wished by friends of the school) the book has made a great step forward.

We come to Köhler's treatment of certain crucial topics: meaning, association, reproduction, behavior, and insight. The treatment of *meaning*, which is scattered throughout the book, engages Köhler almost from the very beginning, for we are told in the third chapter that meaning has been the bugbear of the introspective school. Köhler's theory of meaning, if he may be said to have one, seems to be that meaning is immanent, a property of organized wholes, following lines drawn by natural organization. Since segregation precedes meaning, the latter cannot be used to explain the former. Nor does the concept of 'past experience' help to explain why a given object has this meaning here and now, since present organization seems to be the determining factor. For the meanings given in cross-section, the kind dealt with mainly by Köhler, the theory seems adequate enough, but the reviewer doubts if the temporally extended events involving the growth and decay of meanings can be treated in this way. The application of configurational concepts to the temporal cases has been promised for a number of years but still remains to be worked out with the certainty and force that characterize the static cases.

Association and *reproduction* are presented as amenable to the same principles Köhler has enunciated for perceptual patterns. The form of the segregated whole determines what will be associated or reproduced from past experience. It is this which distinguishes *Gestalttheorie* from such a theory as redintegration which does not provide for the differentia of association and reproduction. "If association as a basis for reproduction is just another expression of the fact that the traces of functional wholes are themselves

detached wholes, one might somewhat rashly deduce from this theory that, after a trace is formed, any group of stimuli which represents a considerable fraction of the original constellation of stimuli will reproduce those parts of the original process the stimuli for which are not actually given" (p. 311). Association and reproduction emerge as subordinate to the principles of organization.

The chapter on *behavior* will come as a surprise to many who will find the method of approach to behavior much different from that common in this country. The problem which Köhler attacks is that which concerns the manner in which we come to ascribe to others experiences similar to our own. Indeed, others may judge my feelings by my behavior better than I myself can. The manner of speech may be more indicative of attitude or of meaning than what is actually said. Even inanimate objects and events may, on the face of them, bear aspects which can only be described in such anthropomorphic terms as 'threatening,' 'fearsome' or 'alarming.' There is no dualism between what we see on the behavior side and what is directly experienced from within. There are glimmerings here of a new theory of empathy and the whole discussion points toward a *verstehende Psychologie* which as yet is almost purely descriptive.

We have now arrived at the last and most difficult topic in the book—*insight*. The importance of this concept is indicated in the statement that "the chief dividing line among contemporary psychologists would separate those who acknowledge obvious, direct determination as it appears in common experience from those who can only admit indirect connections" (p. 383). By *insight* is meant the fact that we may know that certain things are related as we experience them, without having to learn or go through a process of conditioning to arrive at the thought. We see that our attitudes arise from certain situations and that, in turn, situations may be colored by our already formed attitudes. Köhler admits that there are many cases of behavior in which insight is lacking. And when insight is admittedly a product of learning, a problem still remains as to what degree of dynamical relationship exists between the parts of a situation. The theory is finally stated in terms of central brain processes which may help to make it more concrete for some psychologists.

To evaluate a book of this kind, written partly for the layman, is extremely difficult. That it is not Köhler's *magnum opus* the reviewer feels very sure, preferring to reserve the palm for *Physische Gestalten*.

In one or two places, the reviewer would ask for more than has been given. Thus he feels that, in spite of the objections which may be urged against behaviorism, it has been a most productive movement in this country, furthering the study of animal and child behavior, the establishment of institutes and clinics, and providing an incentive for a more functional view of the organism. Of these facts Köhler does not seem to be aware. Again, where we might have expected some word on such a moot problem as the configurationist's position with regard to analysis, nothing is said. In answering critics, Köhler's failure to mention the names of the persons he is discussing leaves a question in the reader's mind as to what the critic really said or meant. The lack of specific references to names and the literature lends an air of thinness to many

of the arguments and counter-arguments which might easily have been avoided. However, if one trusts his author and enjoys an outing on a "Sunday of research" this book will come as a pleasant, perhaps shocking experience which, in any case, is worth having.

Bryn Mawr College HARRY HELSON

Lehre von den Gesichtsempfindungen auf Grund hinterlassener Aufzeichnungen. By FRANZ HILLEBRAND. Edited by Franziska Hillebrand. Vienna, Julius Springer, 1929. Pp. v, 205.

Hillebrand's work was devoted largely to investigations of visual phenomena. His chief contributions cover the period of 1889-1922. It had for some time been his plan to bring together and organize the results of his researches in a single critical volume, but this work was cut short by his death. The present volume is edited with annotations by his wife, Dr. Franziska Hillebrand. Since its contents have largely appeared in earlier publications it is unnecessary to review it critically here. The volume is a detailed and critical exposition of the general field of visual sensations and perception rather than a new contribution to a particular problem. The first part analyzes color phenomena and investigates thoroughly their relationships to variations and complexities of external stimuli and to retinal adaptations and contrast effects. The extreme complexities of these relationships are well brought out and illustrated by numerous figures, and it is shown that hue, saturation, and brightness do not have a one to one relation to the physical variables in the stimulus. Hillebrand recognizes the defects of both the cylinder and the prism as means of representing the interrelationships of the factors in color phenomena. He maintains that the W-Bk series is really not a series of brightnesses but one that *has* brightness, since black is not the mere absence of something. The energy curve of the light-intensity series is an entirely different thing from the brightness curve as determined photometrically; and hue, brightness, and saturation are not variables which are independent of one another. In this discussion the activity of the retina receives much emphasis. There is a technical discussion of the laws and principles of color mixing, in which the nonpredictability of the results of different mixtures is emphasized as well as the necessity of depending always on empirical procedures. A critical evaluation of color theories completes Part I. One is disappointed to find that the Ladd-Franklin theory is not even mentioned either by the author or the editor.

Part II is devoted to visual space. It is here that the author has done his most original work. Hillebrand is a nativist, as is well known; so for him this topic is as much a matter of sensation as is that treated in Part I. The geometrical aspects of localization are stressed at the expense of the genetic aspects, and localization *in situ* is thoroughly considered. Comparisons between 'sensed' and 'real' space are frequent. For single vision "the stimulation of any retinal point produces in consciousness a single, completely determined direction of vision" (p. 104). In the case of binocular vision, therefore, "The sole source of the perception of depth is single vision with disparate retinal points" (p. 125). Without such single vision we could see the double images resulting from stimulation of non-corresponding points only as crossed or uncrossed,

not as far or near. Muscular strains of accommodation and of convergence are of secondary and minor importance. On this point Hillebrand came into conflict with both Wundt and Helmholtz. Hillebrand, because of this position and his acceptance of the nativistic view of visual space perception. devotes considerable space to a detailed consideration of the horopter. In his critique of empiricism he dismisses the effects of the 'feelings' of muscular movement and of impulses as factors in visual depth, with the assertion that a view based on such factors only assumes what was to be explained; if no spatial factor is natively present, none can be built up by the experience of these muscular impulses. Therefore, he contends, any assumption of spatial perception from muscular activity is only another form of nativism. In similar fashion he meets objections to his view brought in from the developmental factors in children and from the experiences of persons blind from birth with cataract and made to see by operation. Hillebrand saw only the alternatives, mentioned above, of conscious retinal factors or conscious muscular factors; he did not recognize the localization effects of muscular contractions of which one is not directly aware, in one's reactions to space relations or situations. (See the reviewer's article in *Psychol. Rev.*, 1926, 33, 218-236.) We cannot here enter into the controversies of nativism versus empiricism, but it must be noted that the so-called secondary factors are rather efficient in the case of vision with one eye only; moreover, directly in opposition to Hillebrand's main source of depth perception as quoted above—single vision by corresponding points—one may recall that wall-eyed and cross-eyed individuals must acquire new corresponding points.

This monograph, summarizing the experiments on visual phenomena from the standpoint of Hillebrand's work, is well worth owning by experimental psychologists.

J. P.

The Psychology of the Adolescent. By LETA S. HOLLINGWORTH. New York, D. Appleton and Co., 1928. Pp. xii, 227.

The Psychology of Adolescence. By FOWLER D. BROOKS. Boston, Houghton Mifflin Co., 1929. Pp. xxiii, 652.

The titles of these two books connote in some degree the general difference in the expositions. Both books are concerned with much besides the psychology of youth, unless that term be taken in one of its more popular uses. Both authors use the results of educational research and of psycho-clinical, biological, and sociological studies of the pre-adolescent and adolescent years. Dr. Hollingworth's study of the problems of the average adolescent has a strong clinical and anthropological tone throughout. Dr. Brooks, in presenting the facts of hundreds of individual investigations into the physical, mental and social factors in the period of adolescence has necessarily presented much statistical material as well as many psycho-clinical details of mental hygiene. Hollingworth addresses her book to parents and teachers, particularly to those who have the care of children in the pre-adolescent years, as she wisely believes that the maximal gain results when adolescence is anticipated in the rearing of the infant and the child. Brooks sets himself the task of "describing adolescent nature, growth, and development so as to facilitate both reliable prediction

and suitable guidance and control of behavior during the teens." The first author states that much of the "lore about adolescence" rests "upon the mere opinions of professional observers, rather than upon exact quantitative researches." The second presents in much detail the impartial evidence of such researches, adding his own critical analyses and conclusions. Both books contain an index and selected references. Brooks has placed a list of references at the end of each chapter as well as a list of problems for discussion. In addition he has appended a glossary of technical terms. Both books are valuable and important additions to the current literature of youth. Hollingworth's volume is for the intelligent and inquiring layman, while *The Psychology of Adolescence* may be considered more in the nature of a text and reference book for those who desire to study in greater detail the whole problem of this period. The general point of view of both authors is that the continuity of life is not broken either at the onset of puberty or upon the attainment of adulthood.

To illustrate the difference in exposition and the point of view in the two books let us take a specific statement from each author. Hollingworth holds that "the attainment of puberty is positively correlated with intelligence," that the gifted child will attain puberty earlier than the average and much earlier than the feebleminded. In his Chapter VI, Brooks examines critically the correlation and significance of physical and mental growth at and during adolescence, discussing various kinds of maturity. After citing impartially a great many investigations, he concludes that the correlation between physical maturity and mental status is so low that "predicting mental or scholastic ability from physical traits is only a little more accurate than guesswork." As extreme instances he mentions two cases of puberty praecox of Gesell's, one a girl who matured at the chronological age of three and one-half years and developed physically with great rapidity but mentally as an average normal child, the other a girl who matured at eight years but remained an imbecile although her height and weight were normal or above. Here is expressed not merely a difference of opinion but a fundamental difference in method as between Brooks's exhaustive examination of the facts available and Hollingworth's acceptance of general, clinical opinion based upon observation and, in this case, on Terman's *Genetic Studies of Genius*. There is no implication that one book is inaccurate or superficial while the other is exact and careful; but in this instance the weight of evidence seems in favor of Brooks. Whenever possible throughout his book, Brooks so presents his material that the reader may judge for himself, while Hollingworth, addressing the parent, is descriptive, explanatory and generalized.

These books generally complement each other. Each deserves attentive reading. Brooks's genetic and pedagogical psychology, based on original nature, learning and interests, is chiefly that of Teacher's College at Columbia. His volume includes a chapter on the "Disturbances of Adolescent Personality" and one on "Prediction of Adolescent Behavior" where the correlational methods are succinctly and admirably explained. Hollingworth is concerned with the average normal adolescent and does not discuss either psychoses or neuroses. Her chapter on "Psychological Weaning" is a masterly exposition of how parents can help or hinder a child to pass from dependency to a free and adequate maturity, and her discussions of the mental hygiene of the in-

dustrial life of the young and of the social and psychological factors involved in mating are valuable contributions to the study of adolescence. Her volume was chosen by the American Library Association for the League of Nation's Committee on Intellectual Coöperation.

Cornell University MARGARET RUSSELL BENTLEY

The Mind of the Savage. By RAOUL ALLIER. New York, Harcourt, Brace & Co., no date. Pp. xiv, 301.

This volume is a translation of *Le non-civilisé et nous* (1927), a sequel to the more ponderous *Psychologie de la conversion chez les peuples non-civilisés* which appeared in 1925. In spite of the obvious bias of its author, the book is eminently readable. To the student of the 'collective mind,' the 'social' psychologist, it offers a wealth of illustrative material, drawn from a variety of sources. The initial chapter-heads (Magic and the Arrest of the Intelligence, and Magic and Moral Disintegration) afford a clue to the general drift of the argument. The task set himself by the author (who is dean of the Protestant Faculty of Theology at Paris, with a major interest in the colonial missions and native tribes of Madagascar) is the charting of the gulf which separates the civilized from the surviving primitive. Is the difference in mentality posited by Lévy-Bruhl irreducible, he queries, the mark of a divergent evolutionary trend? Or are the uncivilized simply retrogressive communities of the main stock?

Following Gräbner's lead, Allier summarily dismisses evolutionary argument, deciding in favor of cultural diffusion. Arrested development or deterioration is the sequel, not of evolutionary factors, but of the wilful adoption of magical belief and practice. Reliance on an omnipresent *mana*, controllable through charms (*vuriké*) and the offices of the witch-doctor, paralyzes observation and reason, and demoralizes character. Life among the Bantus and other African tribes is luridly depicted as a long nightmare of terror and illusion, a kind of chronic paranoia, with suspicion directed continually against one's associates and family. The activities of secret societies of sorcerers (*Wihibis*), whose rites demand human sacrifice, foment the psychosis.

The remedy is moral and emotional, rather than intellectual, regeneration (in short, conversion), if the findings of Durkheim and Lévy-Bruhl as to the mystic, pre-logical character of mental processes in primitives are to be trusted. The feasibility of reclaiming these backward members of the race is guaranteed by certain elements of identity in human nature, discoverable everywhere under apparent difference. Two of the most illuminating sections of the book are devoted to establishing this thesis (Chs. IV and V). Evidence bearing on the recrudescence of magic and the use of charms and amulets during the recent war, especially among the soldiers draughted from civilized nations, is given in detail. These and other modern instances of semi-magical beliefs and reactions among the civilized are classified by Allier not as survivals, working up from lower strata of culture, but rather as *reinventions* of magic. Binet, Piaget, Lévy-Bruhl and others are cited in support of a universal anthropocentric trend. The bent to construe emotionally rather than cognitively—in short, to *participate*, in all perceptual processes—is present, especially in childhood, no matter what the cultural level.

The sources utilized in the account of *vu-ikε* and voodoo are in the main identical with those employed by Lévy-Bruhl—the memoirs, in large part, of African missionaries and governors, Moffat, Junod, Julien. Allier's marshalling of evidence in support of his own hypotheses, however, rarely convinces. *E.g.*, the data, *as presented*, would seem, to the reviewer's mind, to point to 'germinal arrest' rather than magical practices as the causal factor in savage backwardness. Evangelical fervor, one suspects, may color one's perceptions no less insidiously than do magical prepossessions; and the phenomenon of participation may be commoner even than our author would be willing to admit. Cf. especially the construction put upon native suicides (80 ff.), upon Arabian voluptuousness (247 ff.), and upon the effect of the hunt upon the character of the hunter (245 ff.).

The book is provided with a usable index (though of proper names only); and there are appendices on the belief in men-tigers, on the system of vendetta among savages, and on the so-called 'instinct of orientation.' The translation of the text is hardly of uniform excellence, sometimes, in fact, absolutely misleading. Yet with all its defects the work is an interesting document, an attempt to link up modern theories and the findings of the social sciences with the practical problems of colonization.

Cornell University ELSIE MURRAY

Le développement mental et l'intelligence. BY HENRI PIÉRON. Paris, Félix Alcan, 1929. Pp. xii, 97.

This small volume consists of four lectures which the author gave in the University of Barcelona. The intention in these lectures was merely to trace the main lines of progress and to indicate the chief problems that had arisen in this field of psychology. The first lecture takes up the stages of mental development. The author emphasizes the general organic unity so significantly brought out in modern biology, and he recognizes the purely practical motives in separating physical and mental growth, as well as the abstract and isolated picture set up by science as compared with the living individual. The great variability in the mere appearance of this or that definite activity or aspect of physical development, he maintains, must be studied in a close and correlated manner. He points out that even at birth a certain level of development (at least the neural) might be worked out and any given new-born child classed as retarded or advanced, but nervous development can be correctly based only on a variety of behavior manifestations instead of on single acts. He is certainly on somewhat questionable ground when he maintains that early walking acts depend solely on certain neural maturations. Piéron admits that physical and mental retardations are not closely related, being somewhat differentially affected by hormones, etc. He is somewhat too positive as to the underlying organic causes of certain variations from normal development; we can not go ahead of our verified hypotheses. Mental development is characterized as the general capacity of coördination, of the unification and stabilization of conduct. It is adaptation not only to present but also to future and remote circumstances through mechanisms not made clear. Future circumstances are, of course, aspects of present conditions. The use of symbols, particularly in language, is

important in higher mental development and is stimulated by 'social instincts,' including impulses to imitate.

The second lecture traces the development of intelligence scales from Binet down through the American and European countries, and tests for persons laboring under certain handicaps like deafness are also included. The "fundamental postulate" in the application of any scale, he maintains, is that all subjects have been subjected to comparable educational and social influences. The next lecture takes up levels and profiles of mental development, presenting different theories of mental growth in individuals and races. One certainly needs more evidence than Piéron gives (or than exists yet) for holding that abilities in some races, e.g. negroid, manifest natively higher levels at first and later show earlier attainment of limits than do other races. This is a pretty theory but is not supported in test data thus far obtained. Profile schemes like those of Rossolimo and Lasoursky of Russia, Vermeylen of Belgium, are briefly presented and evaluated The work of Downey and of Seashore is not cited. It is recognized that the reliability of such various traits is generally not well established. The last lecture takes up the problems of analysis of intellectual capacity and reviews the theories of Spearman and others, now so well known. To locate centers of automatic activity, instinct, "the purely affective life" in mammals in the striate bodies and thalamus he cites the work of Rothmann, Zeilony and Dusser but makes no reference to Cannon's and Stone's work on these subjects nor to the study of more general mental functions by Lashley, though his view does not differ significantly from that to which Lashley's work has led regarding integrated mechanisms. Piéron points out that a rather large percentage of the 'genius' manifested in certain of our American tests for children (based upon Terman's classification) does not seem to be present in adult society. He wonders whether the sort of profundity seen in the work of Newton, Einstein, etc., directed to the solution of problems irrespective of the time required, should not be given more consideration in testing. The mere rudimentary nature of various analytic techniques in present-day testing is indicated.

The booklet is a more detailed, critical, and comprehensive piece of work than most similar American surveys or summaries, and is well worth reading.

J. P.

The Influence of Habit on the Faculty of Thinking. By MAINE DE BIRAN. Tr. by Margaret D. Boehm. Introduction by George Boas. Baltimore, The Williams and Wilkins Co., 1929. Pp. 227.

This is the third of the series of *Psychological Classics* edited by Knight Dunlap. It is valuable to English readers in making more easily available to them the classical work of a writer diverging somewhat from the views of the earlier French school called "ideologists" and laying the basis in important respects of the French spiritualism' which culminated in the works of Henri Bergson. The translated volume contains, besides the illuminating historical Introduction by Boas, a report of a committee appointed by the Academy of Moral and Political Sciences of the Institute of France, to examine and evaluate seven memoirs entered in competition upon the question "To determine what is

the influence of habit upon the faculty of thinking; or, in other words, to show the effect which the frequent repetition of the same performances produces upon each of our intellectual faculties." The first contest brought no results, as none of the contributions was regarded as fulfilling the designs of the Institute. The second competition resulted in the award of the prize to the author (Maine de Biran) of the present work; and M. Destutt-Tracy of the Committee, who was selected to write the report, makes an exposition and analysis of the contribution, covering 17 pages. It should be said that Maine de Biran's paper had been offered before and had received honorable mention as well as criticism and advice from which the author had profited in the re-writing.

The subject is clearly a psychological one but was attacked in that day (1803), as such problems were then, by a non-experimental method. Psychologists interested in the historical background of their subject will find themselves much indebted to the persons who have made available this excellent translation of an important monograph which marks the point of divergence of French 'will' psychology from the earlier extreme developments of associationism. This indebtedness is real whatever they may think of some of the modern fruits of this divergence and of the influence of Maine de Biran on the development of experimental psychology in France.

J. P.

Problems of Instinct and Intelligence. By R. W. G. Hingston. New York, The Macmillan Company, 1929. Pp. xiii, 296.

Major Hingston is a 'naturalist' who writes in fluent style a book which is best reviewed by reference to the extremely cautious preface written for it by Bertrand Russell. After some general and complimentary remarks, Russell is impelled to point out that Hingston's treatment of insect memory "goes beyond what his data warrant" and that his view of mental evolution "assumes the inheritance of acquired characters in a very extreme form" (p. xii). The very presence of such criticisms in the introduction may serve as a warning that the reader must proceed at his own risk in accepting without question Hingston's not infrequently startling conclusions.

The author obtains most of his factual material from his own observations in the field, supplementing this, however, with a well-documented supply of quotations, chiefly from the Peckhams, Forel and Fabre. His early chapters advance evidence of this sort regarding the perfection, wisdom, rhythm and variability of instinct, as well as its limitations, flexibility and folly. He is convinced that insect behavior is not limited to instinctive patterns, but that it includes, as well, behavior typical of memory, of intelligence, and of an 'unknown sense' which serves to explain the activity of migration. Observing largely among the Hymenoptera, he finds many cases in which insects depart from conventional patterns of behavior in meeting novel situations. Thus he describes how ants build bridges and platforms to help one another, how wasps vary their methods of attack under special circumstances, and how other insects meet new situations 'intelligently.'

Hingston holds that "We cannot explain psychic phenomena by reducing things to physical and chemical laws. Even insect psychology has something else. It has memory and conscious mind" (p. 247). Instinct, he finds, "began

in a reasoned act . . . this act, through being continually repeated, tended to lose the reasoning element and to become more and more unconscious. As this process continued through generations, the mental machinery by which it worked got more indelibly engraven in the mind. And in the end it became automatic—in other words, it became instinctive' (p. 268).

There is a wealth of descriptive material in the book, but it is to be regretted that the author chose to illustrate entirely by sketches, without the use of photographs. There is a singular lack of negative evidence, as well as an irritating refusal to define terms, both of which detract from the value of the treatment. Thus Hingston refuses to define his terms 'instinct' and 'intelligence,' because he regards them as "incapable of accurate definition," and "everyone knows what is meant" by these terms. His writing is anthropomorphic throughout the volume; thus he finds that an ant returns to a former feeding place because "the thought of food has recalled to its memory its experiences of twelve hours before" (p. 242), while a wasp acts preoccupied because "her attention is absorbed in energetic effort; hence she cannot fix it on other things" (p. 237).

Iowa State College JOHN G. JENKINS

Educational Psychology. By RUDOLPH PINTNER. New York, Henry Holt & Co., 1929. Pp. xiv, 378.

The content of textbooks in educational psychology is ever changing, and very properly so. In this book the author has departed from the custom of devoting much space to neuro-anatomy, and has proceeded directly to the material that the practical educator needs and in which the up-to-date teacher of educational psychology is interested. The treatment of original nature, character education, and non-intellectual traits is of such a nature that it might well be called 'educational psychiatry." This is as it should be, for educational psychology should certainly apply in the matter of developing personality and character as well as in the matter of teaching facts.

There may be some objection to the use of the term "trait" as applied to character. It is true that people vary a great deal from time to time in honesty, industriousness, frankness, and other types of conduct, while "trait" indicates greater fixedness. There seems to be no other term, however, that can be substituted for this; no term that covers the meaning more satisfactorily. On page 71 the author says "When we study the conduct of an individual we find a set of habits of action, more or less loosely organized. These modes of reaction are based upon his original tendencies, but they have been conditioned by his environment." Growing out of these statements would seem to be the idea that a "trait" is a type of conduct, either inherited or acquired or both. For general practical purposes this concept should be satisfactory.

Dr. Pintner's 'divisions of intelligence' seems open to question. On page 128 he says: "The use of different instruments of intelligence measurement has brought to our attention the fact that there are different kinds of intelligence, such as abstract, concrete and social." Such a classification will hardly bear analysis, for it is in social life that abstract ideas function to the greatest extent. Also the concrete and the mechanical have their abstract and social aspects.

Furthermore, it may be noted that language falls into the classification of the abstract and that many individuals learn one language with much greater ease than they do another. Likewise, an individual may be very successful in dealing with one type of concrete situations or material but an absolute failure, in terms of modern efficiency, in dealing with another. It might be more nearly correct to speak of mechanical and social "insight" or "ability" if we agree that "insight" and "ability" are due to native capacity plus experience or training. In like manner we might speak of "political insight," "legal insight," or "financial insight," but we can hardly speak of political, legal, or financial intelligence. A politician, lawyer, or financier might be very successful in one situation, but have very little success in another in which he has had little or no experience.

The author's discussion of the practical application of intelligence testing in the matter of vocational guidance is unquestionably sound. On page 143 he says "In a broad sense all intelligence testing may be used for general educational and vocational guidance. In the narrower sense of giving specific educational advice as to whether to go to high school or college, or as to the course of study to be chosen, intelligence tests may help but they cannot be used in a mechanical or arbitrary way. The wise counselor will always make use of them but never depend upon them alone." From page 144, "Intelligence, however, prescribes the limits within which the individual must function."

The chapter on the "Laws of Learning" is too brief. The treatment is of a rather hurried type, such as an author sometimes gives to a matter that he thinks is already well known to every one. More time should have been given to discussion of the psychological principles back of these laws, to illustrations of their practical application, and to explanation of the co-functioning of these laws in the case of the most effective learning. It would seem, also, that some mention should have been made of the synaptic and the *Gestalt* theories of how learning takes place. The other chapters on learning, however, are much fuller, including the matter of efficient learning, practice curves, improvement curves, motivation, and the curve of forgetting.

The discussions of "New-Type Examinations" and the use of measurements in educational work are reasonably adequate and very much to the point. It seems obvious that if education is ever to become a science we must recognize the fact that the measurement of the results of our teaching is just as important as the use of the best psychological methods in daily classroom instruction.

Teachers College, Syracuse University ROBERT P. CARROLL

AN ACKNOWLEDGMENT

The portrait of Hermann Ebbinghaus, which appears in this number of the Journal, was reprinted from the frontispiece of the *Zeitschrift für Psychologie*, vol. 51, 1909. The signature which appears below it is a facsimile of one in the collection of autographs owned by the Department of Psychology of Cornell University. K. M. D.

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By J. P. Guilford, University of Nebraska

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